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AND

HUGH THURSFIELD, M.D.

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President—Dr. J. W. Carr, C.B.E.

[*October* 12, 1928.]

CASES.

Acromegaly with Amyotrophic Lateral Sclerosis.—C. Worster-Drought, M.D.—M. F., male, aged 54, complains of headache, weakness and enlargement of the hands, and defective vision.

History.—About six years ago the patient began to complain of unsteadiness of gait, occasional headaches, and visual impairment. For the past five years he has noticed enlargement and weakness of both hands, gradually becoming worse, together with some increase in size of head. The headache, fronto-vertical and mainly left-sided in distribution, has increased in intensity during the past twelve months, and for six months he has been very drowsy and able to sleep at any time of the day as well as all night. More recently he has noticed that his voice is less clear and that movements of the jaw tire easily; occasionally he has difficulty in swallowing.

Previous History.—Has worked as a hospital porter. Mastoid operation nine years ago on left side. No family history of importance.

Present Condition.—Pupils: right slightly larger than left but reaction is normal. Left optic disc shows pallor of temporal half, otherwise normal. Visual fields: outer and lower parts of both contracted, especially in left eye. Early lenticular opacities.

Upper limbs show general muscular weakness, with some wasting of shoulder-girdle and arm-muscles, also of the interossei, thenar and hypothenar muscles, especially of left hand. Abdominal reflexes absent, knee- and ankle-jerks moderate and equal; occasional ankle-clonus. Plantar reflexes, indefinite extensor. No sensory loss.

Hands broad and somewhat enlarged. Frontal region of skull prominent and overhanging, but face and lips not unduly prominent. Ears larger than normal and feet broad.

Heart normal; pulse 64; blood-pressure: 100 systolic, 70 diastolic; temperature usually sub-normal (97° F.). A blood-count shows no abnormality. Wassermann reaction negative; cerebro-spinal fluid shows no abnormality. Resting blood-sugar 0·10. Sugar-tolerance curve following 50 grm. of glucose, rises normally, but shows a very slow fall. No glycosuria.

X-ray Examination.—General enlargement and thickening of skull-bones with pituitary fossa not definitely enlarged but irregular in outline. Calcified pineal body. General enlargement of small bones of hands. Considerable bony proliferation in the joint between eleventh and twelfth dorsal vertebræ.

Mentally, patient is lethargic and irritable, and exhibits very poor memory.

One point for discussion is as to whether he is suffering from acromegaly or from early and atypical Paget's disease; there is more evidence in favour of acromegaly. There has been definite enlargement of the hands, and of the prominent parts of the face; the bony enlargement of the face shows better in the radiogram than in the patient. Further, there is bilateral restriction of the temporal halves of the visual fields. Admittedly the pituitary fossa is not definitely enlarged, although it is somewhat irregular in outline. The other feature of interest is the association, probably accidental, in the same patient, of amyotrophic lateral sclerosis.

Dr. F. Parkes Weber said he considered this an undoubted case of acromegaly, as, besides the bony enlargement, there was likewise obvious enlargement of the soft parts, notably the ears and face. The patient was quite sure that his ears had increased in size.

Anæmia of Obscure Origin.—Terence East, D.M.—A girl, aged 19.

History Previous to Admission.—Pallor, weakness, dyspnœa, steadily increasing since March, 1927.

Past History.—Poor health for two years; has been under observation since July, 1928; slowly lost weight, and there has been irregular fever.

Anæmia has been severe. Red cells have only once been above 2,000,000 per c.mm.: they are now slowly falling; the present level is 1,150,000. Hæmoglobin has been under 40 per cent., except twice; now it is 35 per cent. Colour-index has been continually over 1, and now stands at 1·3. The blood bilirubin has been consistently low—under 0·2 unit by van den Bergh's test.

There has been persistent leucopenia; present level 1,600, of which 30 per cent. are polymorphonuclears. Eosinophils are scanty. Spleen is not enlarged. No finger clubbing. Gastric residuum has normal acidity (0·5 per cent. hydrochloric acid). Heart a little enlarged, systolic murmur, presumably hæmic in origin, heard all over it. No blood in fæces; urine normal; urine and blood cultures are negative.

A hæmolytic streptococcus has been isolated from the root of a tooth, and patient has received a vaccine from it. Blood transfusion (275 c.c.) caused no improvement. Liver and arsenic in full doses have had no effect. Artificial sunlight has been tried.

As a possible diagnosis a streptococcal infection of the mitral valve might be suggested, but after three months such has not been found. It appears to be an anæmia of aplastic type, but of very long duration; she has been remarkably free from spontaneous hæmorrhages. The cause of the condition is quite obscure.

Discussion.—Dr. F. PARKES WEBER said he believed this case to belong to a relatively little known group of chronic or subacute anæmia of aplastic type in children and young adults. In two cases cure or great improvement had occurred; in Dr. Porter Parkinson's case following the shock of a blood-transfusion, in Dr. A. G. Gibson's case under adrenalin treatment. Dr. Parkinson had unfortunately lost sight of his patient, so that the subsequent history was unknown.

Dr. LESLIE WITTS asked as to the frequency of thrombopænia and purpura in these cases, especially in those which Dr. Parkes Weber mentioned.

Dr. SALISBURY SHARPE said he thought two points would throw further light on this case. One was the differential count of the polymorphonuclears, another the examination of the blood by dark-ground illumination. With regard to treatment, in some of his own cases of severe anæmia, bone-marrow had been beneficial.

Dr. PARKES WEBER (in reply to Dr. Witts) said that in the cases of anæmia of chronic aplastic type the thrombocytes (blood-platelets) seemed not to have been counted, as far as he (Dr. Weber) knew, but in acute aplastic anæmia they were no doubt greatly diminished in number.

Dr. AWROUNIN said that this patient had stated, in reply to questions, that she had had profuse epistaxis, and that the bleeding at some of her periods had been very profuse.

Dr. EAST stated (in reply) that bone-marrow had been tried in this case, but without avail. With regard to any hæmorrhagic tendency, the patient had had teeth withdrawn, but bleeding had not been unusual. Epistaxis had occurred once after a fall which hurt her nose. The platelets had not been investigated. He hoped to obtain good results from adrenalin.

Traumatic Cyst of Face.—J. H. EASTON, M.B. (introduced by CECIL P. G. WAKELEY, F.R.C.S.)—The patient, a girl aged 5, was struck on the right cheek six weeks ago by the edge of a swing. A bulging in this situation was noticed subsequently.

On Examination.—There is a prominence of the lower part of the cheek over the right maxilla opposite the antrum. This is caused by a painless circumscribed swelling, an inch in diameter, which fluctuates and is translucent. The overlying skin is normal. The swelling can be moved freely over the deeper structures.

Associated with it is a fracture of the underlying bone.

Theoretically, a swelling of this nature should be a hæmatoma, and ought to absorb slowly, but it did not feel like one. Two other suggested diagnoses had been made. One, that it is a mucocele following fracture of the outer wall of the antrum of Highmore, a little portion of endothelium coming through and forming a cyst.

The fluid in such a case would be thick, and so would not come away in the aspirating needle.

The other suggestion was that it is a lesion of the duct of the parotid gland, but I think the swelling is too far forward for that diagnosis.

Mr. C. E. SHATTOCK regarded the swelling as an inflammatory mass. He had had two similar cases, and after a policy of "wait and see" the swelling had disappeared.

Diaphysial Aclasis.—J. H. EASTON, M.B.—Patient, a girl aged 12, has noticed during the last few years the appearance and gradual growth of several hard swellings in different parts of the body.

The swellings, which are roughly symmetrical and bony in consistency, are outgrowths of the bone near the growing points.

They are situated at the lower ends of both femora, the distal ends of both radii, the vertebral border of the scapula, and near the costochondral junction of a lower rib.

There is a strong family history of a similar condition in the father, and paternal uncle, aunt and cousins.

It is rather a disorder of bone formation than a new growth of bone. Another interesting point in the case, though I do not know that it has any connexion with it, is that the girl came for operation for tonsils and adenoids, and after that operation she had several very severe hæmorrhages. I do not think these had anything to do with this condition. There is probably some trouble with the calcium content of the blood.

Single Exostosis.—CECIL P. G. WAKELEY, F.R.C.S.—I brought this case up in contradistinction to Mr. Easton's case of multiple growths. The patient is a girl aged 9, who first noticed in March, 1928, a lump in the upper end of the left humerus. This has gradually become larger. X-rays show an exostosis on the inner part of the humerus.

In Sir Arthur Keith's view diaphysial aclasia is a term in contradistinction to diaphysial clasia, the normal closure. I think that neither Virchow's nor Keith's theory gives us the actual pathology. It is true that multiple exostoses occur at the growing ends of bones, as Mr. Easton's case shows very well. I think it is hardly possible for an exostosis in the middle of the rib to have migrated from the epiphysial line of the rib.

Discussion.—Dr. F. PARKES WEBER said that "aclasis" seemed to mean "absence of breaking," but that in Sir Arthur Keith's use of the term it meant "absence of pruning." In some cases of "multiple exostoses" (which, admittedly, might be a familial condition—obviously an abnormality in osseous development) an important feature of the condition was, as Sir A. Keith himself pointed out, aplasia of the distal end of the ulna. One could not prune what did not exist and what had never been formed.

Dr. KINGSTON BARTON said he had heard Sir Arthur Keith's original lecture in 1919, when he first used the term "multiple exostoses" just as a clinical term to embrace all cases which differed so much in position, size, etc. Probably the lecturer's view was correct, i.e., that there was some disorder in the development of bone, his theory being that bones were laid down on a certain model, that the whole outline of a bone was under the periosteum, and the membranous portion of that outline was where the development of the bone proceeded, and that in all the long bones the outside of the bone was developed from membrane, not from cartilage. The microscope showed, that in the early stage there were apertures in the periosteal bone through which it was easy for cartilage to escape. If those outgrowths remained open long enough it was the exodus of cartilage on to the periosteum which proceeded to lay out the tumour called an exostosis. One reason why he thought Sir Arthur Keith was right in his theory was that it was at the extremities that the defects took place, leading to some error in the moulding line of the periosteal fibrous tissue. Here there was no bone formed at the lower end of the ulna, and that fact threw light on the question as to whether the defect was in the blood-supply or in

the nerve-supply. Inspection of sections led one to think it was more likely to be an error in blood-supply, and that there would not be enough cartilage to make bone.

Mr. C. E. SHATTOCK said he remembered one case in which there were exostoses on the vertebræ as well. So it was not only a change affecting shafts of bones.

Myelogenous Leukæmia.—R. COVE-SMITH, M.B. (for Dr. TERENCE EAST).—Female, aged 28. Had been comparatively well until March, 1928, when she began to complain of pain in the stomach and of shortness of breath. She consulted her doctor who, in view of a history of "growing pains" and the presence of mitral stenosis, thought her condition was probably cardiac in origin.

During the summer, however, she became much worse and was admitted to King's College Hospital. At that time she had a cough, with pain in and swelling of the abdomen. Her joints ached and her feet were swollen at night.

Family history is good. Parents are alive and well. Three brothers and two sisters are alive and well. Mother and one sister have had "rheumatic pains." Patient had interstitial keratitis at the age of 5 years.

Present Condition.—The patient is short of breath; lips slightly cyanosed; frequent, unproductive cough.

Cardio-vascular System.—Apex beat $5\frac{1}{2}$ in. from mid-sternal line, in 4th space; right border extends $\frac{1}{2}$ in. beyond edge of sternum. Presystolic thrill in mitral area corresponding with presystolic murmur and loud slapping sounds audible on auscultation in this area. Blood-pressure $\frac{110}{85}$.

Electro-cardiogram shows myocardial defect with a P-R interval at the upper limit, and complexes slurred and notched.

Respiratory System.—There is an impaired note at the left base posteriorly with prolonged expiration and moist crepitations.

Abdomen.—The spleen is hard and firm; extends to left anterior superior spine and can be felt in the mid-line $3\frac{1}{2}$ finger-breadths below umbilicus. A notch is palpable just above the umbilicus.

The liver extends for 4 finger-breadths below the right costal margin.

Central Nervous System.—Left pupil larger than right; both react to light and accommodation. Reflexes sluggish. Plantars flexor.

Blood.—Red blood-cells, 2,376,000; hæmoglobin, 40 per cent.; colour index, 0·86; white blood-cells, 902,000. Wassermann reaction: weak, positive.

Differential Count.—4 normoblasts in 250 cells; polymorphonuclears, 34·0 per cent.; eosinophils, 2·0 per cent.; basophils, —; lymphocytes, 1·6 per cent.; large mononuclears 0·2 per cent.; neutro- and basophil myelocytes, 58·6 per cent.; eosinophil myelocytes, 3·6 per cent.

In this case I think we are dealing with a coincidence of two, if not three, diseases. In some cases we get evidence of a peripheral poison at work affecting the blood or the blood-forming organs. It does not, however, seem possible to account for the myelosis by the presence of a peripheral poison. Nowadays we look upon myelosis as a neoplasmic condition, so a triple diagnosis seems necessary. The patient shows definite signs of endocardial and myocardial lesions due to old rheumatic infection, there are signs of congenital syphilis and of the presence of disease in the blood-forming organs.

The patient has had arsenic by injection, benzol by the mouth, and application of X-rays to the spleen since she came into hospital and this treatment has increased the red-cell count, but the white-cell count has also increased and is now more than a million.

Discussion.—Dr. F. PARKES WEBER said this case might be grouped with the rare instances of myeloid leukæmia occurring in the late stages of syphilis or following malarial splenomegaly. The question seemed to be was there any real causal connexion with syphilis and malaria respectively? The same question was asked as to the leukæmia which in very rare cases had followed exposure to X-rays. He thought the association in such cases

was not merely a matter of coincidence. (Although the association was rare, one should remember that when cancer followed chronic irritation, a causal relation was admitted, though the sequence might be rare.)

Dr. BERNARD MYERS said that the practical issue was to discover the causation, and if possible the cure, of myeloid leukæmia. The actual exciting cause of myelosis or pan-myelosis was not yet evident. There was a decrease of erythrocytes and an increase of leucocytes in the typical way in these cases, but he showed a case last year which was a mixture of Osler's disease and myeloid leukæmia with distinct increase of red and white cells. One merely suspected infection but could not prove it. He was giving splenic extract in a case of myeloid leukæmia at present but it was too soon to judge the result.

Dr. J. W. CARR (President) said that Sir William Gowers used to teach dogmatically that leukæmia not infrequently followed upon malaria, but malaria did not come into the present case. Both congenital syphilis and leukæmia were fairly common diseases; might not their conjunction in this patient be merely a coincidence?

Dr. COVE SMITH (in reply) said that a number of these cases associated with syphilis or tuberculosis had been recorded. Swirtschewskaja said he thought all cases of myelosis were associated with tubercular infection. Curschmann recorded cases in people who had had reducing treatment, and suggested some hormonal disturbance. In each case there was an associated factor which was draining the system. Whether the infection wrought damage on the blood-forming organs directly or whether it set up chronic irritation and over-stimulation which caused them to undergo neoplastic change, was at the moment difficult to elucidate, although the latter seemed the more probable.

Bilateral Pseudocoxalgia.—CECIL P. G. WAKELEY, F.R.C.S.—Patient, L. S., aged 4, has always walked badly since the age of 2 years. Has been treated for rickets in various London hospitals. Came under my observation in September, 1928, because of his waddling gait.

On examination: The legs are of equal length and all movements of the hip joints are free except abduction, which is limited on both sides. Clinically there is no evidence of rickets at the present time, although skiagrams of the knees and wrists suggest that the condition has been present and is now cured.

X-ray examination of the hip-joints reveals a condition of bilateral pseudo-coxalgia. The upper femoral epiphyses are flattened and poorly developed.

The spine appears normal but shows distinct notches on the anterior surface of the bodies in lateral skiagrams.

The testes are imperfectly descended.

Meningioma of the Sensory Cortex: Removal.—I. M. ALLEN, M.D. (introduced by NEILL HOBHOUSE, M.D.).—E. E., male, aged 48.

Complaints.—(1) Difficulty in using right arm and leg; (2) attacks of "pins and needles" associated with movements on the right side. *Duration.*—Fifteen months.

Summary of History.—Sensory Jacksonian attacks on right side beginning in right foot. Motor Jacksonian attacks beginning in right foot and associated with the above for nine months. Residual hemiplegia for five hours after the motor attacks. Gradually increasing permanent hemiplegia right side. Pain deep in right arm near shoulder. Emotional attacks. Occasional slight hesitation in speech.

Previous History and Family History.—Nothing of importance.

Condition on Examination (Summary).—Cranial nerves: Weakness of right side of face for voluntary movements; otherwise normal. Sensation: Spontaneous as above; all sensations impaired on right side compared with left, with maximum impairment over right foot. Motor: On right side, decrease in motor power; increase in muscle tone in flexors of arm and extensors of leg; tendon jerks exaggerated; plantar reflex-dorsiflexion. Abdominal reflexes: Impaired on right side. Gait: Right leg moves stiffly and swings in semicircle from hip. Spontaneous movements: Clonic movements of right quadriceps. Coördination: Coarse tremor of right arm on movement. Cerebro-spinal fluid: normal; Wassermann negative. Blood: Wassermann negative. Skull: Tender area on left side of vertex about upper limit of fissure of Rolando. Skiagram: Gap in inner table about midpoint of vertex.

Operation (by Mr. Cecil P. G. Wakeley).—Large osteoplastic flap left side, and brain in front of and behind fissure of Rolando exposed. Dura adherent over post-Rolandic cortex close to middle line. Large tumour shelled out from this area. It occupied the leg area and was overlapped by and replaced the brain tissue in front, behind and laterally. Cavity filled with blood-clot. Dura, osteoplastic flap and scalp closed.

Post-Operative Findings.—Slight motor aphasia for seven days. Right flaccid hemiplegia. Motor Jacksonian fits confined to right side of face and eyelids. Sensation: (1) Tingling in right hand and foot; (2) feeling as if holding solid bodies in right hand: (3) feeling that objects present and felt in the left hand were also present in the right hand; (4) persistence of sensation in right hand after an object withdrawn from it.

At End of Six Weeks.—(1) Speech normal. (2) No further fits. (3) Spastic right hemiplegia. (4) Spontaneous sensations no longer felt. (5) Complete recovery of sensation except over right foot. (6) No evidence of repair in bone flap.

Discussion.—Mr. WAKELEY said the operation had been very easy; the tumour had readily shelled out.

Dr. F. PARKES WEBER asked whether meningiomata in this position were derived from Pacchionian bodies. A meningioma arising from a Pacchionian body might be termed a Pacchionioma.

Mr. WAKELEY (in reply) said that the inner table of the skull was eaten away by the pressure of the tumour; there was no soft bone. He had previously shown a case of vascular meningioma which, though it had a similar appearance, was different from this. Bleeding in that case had been stopped by diathermy cautery. The growth had recurred in twelve months' time and was then carcinomatous. It had not had the encapsulated surface which characterized the tumour in the present case. He regarded the prognosis in this case as good.

A Case of Goundou.—W. SALISBURY SHARPE, M.D. (introduced by F. PARKES WEBER, M.D.).—W. T., male, aged 43, has never been out of England. Wassermann reaction strongly positive. No history of any illness resembling yaws. A complete account of this case, together with skiagrams and photographs, will be published in the November issue of the *Transactions of the Royal Society of Tropical Medicine.*

Discussion.—Dr. HUGH STANNUS said the case clinically resembled one of goundou as seen in tropical countries, where it was generally considered to be due to yaws. Bottreau-Roussel, having operated on a single case successfully, was sought after by 130 patients in one year on the French Ivory Coast. All his cases were frambœsial and syphilis did not exist among these natives. The present case was a most important one, and it was as far as he (Dr. Stannus) knew, the first definite case of goundou due to syphilis in a white. Many of Bottreau-Roussel's cases showed osteitis of other bones, chiefly tibia and jaw as in the present case. All cases of yaws gave a positive Wassermann reaction. Whether the reaction persisted as positive in cases of goundou long after active yaws had disappeared was unknown.

A few cases of goundou had been written down as congenital, but in each case the statement depended on the history given years afterwards by the mother and probably should be discredited.

The relationship of this condition with what used to be called leontiasis ossium should not be lost sight of.

Dr. J. GRAHAM FORBES showed the photograph of a case of the rarer unilateral form of goundou which he had met with in West Africa, about twenty-five years ago, in a middle-aged native of the Gold Coast Colony. A hard bony tumour, the size of a large hen's egg, was situated below the right orbit, almost obliterating the palpebral fissure and displacing the nose to the left.

He said he had also seen in West African natives other bony deformities attributed to yaws, a disease which, though now known to be of spirochætal origin, was certainly not a syphilitic manifestation.

Dr. PARKES WEBER said it was important to remember that symmetrical hypertrophic non-suppurative, periostitic thickened conditions were sometimes seen in syphilis, especially in cases of congenital syphilis. He did not think that all cases of leontiasis ossea, or ostitis fibrosa of the face, were due to syphilis.

Dr. SALISBURY SHARPE (in reply) said he was glad to see among cases of frambœsial goundou one in which the condition was unilateral, as a case was mentioned in Castellani and Chalmers' "Tropical Medicine" of unilateral swelling of the same character seen in this country by Cantlie. In the Report of the United Food Company of New York for 1927 there were notes of a case, diagnosed as goundou, in which the enlargement was unilateral.

Stenosis (Co-arctation) of the Aortic Isthmus with Subcutaneous Pulsating Arteries on the Back.—F. PARKES WEBER, M.D., and F. KNOP, M.D.—Patient, L. C., an Italian, aged 54, until three years ago a carter distributing ice-blocks. Sent to German Hospital September 27, 1928, for chronic bronchitis and cardiac weakness. On admission the tortuous, hard subcutaneous arteries (pulsation detected on careful palpation), symmetrically grouped between the scapulæ and on both sides over the lower back of the thorax, could be both felt and seen projecting beneath the skin, and showed at once that this was a case of stenosis (co-arctation) of the aortic isthmus of the so-called adult type. In no other condition are subcutaneous arteries of the kind known to have occurred.

Patient says he has always been short of breath, but during the last six years has suffered from gradually increasing bronchitic trouble, which obliged him to give up his work.

Present Condition: Heart somewhat enlarged to left; impulse heaving; apex-beat felt in sixth intercostal space, one inch to left of nipple line. Loud systolic murmur, best heard at apex. X-ray examination of thorax excludes aneurysm. Brachial blood-pressure (September 29) is the same on both sides and remarkably low compared with that in many cases of the kind; it is: systolic, 125 mm. Hg, and diastolic, 70 mm. Hg. Rather weak pulsation can be felt in the femoral artery at both groins, but none in the arteria dorsalis pedis on either side. Blood-count

(September 28): Hæmoglobin, 98 per cent.; erythrocytes, 4,392,000; white cells, 6,600 (polymorphonuclear neutrophils, 80 per cent.; lymphocytes, 5 per cent.; monocytes, 7 per cent.; eosinophils, 8 per cent.). Urine: free from albumin and sugar. Wassermann reaction, negative. Patellar and Achilles reflexes not obtained on either side by the ordinary method, but slight patellar reflexes can be elicited by Jendrassik's "reinforcement" method. Nothing abnormal by ophthalmoscopic examination. External sexual organs normally developed. The patient has had eleven children, of whom seven are living.

To show position of the subcutaneous arteries that can be easily felt.

The reason why the patient was able to follow a laborious occupation till three years ago is probably that the arterial collateral circulation was good, and that the aortic isthmus was only stenosed and not blocked by the paulo-post-natal malformation in question.

The literature on the subject was summed up by one of us in April, 1927,[1] and shortly afterwards an excellent example, in a young man, was shown at the Belfast Meeting of the Association of Physicians (1927) by Professor MacIlwaine and Dr. Turkington. An example in a child, with malignant aortitis (beyond the stenosis), was communicated by Drs. Poynton and Sheldon at the Liverpool Meeting of the Association of Physicians (1928). These, and some cases from St. Bartholomew's Hospital, and Dr. C. E. Lakin's case from the Middlesex Hospital, have as yet not been published, but Dr. Donald Hunter[2] published a case last year, in a boy, aged 14, from Guy's Hospital. Another case, in a boy with *Staphylococcus albus* endocarditis, was described by G. M. Mackenzie.[3] In the third edition of "Modern Medicine" (1927, iv, pp. 772-787) Dr. Maude Abbott analysed the accounts of 237 cases. Dr. F. H. K. Green, in his paper on "congenital" cerebral aneurysms (1928),[4] drew special attention to the fact that in one of his cases the aneurysm was associated with stenosis of the aortic isthmus, a bicuspid aortic orifice, and fenestration of the mitral cusps. A very important paper on the whole subject (with new cases) is that by Dr. L. Minor Blackford, of the Mayo Foundation (1928).[5] In recent numbers of the *American Heart Journal*, Drs. W. F. Hamilton and Maude Abbott[6] have described a case of complete obliteration of the aortic isthmus (associated with bicuspid aortic valve) in a boy aged 14—cerebral death, and

1 F. Parkes Weber, *Proc. Roy. Soc. Med.*, Section of Medicine, 1927, xx, p. 29-42.
2 Donald Hunter, *Proc. Roy. Soc. Med.*, Clinical Section, 1927-1928, xxi, 34.
3 G. M. Mackenzie, *Amer. Journ. Med. Sci.*, Philadelphia, 1927, clxxiv, 87.
4 F. H. K. Green, *Quart. Journ. Med.*, Oxford, 1928, xxi, 419-432.
5 L. M. Blackford, *Arch. Int. Med.*, Chicago, 1928, xli, 702-735.
6 Hamilton and Abbott, *American Heart Journal*, St. Louis, 1928, iii, 381, 574.

impending rupture of the aorta. In that last paper all the cases are most elaborately considered, and a complete bibliography is appended.

Discussion.—Dr. J. W. CARR (President) wondered whether patients suffering from this condition frequently continued to lead a fairly laborious life until well into middle age. The stenosis in this case, even if not complete, must be very marked, as the collateral circulation was so pronounced. Why was the murmur best heard at the heart's apex?

Dr. PARKES WEBER (in reply) said he did not know how often a systolic murmur, as in the present case, was met with in association with stenosis (co-arctation) of the aortic sthmus.

He regarded the latter condition as the opposite of patent ductus arteriosus. Both were paulo-post-natal developmental abnormalities, that is to say, strictly speaking they were not congenital abnormalities. A patent ductus arteriosus was not an abnormality unless it persisted after birth. When the ductus arteriosus closed, and, so to speak, partially closed the aortic isthmus with it (that is to say, when the "closing process" extended beyond the ductus arteriosus—its normal limit—so as to involve the aorta) the result was "co-arctation" (stenosis) of the aortic isthmus, as in the present case. When that occurred, the adaptive compensatory powers of the new-born child were, nevertheless, able to establish a collateral arterial circulation. This reminded one of the wonderful reparative powers exhibited in some young animals which had been experimentally injured.

Case for Diagnosis: ? Gummatous Osteitis of the Humerus.—C. E. SHATTOCK, M.S.—J. B., female, aged 5, fell down a month ago and twisted her right arm. Since then she has not been able to use it.

FIG. 1.

FIG. 2.—Three weeks later.

The muscles around the shoulder are flabby and the child cannot lift the arm voluntarily. The humerus is not enlarged. A skiagram shows diffuse rarefaction of the upper part of the diaphysis, with no new formation of bone (fig. 1).

The Wassermann reaction is weakly—but definitely—positive.

Subsequent Note.—The Wassermann reaction of the mother is negative. A skiagram taken three weeks later than that exhibited at the meeting shows marked extension of the disease and involvement of the epiphysis (fig. 2). The disease is obviously malignant.

Section of Neurology.

President—Mr. Donald Armour, C.M.G., F.R.C.S.

[*October* 11, 1928.]

PRESIDENT'S ADDRESS.

Some Considerations on Head Injuries.

By Donald Armour, C.M.G., F.R.C.S.

John Bell, over a century ago, thus expressed the main thesis of this paper:—

"We have this encouragement to consider the pathology in place of the anatomy of the skull, as the rule of our prognostics, that while anatomy and the enumeration and classification of fractures have led to an undue propensity to operation, the study of the living powers, and the mutual dependence of these parts, leads to a reserved, modest and rational practice—to a just confidence in the powers of Nature—to a careful and solicitous attention to all the insidious symptoms of an injured brain."

In our own time the same plea has been made by Wilfred Trotter:—

"In the case of head injuries it is of cardinal importance that diagnosis and treatment should be kept in a close and living association with pathology, In every case it is essential that the surgeon, before attempting to begin the treatment, should make a determined effort to call up the clearest possible picture of the actual conditions within the skull of the patient he has to deal with."

A wise injunction that should be in the mind of every surgeon when dealing with head injuries.

Unfortunately, the subject of head injuries in surgical textbooks appears in the section on "Fractures," and thus early the mind of the medical man is imbued with an exaggerated importance of the "enumeration and classification" of fractures of the skull to the detriment of a proper appreciation of the much more important part of the subject, viz., the damage, functional or organic, that the skull contents may have suffered. This is constantly revealed by the early insistence upon an X-ray examination of the skull after a head injury, as if the presence or absence of a fracture of the skull were the important factor, and as if the result of the X-ray examination would be sufficient to determine how the associated cerebral injury should be treated. Moreover, its very use constitutes at times a pitfall in the prognosis of a head injury, because of the false importance attaching to the presence or absence of a fracture of the skull; its absence always presuming a better prognosis than its presence. For the same reason, there is a widespread lack of appreciation of the importance of so-called minor head injuries. "Minor" perhaps in the estimation at the time, of the patient and of his doctor, but often "major" in its later manifestations and sequelæ. It is to this aspect of the subject, namely, head injury without fracture, that I wish mainly to direct attention.

A general increase in intracranial pressure is the commonest result of a severe head injury. This is brought about, in the first instance, by reactionary œdema or hæmorrhage, or both. Later, this increased pressure may be aggravated or maintained by interference with the paths of absorption of the cerebro-spinal fluid, as the mechanical result of the swelling or displacement of the brain itself. This swelling or displacement interferes first by blocking the cerebral subarachnoid spaces, and as a result the cerebro-spinal fluid collects in the basal cisterns and in the ventricles.

The enlargement of the basal cisterns forces the mid-brain up, occluding the narrow spaces of the incisura tentorii. The swollen brain, with its distended ventricles, pushed up against the resistant dura, interferes still more with absorption and so a vicious circle is established. Added to this, we have the direct pressure on the medulla of the fluid in the basal cisterns. The sum of all this mechanical

pressure is exerted upon the blood-vessels of the brain, either locally or generally, giving rise to focal or widespread symptoms.

Such is the result, at any rate, of a severe injury. Can the same factors, in a lesser degree, explain the symptoms associated with or following upon a so-called minor injury?

The term "concussion," according to Trotter, should be used

"to indicate an essentially transient state due to head injury which is of instantaneous onset, manifests widespread symptoms of a purely paralytic kind, does not, as such, comprise any evidence of structural cerebral injury, and is always followed by amnesia for the actual moment of the accident."

Two exceptions may be taken to this classical definition of concussion. Is it possible to affirm that the state is essentially transient, unless we limit the expression strictly to the unconsciousness and the immediate paralytic phenomena associated with it? Is it correct to affirm that "concussion of the brain" does not, as such, comprise evidence of structural cerebral injury?

How are we to explain the onset and persistence of those post-concussion symptoms so often included under the misnomer "post-concussion neurosis"? Undoubtedly, they are largely the direct expressions of disturbance of intracranial pressure; usually in the direction of hypertension, though in some few cases hypotension has been found.

Cassasa explains the mechanism of concussion as depending upon the identification of "a network of fine fibrils connecting the external wall of the blood-vessels with the surrounding brain tissue across the perivascular lymph spaces." It is conceivable that any sudden overfilling of the perivascular lymph spaces with cerebro-spinal fluid could produce laceration of a vessel by the tearing of its wall in the neighbourhood of such a fibrillar attachment.

On the other hand, without such an attachment, the laceration of a vessel surrounded by fluid could not be produced by any pressure exerted through that fluid, which would tend to compress the vessel but not to lacerate it. Such an increase of cerebro-spinal fluid in the perivascular lymph spaces might be caused by the cerebro-spinal fluid from the surface of the brain being driven into it by the pressure exerted by the change of shape of the skull, the result of a blow or a fall.

This change of shape under an area of violence is in the direction of flattening and diminution of space for the cerebro-spinal fluid in that area, which must find its way out through the various sulci of the brain. Such fluid as cannot do this must find a way into the perivascular lymph spaces in the reverse direction of the normal flow of the cerebro-spinal fluid in these channels. Now Weed's experiments have demonstrated that the perivascular lymph sheath—the Virchow-Robin's space—is directly connected with the finer spaces about the central ganglion cells.

Cassasa suggests that sudden physical distension of these periganglionic spaces by cerebro-spinal fluid, in cases of concussion, causes a direct physical change in the ganglion cell, which may explain all the phenomena of concussion of the brain. He thinks this simple change may occur without minute hæmorrhages, and explains the completely negative post-mortem findings in many cases of concussion.

Osuati and Giliberti go further in emphasizing the structural factors in concussion of the brain. Basing their opinion upon the study of one hundred cases, they conclude that not only is there actual cerebral injury in cases of concussion, but in a few instances complete resolution does not occur, and that there is a strong likelihood that secondary degenerative changes develop. They suggest the term "traumatic encephalitis" in place of "post-concussion neurosis."

Whether these engaging theories are capable of proof or not, there can be no doubt that the symptoms usually following concussion—the commonest of which is headache of varying quality—are the expression of an altered pressure of the cerebro-spinal fluid. Not only a stable alteration in pressure, in one or other direction,

but an instability of pressure, such as has been described in epilepsy, would seem to be the cause in a certain number of cases.

Mestrezat, Bouttier and Logre, examining a large number of cases of concussion without head wounds, found that 80 per cent. of them showed definite changes in the cerebro-spinal fluid. The essential features were an increase in tension and an excess of albumin, while the chlorides were normal, the sugar rather increased, fibrin absent and the permeability normal.

These authors interpreted such a fluid as being due to an aseptic disintegration of nerve tissue without meningeal involvement, combined with a marked tendency to hypersecretion.

This "formula of concussion" is also characterized by its evolution, the excess of albumin usually appearing at the end of two to three days, disappearing, as a rule, at the end of some weeks or months with return to normal. Sometimes there was an indefinite persistence, or a slow regression of the syndrome during a number of months.

They would use this "formula" for the diagnosing of an "associated concussion" in cases when no evidence of a head injury was adduced, but at the same time they would not exclude concussion, owing to the absence of an anomalous cerebro-spinal fluid.

A caution, however, is necessary in cases of associated nerve contusion giving the additional signs, more or less marked, of a meningeal hæmorrhage.

From a medico-legal point of view the possible imitation of this formula in syphilis of the nervous system is of the greatest importance, calling for a Wassermann test in cases showing a lymphocytosis, however slight.

In confirmation of these findings, Claude, Lamache and Dubar, have lately carried out similar investigations on the cerebro-spinal fluid of patients who had suffered from a head injury without fracture of the skull. Very few had suffered from total loss of consciousness. The time intervening between the receipt of the injury and the examination of the fluid varied from two or three weeks to several months.

These patients sought relief on account of such subjective symptoms as headache, dizziness, weakness, insomnia, tinnitus aurium, and visual fatigue. Increase in albumin was found in nine out of the twenty-two cases examined, the excess tending gradually to disappear as the symptoms subsided, following repeated lumbar puncture. Otherwise, the fluid showed no chemical or cytological abnormality. But the tension of the cerebro-spinal fluid was altered in all the cases except five; hypertension, hypotension and instability of tension were found, the first most frequently.

What explanation can be offered for such alterations in the tension of the cerebro-spinal fluid following head injury? Obviously, the increase following immediately upon a severe injury, with associated œdema or hæmorrhage, or both, is largely due to the mechanical causes already described.

The increase in tension following upon slight injuries and maintained over a considerable period may be due to such factors, acting alone or in combination, as: (1) Increase in the amount of cerebro-spinal fluid secreted. (2) Interference with its circulation and absorption. (3) Variations in volume of the cranial contents, nervous, ventricular, and vascular.

Granting that the choroid plexuses are the chief source of the cerebro-spinal fluid, whether the process be a secretion or a transudation, or, according to Mestrezat, a mere dialysation from the blood, can we find any conditions associated with or following upon cerebral trauma which will activate or inhibit their process?

The Effect of Trauma upon the Choroid Plexuses.—The system of the ventricular cavities and their contained cerebro-spinal fluid has been said to be important in determining the distribution of force within the skull. The position of the choroid

plexuses would thus render them particularly vulnerable, exposing them to the direct action of the wave of repercussion at the time of the accident. Can such trauma, if it occurs, affect their secretory activity?

Some writers consider that an altered fluid, following upon microscopic vascular and nervous lesions, and meningeal irritation, is the cause of a secondary or reactionary hypersecretion.

Duret thinks that trauma causes a cerebro-spinal shock, giving rise to disturbances of the cerebro-meningeal circulation which tend to a congestion of the brain mass and to the production of intracranial hypertension by lessening the cranial space, by venous stasis, and by augmentation of the circulation with increased flow of blood to the secretory organ, i.e., the choroid plexuses. In addition, he postulates an irritation of the meninges, ependyma and plexuses, by the waste products of disintegration in cases of minute pial hæmorrhages and of post-congestion extravasations, a selective poisoning as regards function, by the products of breaking down nerve tissue as a result of microscopic tears and contusions. In short, he would attribute hypertension to hypersecretion of cerebro-spinal fluid.

The causes of hypotension are equally hypothetical. Apart from that due to loss of cerebro-spinal fluid from trauma, following lumbar puncture, and at operations, all giving rise to a sudden hypotension, there remain the occasional cases following head injury without loss, in which the hypotension manifests itself, usually at a later date, by definite clinical symptoms. These may be, according to Leriche, intense frontal headache, dizziness, nausea, sometimes convulsive phenomena, stupor and even coma.

In head injuries, Leriche would explain the hypotension as due to the formation of a porencephalic cyst on the surface of the brain, a sort of supplementary subarachnoid cistern, resulting in a relative insufficiency of cerebro-spinal fluid and a consecutive hypotension. Most probably several factors may be responsible, such as a functional disturbance of the regulating centre of fluid tension, inhibition of the cells of the choroid plexuses, or local modifications of the cerebral circulation.

Foerster and Wartenberg have called attention to certain changes in the shape and position of the ventricles following upon head injuries. These changes are chiefly in the nature of a distension or dilatation of one or both lateral ventricles with a stretching of their summits in an upward and outward direction. The dilatation was usually greater on, or confined to, the side of the lesion—"hydrocephalus internus lateralis"—and the ventricle was drawn up towards the site of the lesion, "Ventrikelwanderung."

Moreover, this unilateral appearance may be met with in the absence of unilateral clinical findings. According to Foerster, this wandering of the ventricle takes place slowly and gradually. The final result is a thin membrane, comprised of arachnoid, pia, and the remains of the cortex covering a dilated ventricle, which he calls a traumatic porencephaly.

Wartenberg describes as an end-result of commotio cerebri, with numerous areas of hæmorrhages or softening, a persistent and progressive cicatricial shrinkage of the whole brain and an alteration in the shape of the ventricle, the latter dilating at the expense of the brain substance (passive hydrocephalus). He describes the whole brain as being drawn from its central position towards the side of the lesion—"Gehirnwanderung."

This passive hydrocephalus may be converted into an active one by a traumatic "meningitis serosa ventriculi," causing an irritation of the choroid plexus with an increased secretion of cerebro-spinal fluid.

Both these authors make a plea for the routine use of ventriculography (encephalography) in the late results of head injuries, particularly if operation is contemplated. Several of their cases are reported as being very greatly improved as a result of the ventriculography alone!

Block and Oppenheimer have made a comparative study of the intraspinal pressure, blood-pressure and intra-ocular pressure in head injury. In eleven cases of fracture of the skull, although the blood-pressure was below the average, the spinal pressure was above normal and the ocular pressure was above the average. They conclude that "no individual parallelism exists between intraspinal pressure, arterial pressure and ocular tension," but on an average a high pressure of one type is associated with a high pressure of the other types; the same is true of low pressure.

Clinically, any attempt to estimate intracranial pressure in the light of general blood-pressure findings is not of any practical value. The presence of initial shock following a severe head injury and lasting for some hours, associated as it is with a fall in the general blood-pressure, will mask the true intracranial condition. Again, the intracranial pressure must reach a dangerous stage of compression before it can affect the general blood-pressure in the direction of any marked increase.

Both in the acute and chronic stages of head injury the value of the determination of the pressure of the cerebro-spinal fluid is beyond question: accurate estimation must be made with a manometer. Attempts to estimate the pressure by counting drops per unit of time, or by the force with which, or the distance to which the fluid spurts, are equally fallacious; many extraneous factors may invalidate such conclusions.

It has been said that "normal pressure may be almost anything," and it has certainly a wide range. Ayer places a normal pressure between 100 and 200 mm. of water, regards as "suspiciously high" pressures between 200 and 250 mm., and as "definitely abnormal" those between 250 and 300 mm. Pressures over 300 mm. are always to be explained on the basis of intracranial pathology.

The presence or absence of blood in the cerebro-spinal fluid following head injuries is often stressed beyond its true clinical significance. Its presence is only of importance as a corroborative sign of intracranial injury, associated with bleeding from an intradural vessel. Its absence, however, does not exclude intracranial hæmorrhage, even be it subdural or subarachnoidal. From the point of view of treatment, neither the presence nor absence of blood in the cerebro-spinal fluid has any importance whatever. The *pressure* of the fluid as an indication of the degree of intracranial tension is the all-important matter.

The Eyes in Head Injury.—Immediately following upon injury, the pupils are more or less dilated, depending upon the degree of the initial shock, the reaction to light being correspondingly sluggish. During unconsciousness both these conditions are increased. As the stage of shock passes off the pupils usually return to their normal size and reaction to light, unless there is a definite increase in intracranial pressure, when the dilatation and sluggishness may remain. The pin-point pupil in the early stage of a supracortical hæmorrhage or œdema passes into dilatation as the stage of irritation passes into the paralytic stage.

Injury to cranial nerves occurs in about 12 per cent. of head injuries, usually in basal fractures. The order of frequency would appear to be the seventh, sixth, eighth, third, and second, the remaining ones being rarely involved. The frequency of combined involvement is not very great, occurring in nerves lying very close together (seventh and eighth; second, third, fourth, and sixth), in single fissure fractures, while in multiple fissures, the groups, both as to the nerves involved and the side affected, are very irregular.

Varying conditions are concerned in the production of these paralyses of cranial nerves. Probably the most frequent cause is the injury of the nerve by bony compression, more rarely from laceration from displacement of the walls of the foramen. Where there has been no bony displacement, injury by bruising or laceration of the nerve sheath, with more or less severe reaction, may have occurred. Certain cases are possibly due to compression from a meningeal hæmorrhage.

But I wish to refer more particularly to the effect of increased intracranial

pressure in the production of cranial nerve symptoms, more especially with regard to papillœdema, or, perhaps, more correctly "papillary stasis," as the French neurologists call it. Cushing and Bordley, some years ago, showed by experiment how rapidly a retinal œdema may be made to appear in association with certain forms of increased intracranial tension, particularly when the compression is brought about by the introduction into the cranial cavity of fluid under tension, and how rapidly also the œdema may disappear when relief from tension has been afforded.

In practice, it should be remembered that great assistance in estimating the grade of intracranial tension may be obtained by an examination of the eye-grounds. For in head injuries with increased tension there may be a degree of papillœdema proportionate to the severity and duration of the intracranial symptoms.

Early signs are those of a dilatation of the retinal veins with venous stasis and an œdema not sufficient to be described in terms of diopters. But, with increasing tension, there occurs an œdematous blurring, first of the nasal margin and then of the temporal margin of the disc, followed even by obscuration of the whole disc. As would be expected, fractures beneath the tentorium cause these changes more rapidly than those elsewhere. It is unusual, however, for them to appear within the first six hours.

In cases of local compression—not those of a generalized form of compression, in which the tension is equally raised in all parts of the cranial cavity—the examination of the discs may be of great aid in determining the side on which the primary focal compression is being exercised. This unilateral intracranial stasis may have confirmatory evidence in the condition of those extracranial vessels which empty into the cranial sinuses, as shown by a dilatation of the vessels of the scalp, forehead and eyelids and even a slight degree of exophthalmos.

Pidoux, in his study of the increased intracranial tension, sometimes found after the injury, calls attention to the frequent occurrence of a bilateral papillary stasis, independent of any basal fracture, or compression by effusion. With the relief of the tension the disc returns to normal.

Turning to the more practical side of my subject I would plead for the more frequent use of lumbar puncture as a therapeutic measure in both acute and chronic head injuries. While in no way a substitute for a decompressive operation in cases of high intracranial tension, and being even dangerous in some cases, owing to the risk of medullary compression at the foramen magnum, yet in milder cases of increased tension the slow withdrawal of fluid, repeated if necessary, is of great value. It often allows the postponement of an operation until all the conditions are suitable, or even the dispensing with operation altogether. The pressure of the fluid, not the quantity withdrawn, is, however, the determining factor as to the further conduct of the case.

Again, for patients obviously not requiring the major operation of decompression, but suffering from severe headache, extreme restlessness, irritability, vertigo, etc., repeated lumbar puncture is of the greatest value in mitigating or curing these distressing symptoms. Therein lies the value of the so-called "spontaneous decompression" in fractures of the skull, with leakage of cerebro-spinal fluid, a value, unfortunately, too often offset by the ever present danger of infection.

We owe to Weed and McKibben the knowledge that it is possible to lower the cerebro-spinal fluid pressure by the intravenous injection of hypertonic solutions, and, conversely, to increase the brain mass as well as the intracranial pressure by using hypotonic solutions. Haden, using a concentrated glucose solution, was the first to apply this knowledge clinically, while Cushing and Foley showed that the same results, though less pronounced, could be obtained by oral or rectal administration, which caused less disturbance of respiration and circulation.

Foley and Putnam believe that the decrease in intracranial tension is not due

solely to decrease in brain volume, but primarily represents "new ratios between secretion and absorption of cerebro-spinal fluid."

Weed and Hughson, as a result of more extended experiments, conclude that, while the normal cerebro-spinal fluid pressure is partially dependent upon cerebral venous pressure and to a lesser extent upon cerebral arterial pressure, it is for the most part independent of either. They further conclude that the alterations in cerebro-spinal fluid, following intravenous injection of hypertonic solutions, while partially parallel to the variations in cerebral arterial and venous pressures, are mainly independent of them and show much more marked depressions.

Fay found that the intestinal instillation of magnesium sulphate was almost twice as efficient as sodium chloride for the reduction of intracranial pressure. He attributes this to the fact that magnesium sulphate is non-dialysable, while sodium chloride is readily dialysed. He also points out that the administration of sodium chloride may produce a secondary wave of œdema and increased intracranial pressure, due to its absorption and immobilization in the tissues, with a subsequent attraction of fluid from the blood-stream. Fay calls attention to the fact that over-dehydration is possible and may be serious, if not fatal.

Howe has undertaken a series of experiments with the object of determining what solutions were least toxic and most efficient in lowering intracranial pressure when given intravenously. Testing a dozen hypertonic solutions he found some too toxic and others relatively ineffectual. Sodium chloride produced the greatest decrease in intracranial pressure of any substance used, but it is toxic unless administered very slowly. He, however, regards it as unfit for general clinical administration, on account of its immobilization in the tissues producing a secondary wave of œdema.

He found that dextrose was absolutely non-toxic and never resulted in the slightest disturbance of respiratory or cardiac action, no matter how much or how quickly it was given. Fifty cubic centimetres of a 100 per cent. solution given in less than a minute had no untoward action. He concludes that "dextrose is the only substance of this group which is non-toxic and produces a satisfactory fall in intracranial pressure."

The practical application of these measures for the relief of increased intracranial tension, whether in acute or chronic head injuries, is too obvious to require further comment.

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Section of Ophthalmology.

President—Mr. CYRIL H. WALKER, F.R.C.S.

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PRESIDENT'S ADDRESS.

Some Recent Changes in the Incidence of Ophthalmic Diseases.

By CYRIL H. WALKER, F.R.C.S.

MY remarks this evening will be confined to a review of the occurrence of eye disease in the comparatively narrow area of the Bristol district, and to a consideration of the extent to which it has altered during the last thirty-eight years. I should have liked to back up my conclusions with statistics, but no complete figures are available, and patients migrate from one clinic to another to such an extent as to render statistics unreliable and misleading. Lest it should be thought that my experience does not fairly reflect the state of the case, I have compared my opinions with those of my colleagues at the four ophthalmic clinics in Bristol. In the main their experience agrees with my own.

There appears to be an impression in the minds of some, who perhaps only take a superficial view, that the varieties of eye disease found in an ophthalmic clinic are more or less constant, and that common diseases will always remain so. In Bristol, at any rate, this is by no means the case; either the incidence of ocular disease has changed considerably during the past thirty-eight years or there are other influences, such as school clinics for example, which deflect patients suffering from these diseases into channels other than the consulting room or the hospital clinic.

It is not unlikely that the incidence of some ophthalmic diseases is changing even more rapidly now than in the past. A century ago about half the blindness in Bristol was due to the ravages of small-pox—and that within twenty miles of the cowshed where Jenner carried out his epoch-making investigations! Up to about 1840 nearly half the inmates of the Bristol Blind Asylum owed their blindness to small-pox. It is now more than thirty years since we had anyone in that institution blind from that disease.

In some instances the cause of the change is fairly obvious; for example blepharitis was excessively common, both in the squamous and the ulcerative form, in all the eye clinics in Bristol until thirty years ago. It has become less and less common, and now we only get a few occasional cases.

Dirt and neglect appear to have been mainly responsible for the appalling frequency of blepharitis, and cleanliness and improved hygiene for its cure. Trachoma, on the other hand, has not been common in Bristol for fifty years, and during the last thirty it has been very rare. The few cases we get are mostly imported from South Wales or from Ireland.

Phlyctenular disease of the eye is another condition which has become much less frequent: there was a sudden and very noticeable diminution in the early years of the war, and there has been a further slight reduction in its frequency during the last ten years. It seems probable that the greatly improved conditions of the poor, thanks to the good wages and abundance of work during the war, were mainly responsible for this, and that lack of adequate food was the chief cause of the disease. I will here describe one of the few cases of phlyctenular keratitis that I have met with in private practice. The patient was a fat but flabby child aged two and a half years. There was no history of tubercle, but she frequently had a few spots of impetigo. The blepharospasm was intense, and no sooner had one patch of keratitis cleared up than a relapse occurred or a fresh patch began. The parents spared no expense and provided the best of everything. The patient was sent to me on ten occasions by the family doctor, each time with some fresh attack of keratitis. It seemed impossible to combat the recurrence; one day, however, I discovered that

the child was induced to take her food by having sweets given to her before meals, and was rewarded with sweets after meals. She had sweets before lying down in the afternoon and when she went to bed at night: in fact she had sweets on every conceivable occasion. When sweets were stopped entirely recovery began at once. No other alteration was made either in diet or treatment, and since then there has been no recurrence. It seems probable that in this instance the patient was being poisoned by the ingestion of excess of sugar.

There can be no doubt that besides improved food and hygiene the widespread recognition of nasal obstruction and the removal of tonsils and adenoids have greatly helped in the treatment and even the prevention of phlyctenular disease.

It is many years since I have seen acute gonorrhœal ophthalmia in an adult, whilst ophthalmia neonatorum is much less frequent than formerly. Since notification became compulsory there has been a steady reduction in the number of cases reported annually; in 1922 it was 119, in 1927 only 39. The chief causes of this improvement appear to be the education of the public, V.D. clinics, and (most important of all) the instruction of nurses and midwives and, through them, of the mothers. There had always been difficulty in persuading nurses to use the Credé method, or some other efficient prophylactic measure. The reaction, following the use of nitrate of silver, rendered it objectionable. Substitutes, such as argyrol and protargol, seemed unreliable or ineffectual. About four years ago the instillation of a 1 per cent. solution of mercurochrome was introduced as the routine treatment at our various ophthalmic clinics, and was soon adopted in the maternity departments at the infirmary and hospital. This method has proved eminently successful. With the superseding of nitrate of silver by milder treatment it seems likely that one of the causes of trichiasis and entropium is being removed. It is too soon for us to be able to judge properly the results of these precautions and of this treatment. Our worst cases of ophthalmia neonatorum now come from districts outside Bristol, where precautionary measures and early treatment have been neglected, but, even including these cases, loss of sight from ophthalmia neonatorum is now an excessively rare event. It must be assumed that the same is true for a great part of the West of England. The percentage of pupils in the Bristol School for the Blind, who are there on account of this disease, is only about one quarter what it was fifty years ago, and many of these have so little defect that they would at that time have been educated in an ordinary school.

Largely as a consequence of the improvement regarding phlyctenular disease and ophthalmia neonatorum, nebulæ and leucomata of the cornea are much less frequent. This is especially noticeable in doing retinoscopy. I should like here to mention the comparative rarity of nebulæ in some of the rural districts round Bristol. In 1916-17 I was engaged at Taunton examining recruits who had defective sight. The A class had long been called up. These men were mostly C 3: many had gross, uncorrected refractive error, but relatively few had corneal opacities. It would seem that in rural districts there is less chance of septic corneal ulcers, both in children and adults. Whilst this, I believe, is generally true, it must be admitted that some of our worst cases of corneal ulcers are from the country, where injuries, supposed to be trifling, are at first neglected. This is particularly noticeable in workers in stone quarries who are so familiar with foreign bodies in the eye that the injury is at first ignored.

Lamellar cataract is now infrequent, and the collateral malformations of teeth and bones are rarely seen now that the cause and prevention of rickets are understood. This condition of the lens is indeed so rare that it is very difficult to find cases for demonstration. All varieties of senile cataract are as numerous as formerly, excepting glassblowers' and furnacemen's cataract. Whether it is due to some nutrition defect in childhood or even before birth, little reduction can be expected in the frequency of senile cataract until its causation is better understood.

At any rate it does not seem probable that any of the much-vaunted remedies for incipient cataract, such as alkaline calcic iodide, will materially affect hospital or private practice.

Cyclitis and irido-cyclitis appear to be more frequent now than formerly, but the "typical" variety of simple cyclitis, with the characteristic triangular deposit on the back of the cornea is certainly not more common than it was twenty or thirty years ago. The irido-cyclitis now so often met with both in private practice and at our ophthalmic clinics is characterized by a deposit of very fine dots covering the whole of Descemet's membrane and presumably the whole of the walls of the anterior chamber. In the first instance they are so small that they can only be seen with the aid of the slit lamp or on very careful focal illumination. The condition, coming on spontaneously, or in some cases five or six weeks after an operation, which, up to then had done quite well, has in almost every instance proved exceedingly intractable. Salvarsan, or allied treatment, has in one or two cases seemed to be beneficial; vaccines may in a few instances have done some good, but in many cases treatment has proved of little value, and the inflammation has subsided only after a long period with the use of hot bathing and the applications usual in cases of iritis. The causation in these troublesome cases is obscure. Examination of the nose and throat, of the teeth and of the excreta has in most cases afforded no clue. The majority of such cases are apparently of endogenous origin, any operation that may have been performed acting only as a determining factor. Some of the cases present the clinical features of a sympathetic ophthalmitis, but of course the possibility of that cause has been excluded in all the cases to which I refer. I have dwelt upon this class of irido-cyclitis because its frequent occurrence forms a new and very unpleasant feature of ophthalmic work.

Of other forms of iritis, that due to some toxæmic condition is still about as prevalent as it was thirty years ago; but as a result of V.D. clinics, and the education of the public, syphilitic iritis is now almost unknown. When I first came to Bristol I was much struck by the amount of interstitial keratitis. Not only was it common, but it was severe, intractable and irregular. Such statistics as I was then able to collect showed that it was about three times as common as at St. Bartholomew's Hospital. There has already been some diminution in the number of cases and I think it may be assumed that ten years from now interstitial keratitis will have become quite rare.

Acute primary glaucoma is very much rarer in this district than it was twenty years ago. Cases with all the acute symptoms, eructation, vomiting and prostration used to be by no means rare. We still find acute secondary glaucoma, for example, after thrombosis of the retinal vein, but it is two or three years since I have had an acute primary glaucoma under my care. The change has been gradual; I cannot account for it, for one used to find it in all classes and in all ranks of life. Chronic glaucoma, on the other hand, is at least as common as ever. So far as my own experience goes, it is actually becoming more common. I have no satisfactory explanation of this. Early detection and treatment of the chronic cases may have prevented their becoming acute, but this would not account for the disappearance of all the acute cases one used to encounter.

Some other diseases have almost disappeared owing to successful treatment of the conditions which produce them. Conjunctivitis and ocular paralysis resulting from diphtheria are, I suppose, now almost unknown in England. The exophthalmos of Graves's disease is rarely seen, owing to the surgical treatment of that disease in its earlier stages.

Any diminution in the number of patients attending our ophthalmic clinics, as a result of the partial elimination of the diseases we have been considering, is more than counteracted by the increase in refraction cases. This does not imply that ametropia is on the increase. On the contrary I believe it is actually decreasing—

but small amounts of refractive error cause more discomfort as the advance of civilization makes more demands on the sight. It is a moot point whether the suggested relief of the eyes afforded by the gramophone or "wireless" is not more than counterbalanced by the strain caused by the cinematograph.

"Myope schools" have not been instituted long enough and are not so generally available as to enable us to detect much change as yet in the number of cases of high myopia. The usefulness of the "myope school" must not be measured only by the number of pupils sent out with mitigated myopia or even saved from disastrous eye disease. It has an educative value which is even more important. The public is still lamentably ignorant about the dangers of myopia, and it is only by educating all classes that proper precautions for the prevention of myopia will be taken. When the pupils from "myope schools" become parents they will take care that their children are brought up and taught so as not to aggravate the myopia to which they may have a hereditary tendency.

Of diseases that are newcomers to ophthalmic clinics, encephalitis lethargica is almost the only one that has become at all frequent, and it is to be hoped that it is only transient. Doubtless others will crop up from time to time as the conditions under which we live are altered.

The substitution of oil and petrol fumes for the dust and decaying vegetable matter that used to make our streets insanitary, the doing away with gas fumes by the introduction of electric heating and lighting (with the accompanying change in the character of the light), the introduction of new drugs, these and numerous other factors may very possibly affect the occurrence of eye disease. I have attempted to show that, whilst great changes have taken place in the past, similar, or perhaps greater, changes are taking place now. All ophthalmologists have to recognize their indebtedness to their colleagues in medicine, surgery and public health, and we in this Section must try to repay this indebtedness by promoting the science of ophthalmology in order to further the elimination of eye disease.

A Large Implantation Cyst of the Conjunctiva.

By E. Wolff, F.R.C.S.

Implantation cysts arise by the traumatic implantation of skin, epithelium, or hair bulbs into the underlying connective tissues [1].

According to Parsons [2] a cyst is produced by degeneration of the central part of the epithelium. It increases in size by the secretion of the epithelial cells, which are, as it were, inverted, and also probably by a desquamation of the innermost cells.

The contents of the cyst vary according to the type of the epithelium which produced them. Thus, they may contain clear watery fluid, mucoid material (in the conjunctiva), hairs, sebaceous material, and, according to Bland-Sutton, in sheep may contain wool and fat.

They are most common on the fingers [3] and in the palm of the hand, where punctured wounds which give rise to them are most frequent. In the conjunctiva they are uncommon. They occasionally develop there after injury, especially at the site of tenotomy wounds, or around foreign bodies (Parsons).

Parsons also says that only three cases had been subjected to histological investigation (Treacher Collins [4], Uhthoff [5]).

Treacher Collins' classical case was that of a boy, aged 9, who had been wounded by a screw-driver when 9 months old. There was a scar in the cornea; the cyst was semi-translucent and contained a clear watery fluid. It did not communicate with the anterior chambers and was lined by squamous epithelium.

According to Uhthoff the differential diagnosis is easily made from a lymph cyst by the fact that the lymph cyst is placed in the superficial layers of the

conjunctiva, whereas the implantation cyst is obviously deep to it. He also points out that the time or appearance of the cyst after the injury may be as early as five weeks, or after many years.

The following case has a number of points worthy of record :—D., a man aged 60, was seen by me at the Royal Northern Hospital. A piece of coal hit his right eye five years ago; he had been treated and the wound healed satisfactorily. Three years ago a swelling had begun at the site of the wound and had steadily increased in size. On examination, there was a cystic translucent swelling, which protruded from between the eyelids and prevented their closure. It was situated for the most part to the inner side of the cornea, but bulged over the upper and inner part of this structure. It extended upwards to the upper fornix, downwards to the lower fornix and inwards to the inner canthus.

The whitish scar of the original injury could be seen in the conjunctiva near the cornea, and at this point the swelling was notched. There was some choroidal disturbance far forward on the inner side and there were nebulæ on the cornea (fig. 1). A diagnosis of implantation cyst of the conjunctiva was made.

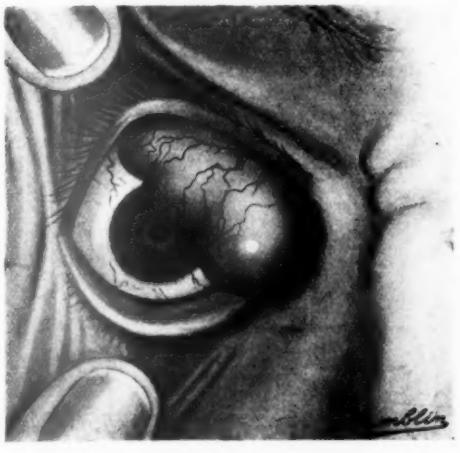

FIG. 1.—The cyst before removal.

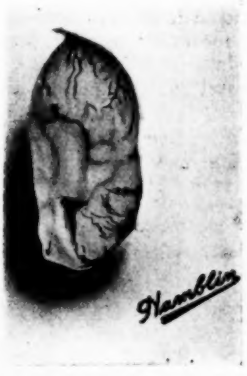

FIG. 2.—The cyst after removal.

The cyst was dissected out whole. It lay, as is usually the case, between the conjunctiva and the sclera, and was but loosely adherent to both except at one point. (In most of the cases described the cyst was matted to the conjunctiva and sclera by inflammatory tissue.) It was therefore easy to dissect the conjunctiva off the cyst and the cyst from the sclera. At the site of the injury, however, the conjunctiva was firmly attached to the cyst, and, as seen in the figure, had to be removed with the tumour. The eye healed rapidly and now appears normal. His vision (owing to the nebulæ) is $\frac{6}{18}$.

The cyst, after excision, measured $1\frac{1}{8}$ in. (2·85 cm.) long, $\frac{3}{5}$ in. (1·45 cm.) broad, and $\frac{1}{2}$ in. (1·3 cm.) deep.

On the surface were several small blood-vessels (fig. 2).

The cyst contained a clear, viscid, mucoid fluid. A piece of the wall was removed and sectioned and found to be lined with several layers of squamous epithelium.

The measurements and pathological section were kindly done for me by Mr. Stallard of the Pathological Department of the Royal London Ophthalmic Hospital, Moorfields.

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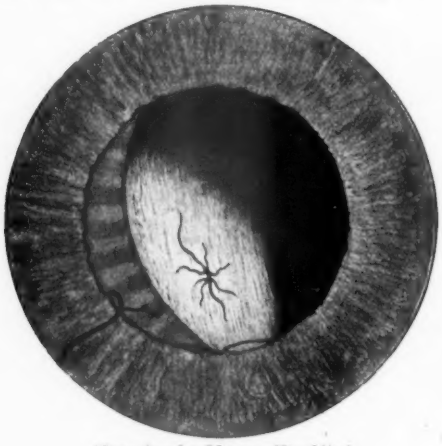
Mr. TREACHER COLLINS said he had seen such cysts under the conjunctiva after squint operations. In the case of his own to which Mr. Wolff referred there was an implantation cyst between the superficial layers of the cornea; it looked like a staphyloma of the cornea clinically and it was not until it was examined pathologically that it was found to be a cyst lined by squamous epithelium. There was always a question in these cases as to whether they were really implantation cysts or downgrowths of epithelium from the surface; whether there was a connecting link between the surface epithelium and the epithelium which lined the cyst, or not, or whether epithelium had extended along a wound and then grown round a space sub-conjunctivally or been separated from the surface and implanted. In cysts in the anterior chamber the epithelium lining it was sometimes connected with the surface epithelium along a scar in the cornea; these would not be true implantation cyst. That detached pieces of epithelium might be implanted in the eye and grow was proved many years ago by Masse, who had made experiments on rabbits by implanting detached pieces of epithelium into the anterior chamber of the eye.

CASES.

Subluxation of Lens and Persistent Posterior Vascular Sheath of Lens in an Infant aged 2 months.—A. C. HUDSON, F.R.C.S.—At first sight the appearance of the white mass behind the lens is rather suggestive of glioma of the retina, but on detailed examination there can be little doubt that it is due to a developmental abnormality, namely, persistence of the posterior vascular sheath of the lens. The points in favour of this diagnosis are: in the first place, the presence of a large vessel passing across the surface of the iris and bifurcating at the pupillary



(*Drawing by Messrs. Hamblin.*)

margin, one branch passing inwards behind the iris, probably to join the hyaloid artery, and the other outwards behind the lens in the direction of the white mass, probably to join the central part of the hyaloid artery at the back of the mass; in the second place, the state of the blood-vessels over the greater part of the iris, which are larger and more numerous than one finds in the normal iris, and represent a primitive form of vascularization; in the third place the fact that it is possible to see the ciliary processes drawn out towards the white mass, and showing as finger-like processes in the gap between the iris and the border of the lens. The lens is almost spherical, and probably that, in the

absence of the suspensory ligament in the part from which the lens is subluxated, is accountable for the extraordinary prominence of the anterior surface of the lens, so that the iris is pushed forward against the back of the cornea in the upper and outer quadrant.

Discussion.—Mr. AFFLECK GREEVES said this eye was similar to one which he had examined microscopically and described a few years before. In that case the lens had been dislocated in the same way, and there had been one large vessel coming forward which, by cutting serial sections, he had traced from the persistent vascular lens sheath into the iris. There was also a persistent hyaloid artery, and blood was still circulating through capillaries in the mass of tissue at the back of the lens, and carried from them by the anterior vessel into the veins of the iris.

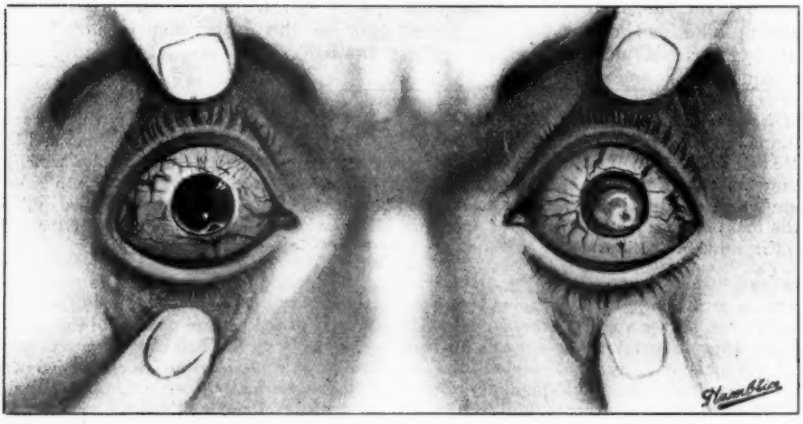
Miss IDA MANN agreed with Mr. Hudson as to the developmental nature of this condition. The main area of the iris resembled that seen in the embryo from the seventh to the eighth month, when all the vessels were visible and the atrophy of the central portion of the anterior vascular capsule had not proceeded as far back as the lesser circle. In the present case one could not make out any lesser circle of the iris. The large vessel curling over the edge of the iris she regarded as a capsulo-pupillary vessel which was, normally, seen only up to the third month, so that that represented an even earlier stage of development than the remainder of the iris. The persistence of that vessel and the fact that the lens was dislocated away from the position of the vessel were of interest, and might be said to have had some connexion in the relationship of cause and effect. If the vessels persisted on that side, the lens would tend to be displaced away from them. Long-drawn-out ciliary processes were often seen in maldeveloped eyes, and they seemed to be common in connexion with congenitally malformed lenses. She thought they were due to persistence of the normal adhesion of the ciliary processes to the lens, which was constant up to about the fifth month, and should then break down. In this case, however, it persisted and the processes got drawn out.

Old Detachment of Retina in a Woman aged 66.—HUMPHREY NEAME, F.R.C.S.—This case is shown on account of the complete disappearance of detachment of retina, the presence of many extensive patches of choroidal atrophy, and the retention, seventeen years after the detachment took place, of vision of nearly $\frac{6}{18}$.

Late Result after Mustard-gas Burn of the Eyes.—HUMPHREY NEAME, F.R.C.S.—In 1918 this man had mustard-gas burns in both eyes, and was in hospital for treatment three months while he was in the Army. Since then he has been engaged as a clerk, and his eye condition has not compelled him to stop work. This right eye now shows the appearance of pallor round the cornea, in the palpebral fissure, with engorgement of the vessels peripherally to that, reminiscent of the area of intense pallor owing to œdema in the early active stage of mustard-gas burn, and a more marked congestion peripherally. In the right cornea there is a depressed scar, with opaque margin, which presumably was an ulcer. He came because he had a hypopyon ulcer in the left eye, and a Saemisch section had to be performed. It healed up, but left a hopelessly blind eye. I shall be glad to hear from those who have seen the effects of mustard-gas on eyes as to whether this present condition is likely to be a sequela of mustard-gas burns.

Discussion.—Mr. J. H. FISHER said he had a case of mustard-gas burning of the cornea in a man who, subsequently to the War, was at work in Woolwich Arsenal. He came with a hypopyon ulcer, first in one eye, and at a later date in the other. He did not do a section on either of those eyes, but adopted the method of suturing the lids, as he would have done in a neuro-paralytic condition associated with hypopyon. The eyes did well under that. He thought he would have adopted that course in this man's left eye, instead of operating by Saemisch's section.

In these cases, he considered, there was considerable destruction of the epithelial layers during the acute stage of the mustard-gas irritation, and the destructive process must involve



the nerve-fibril endings; the surface of the cornea was rendered anæsthetic, and so was particularly liable to traumatism. The epithelium having been imperfectly renewed, the cornea was specially liable to ulcerative processes.

Mr. AFFLECK GREEVES said that in his experience in these cases of gas injury when the cornea had been severely damaged there was a tendency to repeated attacks of corneal ulceration. Healing took place slowly under treatment, and the ulcers were liable to recur at varying intervals.

MISS IDA MANN showed a Case of Implantation Cyst in the Anterior Chamber.

MR. HUMPHREY NEAME showed a Case of Chronic Proliferating Conjunctivitis in a girl aged 19.

Section of Therapeutics and Pharmacology.

President—Professor J. A. GUNN.

[*October* 9, 1928.]

PRESIDENT'S ADDRESS.

Cardiac Stimulants.

By J. A. GUNN, M.D., D.Sc.

[ABSTRACT.—An attempt is made to systematize our knowledge of certain drugs which are supposed to exercise a direct or indirect stimulating action on the heart. The actions on the circulation of strychnine, alcohol, camphor, adrenaline and pituitrin, which have been taken as representative examples, are briefly reviewed, with the object of separating out points in their actions in regard to which clinicians and pharmacologists are in general agreement, and of suggesting explanations for some of the dissensions that remain. The suggestion is made that much of the prevailing confusion in regard to the actions and uses of such drugs is due to the fact that we have as yet no sufficiently accurate classification either of "cardiac stimulants" or of "cardiac failures," wherefore it is difficult to bring a disorder and its remedy into proper alignment.]

MEMBERS of this Section of Pharmacology and Therapeutics have more than once expressed the opinion that the question of cardiac stimulants would be suitable for discussion, and it was suggested that I might utilize the presidential address as a preliminary survey of the subject, in the hope of drawing attention to points that might profitably be discussed.

The word "stimulant" is generally taken to mean something which produces a prompt but transient increase of activity of an organ, whereas the word "tonic" usually conveys the suggestion of an increased activity or tone which is brought on more slowly, but which is more enduring. Though both terms are often loosely employed, this distinction is fairly clear in regard to so-called stimulants and tonics of the heart. Though it would be legitimate from some points of view to regard, for example, the intravenous injection of strophanthin as a cardiac stimulant, I know that you do not wish me to consider the members of the digitalis group. Much research has been done on them, and the nature of their actions and their sphere of use have been delimited with much precision. There is, however, no similar agreement as to the influence on the circulation of such substances as camphor, alcohol, or strychnine. Not only is there among pharmacologists disagreement as to their method of action, but there is the utmost diversity of opinion among clinicians as to their therapeutic value. The task I have set myself is to review an admittedly heterogeneous group of drugs which are sometimes included under the designation of cardiac stimulants, in the hope of throwing into relief such facts as are generally agreed upon, and of suggesting explanations for some of the discrepancies.

It would seem logical, if not imperative, to begin by a strict definition of what is to be included under the term cardiac stimulant, but it will be convenient first to consider the action of certain drugs which have been included in this group.

We may begin with *strychnine*. That strychnine in certain circumstances increases the excitability of the central nervous system, and specifically, among other areas, the vasomotor and respiratory centres, is generally agreed. It is a matter of almost equal agreement that strychnine has no direct action on heart muscle, e.g., on the perfused heart. Clinically, strychnine has been largely used as a stimulant where failure of the circulation with weakness of the heart is one feature, and, since I once saw early in my career an astonishing improvement in the pulse and respiration produced in an almost moribund patient by the subcutaneous injection of $\frac{1}{8}$ gr. of strychnine, I have believed that in such circumstances it may be very valuable. The point of dispute is of course how it acts. I do not believe that, so far as its action on heart muscle is concerned, strychnine ever lengthened or

saved a life. It is a central vasomotor and respiratory stimulant. The main effects of stimulation of the vasomotor centre are well known. The arterioles, especially of the splanchnic area, which are well supplied with vasoconstrictor nerves, contract, while the vessels of the heart, lungs and central nervous system, which are feebly if at all so supplied, dilate. The general effect, therefore, is to raise the aortic pressure and to drive more blood through those organs which are more immediately necessary for the continuance of life. So far as the heart is concerned, this redistribution of blood improves the heart-beat by increasing the blood-supply to the heart through the coronary vessels, which supply is mainly proportional to the aortic pressure; by this increase in its nutrition and oxygen supply the heart may benefit temporarily or more permanently. Even thus far, and in regard to its method of action, there is considerable agreement among clinicians and pharmacologists. Where there is conflict of opinion is as to when, if ever, it should be used, and what is its real therapeutic value. In regard to this I think there are three points which, though elementary, have not been sufficiently recognized; if they were, our knowledge of when and how to use strychnine might be more precise. With regard to the circulation, in the first place it would seem useless to employ it unless the primary failure is vascular and not cardiac, therefore precision of diagnosis is necessary before cases in which it is useful can be separated from cases in which it is useless. Secondly, presuming that a vascular and not primarily a cardiac paresis can be diagnosed, there are other factors which will modify the action of strychnine. It is well known, for example, that the reaction of the nervous system to stimulation depends upon the state of excitability of it at the time. It is probable that, if the vasomotor centre is depressed, e.g., by the action of toxins, or prolonged impairment of blood-supply, strychnine may have little or no action on it. Moreover, if the vascular failure is a capillary stasis, or if the vessel walls have lost their excitability, stimulation of the vasomotor centre can give no benefit. Lastly, there is the all-important question of dosage. I was pleased to find that Whitla stresses this point and advocates the hypodermic injection of $\frac{1}{10}$ gr., and says that it is probably valueless in a dose of $\frac{1}{20}$ gr. I have long been convinced of this, and believe that, especially in conditions of circulatory failure with consequent depression of the central nervous system, it is useless to give less than $\frac{1}{10}$ gr. to $\frac{1}{8}$ gr.

To sum up, strychnine in sufficient doses is useful as a stimulant to the vasomotor centre. In deep depression of this centre, or in cases of peripheral paralysis of the vessels, it will fail to act. If we were in a position to determine more exactly the nature and degree of vascular paralysis, it is probable that the sphere of utility of strychnine would be more narrowly delimited and the present divergencies of opinion as to its value would largely disappear.

No apology need be made for taking *alcohol* next, or indeed for being dogmatic in regard to it; the literature on the subject is vast and it would take more time than I have at my disposal to quote arguments for or against its use.

The antagonistic views in regard to the action of alcohol on the circulation are partly due to the fact that it is administered in two entirely different ways which are frequently confused. Its action must differ according to the method of administration, as must the conditions, if any, for which it is useful.

In the first place concentrated alcohol in the form of whisky or brandy in small amounts has been given, for example, for fainting in more or less healthy people. From experience I agree with the traditional and popular opinion that in such cases it brings about some improvement in the circulation. There is little room for disagreement as to how it acts in such a case, and there seems to be nothing mysterious about it. The effect with which alcohol is credited comes on too soon for it to be due to the action of absorbed alcohol, and it is too independent of the quantity, and too dependent upon the concentration, for it to be other than a reflex

action. Seeing that irritation of the mouth and upper part of the alimentary canal, among other forms of peripheral irritation, is known to stimulate reflexly the medullary centres, it would seem difficult for alcohol in an irritant concentration to avoid exercising this effect. If it is as simple as this, you may well ask why there is disagreement. One pertinent and perhaps sufficient cause for this disagreement is that it is useless to consider the action of such a reflex stimulant unless in relation to the condition of the nervous system of the patient at the time. Of this reservation there are two obvious applications. First, because neat brandy produces no marked reflex effect on a healthy conscious patient, it does not follow that it will not produce some effect on a patient who is semiconscious or unconscious owing to sudden failure of the circulation. Second—and this is only an apparent contradiction—though brandy may produce a reflex effect on the medullary centres in such a case of sudden circulatory failure, it does not follow that it will display the same effect in a patient suffering from gradual circulatory failure occurring in the course of a long illness. If the central nervous system be depressed, not from momentary defective circulation but from prolonged defective blood-supply, or from toxic action, the medullary centres may be long past stimulation by such reflex irritation. Indeed, physiological experiment has shown that in gradual paralysis of the vasomotor centre it is the effect of reflex stimulation that disappears first. Also it must be remembered that there is nothing specific and nothing dramatic about this action of alcohol. It cannot be compared with the action of an intravenous injection of adrenaline. It is an action which may be just worth while in a limited set of circumstances, in just such circumstances in fact as the common experience and judgment of mankind have decided in its favour.

But alcohol is used differently and in quite other circumstances for a supposedly beneficial action on the circulation. Some give it in prolonged illness—in pneumonia, in typhoid or even in impairment of the circulation in old age—in which cases alcohol has a doubtful reputation as a cardiac stimulant. In such conditions it is not usually given in such concentration as to have any reflex action. It may even be administered in such a menstruum as milk, which will mitigate such action. No sudden effect is expected from it, and it may be given continuously for days in regular quantities. Here we are concerned with the actions of alcohol after absorption; as these are highly complex, an estimation of its value in different conditions will be correspondingly difficult. It is not possible here to consider its general action on the nervous system, its antipyretic action or its food value, though the indirect results of such actions on the circulation may be far from negligible; we can only deal with the more immediate effects on the circulation.

Most pharmacologists hold that alcohol has no direct stimulatory action on the heart, and experiments on intact animals and on human patients have shown that it does not markedly influence blood-pressure. Opinions on these points differ; positive claims of some experiments being roughly cancelled out by the negative results of others. On the other hand, there can be no doubt that alcohol after absorption causes some redistribution of the circulation. The most inflexible prohibitionist will admit that alcohol causes flushing of the face, and the fact that the skin vessels dilate is beyond dispute.

The amount required to produce this effect varies in different cases but can be within the limits that are regarded as therapeutic. If more blood flows through the skin and the blood-pressure remains unaltered, there must be a constriction of vessels elsewhere, as has been shown for the splanchnic vessels. So far as concerns the direct action of absorbed alcohol on the circulation, however much other points may be in dispute, there would seem, therefore, to be common ground in the belief that alcohol, without conspicuously altering blood-pressure or pulse-rate, does alter the distribution of blood in the direction of increasing the blood flow through the skin at the expense of internal organs. As to when, if ever, such an action is to

benefit the patient generally, or favourably influence the heart beats specifically, it is difficult to say; but it does not leave things as they were and must alter them for better or worse. That faintness may result from mere coldness of the skin is a common observation, and alcohol by its action on the blood-vessels may improve the heart beat in certain circumstances by favouring the return of venous blood to the heart upon which the efficiency of its contractions so largely depends. It is difficult in our present state of knowledge to lay down laws for the administration of alcohol in the class of case which we are considering, and this difficulty is reflected in the divergence of clinical opinion. Until our knowledge becomes more precise, the therapeutic use of alcohol for its action after absorption on the circulation must remain in any particular case experimental.

Camphor.—If there is disagreement about the action of alcohol, there is hardly less dispute about the effect of camphor, and it is difficult to reach any satisfactory conclusion from the extensive and contradictory literature on the subject. Camphor has been widely used as a stimulant in cases of circulatory failure. The most divergent and conflicting results have been obtained from experiments on isolated hearts and on normal animals. No decisive and accepted proof has been obtained of any direct action on cardiac muscle. There are, however, some isolated points on which there is agreement. Hypodermically injected, camphor acts as a local irritant and reflexly stimulates the medullary centres. So far it resembles the reflex action of alcohol, and its use is subject to the same limitations. One would expect its action to be more prolonged than that of alcohol because the latter is soon diluted in the mouth and alimentary canal, whereas camphor, dissolved in oil, for example, will exert a more prolonged irritant action on the hypodermic tissues and maintain the reflex effects longer, provided the central nervous system is in a condition to respond to them. But, as with alcohol, more has been claimed than this. Camphor is reputed to have some beneficial action on the circulation after absorption, and, as with alcohol, it is here that views differ acutely. Attempts have been made to prove that, if camphor has no action on a normal heart, it does improve the beat in depressed conditions or in irregularities of the heart. Though digitalis, for example, would provide a kind of analogy for such a supposition, it is not quite the same thing to postulate an intrinsically or qualitatively different action on heart muscle in a normal and abnormal condition. On the other hand, the evidence that camphor has a dilating effect on the vessels, e.g., on the coronary and skin vessels, is more convincing. It may have some significance that, so far as their actions after absorption are concerned, both alcohol and camphor cause some redistribution of the blood. There is also the same type of disagreement as to their value. This suggests rather that the improvement in the circulation which is sometimes seen from camphor or alcohol is due not to direct cardiac action but to an alteration in blood distribution, and that the differences in the results claimed from them may be due to want of knowledge of when such an alteration in blood distribution will benefit the patient generally or the heart-beat particularly.

It is a pleasure to pass from the substances we have so far considered to the terra firma of *adrenaline*. So far as its main actions on the circulation are concerned, agreement is general, and it displays the same action in health and disease. It is a peripheral sympathetic stimulant. It augments and accelerates the contractions of the normal isolated heart, and has precisely the same action when the heart is powerfully depressed or even arrested by a wide range of toxic agents. It is the most powerful known stimulant of the heart. It also constricts most of the arterioles, so that intravenous injection of it usually causes a prompt, steep and high rise of blood-pressure. It is unnecessary here to consider its action further as it is so well known; but there are one or two points about it that are perhaps not so well appreciated. So far as its action on the vessels is concerned, the rise of pressure and redistribution of blood is similar to that produced by

stimulation of the vasomotor centre; splanchnic vessels are constricted and the vessels of the lungs, heart, and cerebral vessels are dilated. It therefore causes an important redistribution of blood. Though, in an animal with normal blood-pressure, the effect of adrenaline is transient, in conditions, for example, where a low blood-pressure and a feeble heart-beat are injuriously potentiating one another, adrenaline, by this alteration of the blood-distribution added to increased cardiac output, may produce a lasting improvement in the circulation. The transient nature of its action is therefore not a valid objection to it and is particularly unlikely to be true for those cases in which its use would be desirable. Another point is that its action is independent of the condition of the central nervous system; not only so, but it has been shown that the sympathetic terminations upon which it acts are for practical purposes as resistant as the muscle itself. In a tissue kept for days in cold storage, or in an inflamed appendix, for example, the action of adrenaline is demonstrable so long as the excitability of the muscle endures. On the other hand adrenaline is a two-edged weapon; it acts with greatest certainty when given intravenously, but, so given, it is a powerful poison in slight over-dosage. It throws a sudden heavy strain on the heart and vessels. The tendency has been therefore to restrict its use to forlorn hopes. It has been used repeatedly, often with success, in threatened or even complete arrest of the heart, when that is not due to fibrillation. For this purpose there is no remedy available (other than its immediate chemical congeners) that can be compared with it. It is not improbable that, with increasing familiarity with its action and doses, the clinical use of it in acute cardiac failure will increase, and possibly in the direction of employing repeated small intravenous injections rather than a single large dose.

Pituitrin offers a fundamental contrast to adrenalin in its method of action, though there is a superficial resemblance in their effects on the circulation. The action of pituitrin on the circulation is relatively simple. It stimulates contraction of the capillaries and smaller arterioles. It does not discriminate between the splanchnic and other vessels as adrenaline does and therefore does not cause the same redistribution of blood, though blood-pressure rises with both drugs. Pituitrin has no action on the heart and is of no value when circulatory failure is primarily due to failure of the heart. So far as the circulation is concerned it is useful only where it is desirable to contract the arterioles and capillaries. It has been shown that a variety of toxic agents induce circulatory failure from causing capillary dilatation, wherefore the return of blood to the heart and consequently its minute output is diminished. Capillary dilatation has been shown to occur from histamine, bacterial toxins, snake venoms, proteoses, etc. Indeed opinion has veered round considerably in recent years in the direction of regarding the circulatory failure, not only in shock but also in acute bacterial infections and other intoxication, as due frequently to paralysis of the capillaries rather than to failure of the heart. This would certainly seem to extend and enhance the therapeutic uses of pituitary. It has been tried sometimes with success and sometimes with failure, with the result that its value has been somewhat discredited. Even if capillary paralysis is accurately diagnosed, it is inevitable that a stimulant can act only if the tissue is capable of responding, and that this will vary with the intensity and duration of the toxic action on the capillaries. The tenacious action of toxins of the proteose type is well known. It is almost inevitable that in some cases of capillary paralysis, pituitrin will have no effect; in others it may produce a transient improvement but the circulation will fail again as the capillary paralysis returns. There is reason to hope that in cases where the damage to the vessels is less severe, pituitrin will have a more permanent action. With it, as with other drugs, clinical experience will no doubt in time discover when and where it may be helpful.

Beginning by waiving the exact definition of a cardiac stimulant, we have considered briefly the action of five important drugs which have been used for

beneficial action on the circulation with a direct or indirect improvement of the heart-beat. Other drugs might have been considered, or other factors in the action of the selected drugs might have been taken into account, but the foregoing may provide sufficient data for preliminary if limited conclusions.

With the reservations already made, the following résumé would seem to give a fair statement of agreed opinion in regard to present knowledge of the action of the drugs in question. Strychnine is a central medullary stimulant. Alcohol and camphor, according to their method of administration, may (1) provoke reflex medullary stimulation, or (2) (after absorption) cause a redistribution of the blood. Adrenaline is a peripheral sympathetic stimulant and pituitrin is a direct peripheral stimulant of the arterioles and capillaries.

So far as the heart is concerned, that adrenaline stimulates it is proved, whereas it is more than doubtful if any of the other drugs have any such direct action.

So far as the vessels are concerned, two factors especially have to be kept in mind: (1) the effect on the mean blood-pressure, and (2) the alterations in the distribution of blood with or without changes in blood-pressure; in the present connexion the latter is possibly the more important. Reflex (alcohol or camphor), central (strychnine) or peripheral (adrenaline) stimulation of the vasomotor system agree in causing a redistribution of the blood, in the general direction of driving it from the abdominal viscera into the heart, lungs and central nervous system; but the site of their action is of importance in determining which, if any, will exert this action in particular pathological conditions.

Alcohol and camphor, after absorption, cause a different redistribution of the blood, in the direction of driving blood into the skin vessels at the expense of the abdominal viscera. Pituitrin has no such selective action on arterioles (apart from the kidney vessels which are outside our present consideration), but has possibly more effect than any of the other drugs in causing contraction of the capillaries.

It is clearly impossible to coerce substances so dissimilar in action into a common group, and it is obvious that, from the therapeutic side, there are all the elements necessary for confusion until we have a more accurate classification of (1) "cardiac stimulants," and (2) "cardiac failures." Two examples may illustrate this. Cotton and Lewis have described fainting attacks due to vagal stimulation and relieved by atropine. Inhalation of ammonia has been used as a stimulant in fainting attacks. This inhaled ammonia irritates the afferent fibres of the fifth nerve in the nasal mucous membrane which is believed to stimulate the vagus centre. One would suppose, therefore, that in vagal syncope inhalation of ammonia would be likely to aggravate rather than relieve the condition. But there is no indication of this so long as we speak generally of "cardiac failure" and "cardiac stimulant."

Cardiac failure may occur early in chloroform administration from ventricular fibrillation, or late in the administration from paralysis of cardiac muscle with respiratory and vasomotor paresis. In the first type of failure adrenaline, if it could reach the heart, might do more harm than good; in the second type of cardiac failure adrenaline may save life. "Cardiac failure" due to one poison may therefore be aggravated or relieved by the same "cardiac stimulant."

So far as the "cardiac stimulant" is concerned, at least three definitions are possible. It might be used to denote a drug which in any way, directly or indirectly, increases the output of the heart. This would include, for example, all the drugs we have discussed and would give no indication of how they acted or for what conditions they would be useful. Secondly, the term might be limited to drugs which increase the minute output by some action on the heart or its nerve supply. This would exclude vascular effects which secondarily improve the heart beat, but would still include drugs like adrenaline, atropine, ammonia, etc., which act differently, and the therapeutic applications of which are not interchangeable. Lastly, there is a strictly limited definition as stated for example by Wiggers: "a cardiac drug will be regarded

as a true stimulant if it produces an increased ventricular pressure maximum by a primary effect on the heart operating under the constant dynamic conditions." If the term cardiac stimulant is to be retained at all, it would seem necessary to restrict it to the last definition. If one must have descriptive terms, clumsy though they may be, one must call strychnine a central medullary stimulant, strong alcohol a reflex medullary stimulant, pituitary a peripheral vascular stimulant and so on. The specialists may grow independent of nomenclature but others may easily be misled by loosely associating under a common designation drugs with inherently different actions.

Similar precision is necessary on the clinical side. One is far from suggesting that it is ever easy, or even in the present state of knowledge always possible, to determine the primary cause of circulatory failure in a particular condition, but it is undoubtedly the ideal to be aimed at. Even now there seems no reason why, for example, syncope should be classed as cardiac failure, whether it be due to overaction of the vagus, paralysis of the heart muscle, or even dilatation of the vessels.

Research on the mechanism and disorders of the circulation has, broadly speaking, busied itself with the heart, the blood-pressure, the capillary circulation, and the venous return, roughly in that order, following the usual tendency to proceed on lines of ease of observation and especially of measurement. As failure of the circulation at any point is reflected in change in the heart-beat, and as the latter is relatively easily observed, the tendency has been natural and inevitable that the innocent heart has often been wrongly accused. With the advances in knowledge come classification and terminology, and it is probable that, with disorders of the circulation, the latter have not yet caught up with the knowledge we already possess. In short, one looks forward to the time when the description of a disease will cease to mention "cardiac failure" but will substitute vagal syncope, paralysis of heart muscle, or of the vasomotor centre or of the capillaries or whatever the primary fault may be. When these conditions can be not only detected and separated but also suitably named, and when a corresponding precision is introduced in the classification and terminology of cardiac remedies, disease and drug will be more easily brought into alignment, with a consequent gain to therapeutics.

Section of Urology.

President—J. Swift Joly, F.R.C.S.

[*October* 4, 1928.]

Surgery of the Prostate Gland with Report of Operative Results.

By Oswald Swinney Lowsley, M.D., F.A.C.S.

(From the Department of Urology [James Buchanan Brady Foundation] of the New York Hospital.)

Great progress has been made in the surgery of the prostate gland in the past few years. A review of this progress is interesting. The problem is complicated by the fact that all the patients are advanced in years.

The discovery of anæsthesia and the application of Lister's principles placed this branch within the scope of surgical aid. It was soon found that aged men, most of whom suffered from deficiency in renal power, tolerated ether poorly, and various attempts were made to avoid the frightful mortality that resulted from this operation under ether.

It was early recognized that those patients who were catheterized over long periods, if they survived, were in better condition for operation. This knowledge led to the practice of drainage as a routine preliminary to operation. This drainage may take one of three forms: (1) Regular catheterization once or twice daily; (2) retained catheter *per urethram;* or (3) suprapubic drainage under local anæsthesia.

I prefer the last method because the other two cause discomfort to the patients, and because, despite the most careful precautions, the patient will either be afflicted with a cystitis primarily, or have a secondary exacerbation of an existing infection.

The Urological Department of the New York Hospital is provided with suction in every room, so that the patients are kept dry and comfortable by attaching the suction to the inner of a double suprapubic tube which has been in use for many years. The urine rises in the large outer tube until it reaches the lower end of the small inner tube, which is surrounded by a column of air, and the patient's abdomen is thus kept dry without sucking the bladder wall into the end of the tube.

The general observation of the improvement in the renal power of patients by drainage was placed on a scientific basis by the development of accurate methods of measuring kidney efficiency. The improvement of these methods was stimulated no doubt by the epoch-making work of Geraghty and Rountree on phenol-sulphone-phthalein (1908). Various other tests are in use, including cryoscopy, estimation of output of various substances, such as potassium iodide and sodium chloride. The colour tests, phenol-sulphone-phthalein and indigo carmine, deservedly occupy the first place among kidney efficiency tests in the order named. The Mosenthal concentration test is an important recent addition to this list.

Perfection of methods of estimating retention products in the blood-stream of persons suffering from inefficient kidneys is a most valuable addition to our investigation of the patient, preliminary to operation. By determining this retention, by estimating the excretion of urea in a twenty-four hour specimen, and by a phenol-sulphone-phthalein test, one may get a true picture of the efficiency of the kidneys as eliminative organs.

The most important part of any operation upon the prostate gland is the preparation of the patient; this preparation consists of drainage by any effective method least annoying to the patient.

The patient having been prepared, as indicated by his laboratory tests and as judged by the operator, it is necessary to decide upon the type of operation. It seems most unwise for the urological surgeon to close his mind to any one type of operation. It can be clearly decided by the cystoscope or by digital examination, at the preliminary suprapubic cystotomy, whether the hypertrophic gland should be

removed by the suprapubic route or through the perineum. Unless there is a specific reason for the gland to be removed suprapubically, the perineal operation would seem preferable because, while the latter requires more time and an intimate knowledge of the anatomy, it is not nearly so great a shock to the patient. The reason for the shock accompanying the upper route is that there is always more or less pressure and traumatism to the complicated nervous plexuses over which one must work, thereby producing an effect similar to a blow over the solar plexus. The downhill drainage, the fact that the patient can sit in a chair after the third or fourth day, are additional arguments in favour of the perineal operation of Young and its modifications and of the excellent perineal operation of Voelker, of Halle, and Syms, of New York.

Next to the preliminary drainage, the choice of anæsthetic assumes a more important rôle than the choice of operation routes. Ether is dangerous because it is extremely irritative to the kidneys which, even in the best of cases, are already damaged. Chloroform is almost certain death, the patients succumbing in from six to ten days as a result of liver poisoning, as shown by the experiments of George Whipple. Of the inhalation anæsthetics, gas-oxygen is the most serviceable, but there are definite objections to it. Under all inhalation anæsthesias the blood-pressure is considerably raised, so that the patient may be likened to an athlete at the height of his activities. This naturally causes more bleeding and hence greater shock. Many attempts have been made to perform the operation under local or regional anæsthesia. Of these methods two have survived. Spinal anæsthesia as practised by many urologists from time to time in this country, and sacral anæsthesia, which has lately been modified by the addition of parasacral injections. The number of accidents under spinal anæsthesia have been so numerous and fatal that it has been largely given up by American surgeons. The results with sacral anæsthesia have been so spectacular in a recent series of cases that it seems worth while to discuss the method in some detail.

F. Cathelin ("Les Injections épidurales." Paris: Baillière, 1903), and Durant (1902) used sacral anæsthesia in treating grave neuralgias, sexual neuroses, and incontinence of urine. The former first used plain water, salt solution, and later added cocaine, novocaine, codeine, or morphine. Using this method, Cathelin reported 49 per cent. of the cases of incontinence of urine cured, 35 per cent. materially benefited and 4 per cent. failures. He failed in his attempts to anæsthetize completely the sacral nerves in humans, but was successful in dogs.

A. Löwen (*Zentralblatt f. Chirurg.*, 1910) used 20-25 c.c. of 1·5-2 per cent. novocaine, and placed the patient in a sitting posture for some minutes after injection, with the idea of retaining the solution in the lower end of the vertebral canal.

D. Gros (*München. med. Wochenschr.*, 1910) recommended the addition of sodium bicarbonate, which, he states, permits the solution readily to penetrate the nerve sheaths.

Strauss prepares a solution by the addition of sodium sulphate, which he maintains prevents the decomposition of adrenalin, which he also uses.

Hertzler recommends quinine and urea, using 60-90 c.c. of 0·6 per cent. solution.

Lewis, B., and Bartles, L. (*Surgery, Gynec. and Obstet.*, 1916) reported forty-eight successful cystoscopies out of sixty-eight attempted, and D. R. Pickens (*Jour. Tenn. Med. Assoc.*, 1916) reported eighty-one out of 100 attempted.

Splendid work on this subject has been done by Thompson, of Galveston, who follows the method described by M. L. Harris (*Trans. Amer. Surg. Assoc.*, 1917).

Albert J. Scholl (*Jour. of Urology*, August, 1921) was successful with sacral anæsthesia in 140 cystoscopies out of 150; in these he used the method recommended by Labat.

The cases in which this type of anæsthesia was first used by us were all extremely

grave. All except six cases have been entirely successful. In one of the unsuccessful, viz., a seminal vesiculectomy, a few whiffs of ether had to be given during the last five minutes of the operation; in another a perineal prostatectomy on a man of 86, a little gas or ether should have been used for about the same length of time. A third case was only moderately uncomfortable.

Untoward Symptoms.—Two patients have had short periods of excitement. One had spasmodic contractions of the legs, lasting about one minute. Two others became flushed and somewhat confused temporarily. In most of the cases there is an increase in pulse-rate and slight increase in blood-pressure throughout the operation.

The untoward symptoms in our cases appeared immediately after the injection was given into the sacral canal; caffein is always held ready in case of collapse.

E. Zweifel (*München. med. Wochenschr.*, 1920) reported a death from cardiac and respiratory paralysis, due to the injection of 8 grm. of novocaine into the dural space. His needle had penetrated the dural sac.

B. Kronig (*Operative Gynäkologie, Leipzig*, 1912) reported a case in which the novocaine was injected into the sacral vein. This was followed by a partial respiratory paralysis.

Pre-operative Preparation.—When first seen, the patient suffering from residual urine should have his bladder partly refilled with boric acid solution or sterile water whenever catheterized. The amount replaced can be reduced gradually if the patient shows no toxic signs from this procedure. This is called "decompression," and is of vast importance in preventing uræmia due to complete emptying of an over-distended bladder.

Following the period of decompression, I do a suprapubic cystostomy for drainage, using the suction apparatus devised by Kenyon, of the New York Hospital, and recently modified by Harrah.

The indwelling catheter method of drainage has many objections. The intrusion of a foreign body through the urethra prevents proper drainage from the prostatic tubules and causes additional congestion of the prostate. There is sufficient absorption from the prostatic urethra to keep the patient a little below par at all times.

The suprapubic drainage under suction seems to be the ideal method of procedure as a preliminary to operations on the prostate. It is efficient; the patient keeps dry, clean and odourless; the œdema of the prostate is reduced; absorption does not occur, and at the time of operation the hæmorrhage is considerably reduced. By using a satisfactory binder, the patient may sit up on his fourth post-operative day.

Vaccination of the patient by the organisms carried in his bladder occurs at the time of cystostomy. Thus we see that the shock of the prostatectomy is divided into two parts. Indeed, the patient is usually more uncomfortable after the first operation than after the second.

In regard to the period of preliminary drainage, it is important to have no set time for it. A succession of blood, chemical, and phenol-sulphone-phthalein tests determines when the patient has reached his maximum renal efficiency. If at the same time the patient feels well and is anxious to proceed, the prostatectomy is then undertaken. However excellent the results of the laboratory tests, if he feels weak, or even has a headache, it is wise to wait until he has entirely resumed a state of well-being. The shortest period of drainage in our series has been eight days, the longest twenty-four months; the average time is sixteen days.

As a result of hæmorrhage and infection, the patient may have a badly depleted circulatory system. In such cases transfusion of moderate amounts of blood, repeated if necessary, is particularly helpful.

Purgatives are given on the day before operation sufficiently early to ensure

a good night's sleep. Sodium bicarbonate is administered in small doses for a period of two days before operation. Fluids are taken up to, during, and immediately after operation, so that the patient is not dehydrated.

Operation.—The important features of an operation on the prostate are: (1) proper preliminary drainage; (2) the anæsthetic used: (3) the route by which one removes the gland.

General anæsthesia should be avoided because it raises blood-pressure and so causes more hæmorrhage during operation. As soon as the patient recovers

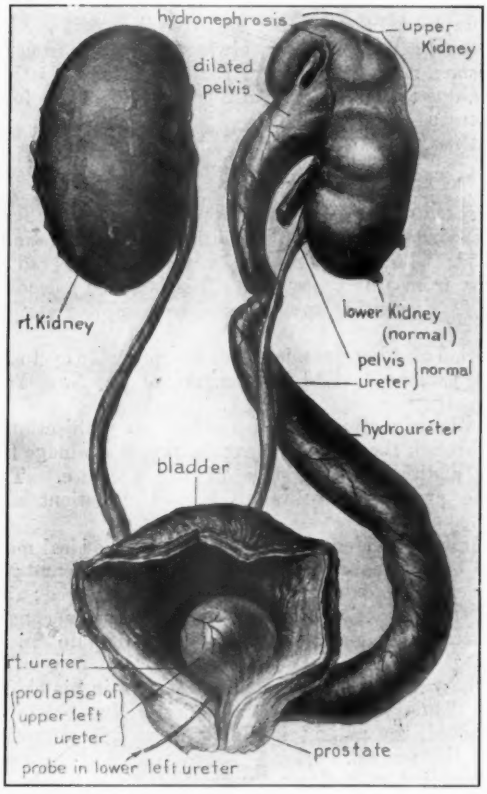


FIG. 1.—Enlargement of subcervical group of tubules with blockage of left ureteral orifice causing hydronephrosis, hydro-ureter and greatly diminished renal function.

consciousness he has the pain caused by a fresh incision. These factors, pain and increased hæmorrhage, explain why we had eight times as many cases of shock when we used general anæsthesia as we have now.

The induction of sacral and parasacral anæsthesia has been thoroughly described by us elsewhere. The method is easily and almost painlessly applied and has been used in many cases with practically no accidents. Its advantages are many. It is possible to get perfect anæsthesia for perineal prostatectomy in 95 per cent. of the

cases; the remaining 5 per cent. require some slight reinforcement, such as smelling a little essence of orange or a few drops of ether sprinkled on gauze.

There is only about one-tenth the amount of bleeding that occurs in the same operation under a general anæsthetic. The anæsthesia persists for six or eight hours after injection, to a certain degree, and for two hours for the purpose of operation. So that post-operative pain is largely obviated and thus the main factors in surgical shock are eliminated. Dehydration is prevented as water is imbibed throughout.

The operation which we prefer is a modification of Young's perineal operation. A Crowell's tractor is first passed into the bladder through the urethra, and its blades are opened. A semilunar incision is made through the skin and the two

FIG. 2.—The central tendon has been incised and the bulb drawn upward. The recto-urethralis reflection of the levator ani is then incised. The finger in the rectum acts as a guide and protects that viscus from injury.

spaces on each side of the central structures are opened by sharp and blunt dissection. The central tendon of the perineal muscles is then incised *behind* the point where the lateral perineal muscles join it. This allows the central structures to be pulled upward with the bulb and lateral perineal muscles. A Kocher clamp is fixed to the posterior part of the central tendon and held in the left hand for the purpose of traction. The index finger of the left hand is inserted into the rectum and the hand covered with a towel. The dissection is carried deeper and the important reflection of the levator ani called the recto-urethralis, attaching the rectum to the urethra at the apex of the prostate, is then incised. The levator ani is then dissected from the posterior surface of the prostate, which is recognized by the glistening fascia of

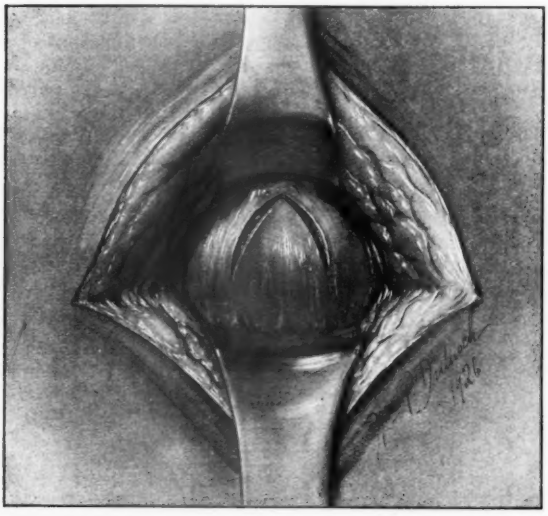


FIG. 3.—The bulb and transverse perineal muscles are drawn upward. The levator ani and the rectum are drawn posteriorly. An inverted V incision is made through the posterior lobe into the urethra.

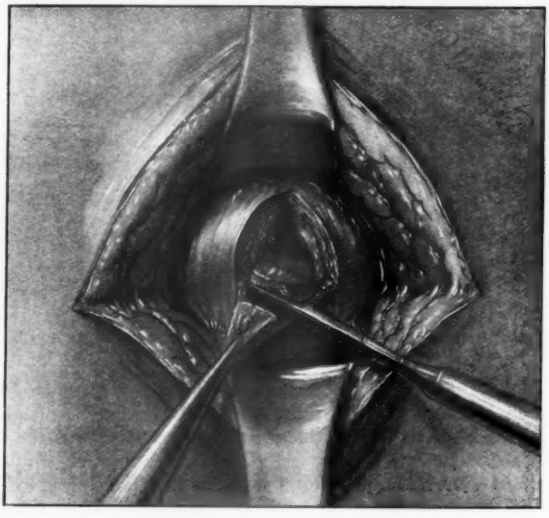


FIG. 4.—The flap containing the ejaculatory ducts is then drawn out. A transverse incision in front of these structures is carried down, separating the middle lobe from the posterior portion which contains the ejaculatory ducts. The entire prostate is then removed by enucleation.

Denonvillier which covers its posterior surface. It is thus noted that not only has the posterior urethra not been opened but often it is not even seen. This is an extremely important point in the prevention of incontinence. The left index finger is now removed from the rectum and the glove changed, a posterior retractor being placed in position.

The prostate is now opened by making an inverted V-slit in its posterior surface, the apex of the incision reaching the exact apex of the prostate and being carried down to the urethra itself. When this flap is drawn back the verumontanum is seen and a transverse incision is made in front of it. This is deepened by blunt dissection which separates the middle lobe from the posterior lobe with its dividing fibrous leaflet which carries the ejaculatory ducts through the prostate. This procedure leaves the ejaculatory ducts uninjured and still retaining their normal orifices. These facts account for the low incidence of epididymitis following our procedure.

The enucleation of the gland is now continued by separating the hypertrophied portion presenting from the capsule. The index finger is introduced into the prostatic

FIG. 5.—The author's lobe forceps are oval with double ridge grasping surface. There is a lock at the proximal end for holding the blades in a fixed position. The rounded oval shape of the distal end allows the forceps to be inserted into the vesical orifice without injuring it.

urethra and the so-called anterior commissure split, care being taken to injure the internal sphincter as little as possible. If the sphincter should be split at the top there is grave danger of tearing into the plexus of Santorini, with resulting hæmorrhage.

The Lowsley clamp is now used to grasp the hypertrophied structure and the enucleation is completed.

The vesical orifice is grasped with an Allis clamp, the Crowell tractor closed and removed and the finger of the operator introduced into the vesical orifice to identify and remove any remaining tags of tissue or a subcervical gland enlargement. Pieces of prostatic tissue as big as a lime are removed with the author's clamp through the vesicle orifice without difficulty. Particular attention is paid to the thorough removal of small nodules of prostatic tissue and to tags of fibrous material as well. These half destroyed tissues either remain as a future menace to the free passage of urine or become necrotic and offer a fertile field for the infection of the entire prostatic cavity.

A Pezer tube, size 26 F, is introduced into the bladder through the urethra and fixed in position by means of adhesive. The vesical orifice and prostatic cavity are now thoroughly packed with sufficient vaseline gauze to arrest any bleeding. The floor of the pelvis is closed by drawing the two sides of the levator ani muscle together with a catgut suture and the skin is closed with silkworm gut.

We have had only one death attributable to the anæsthetic. In the first series there were two patients who could not have been operated upon except by means of sacral anæsthesia as they were not in a condition to withstand inhalation anæsthesia of any sort. In the last series of nineteen patients there

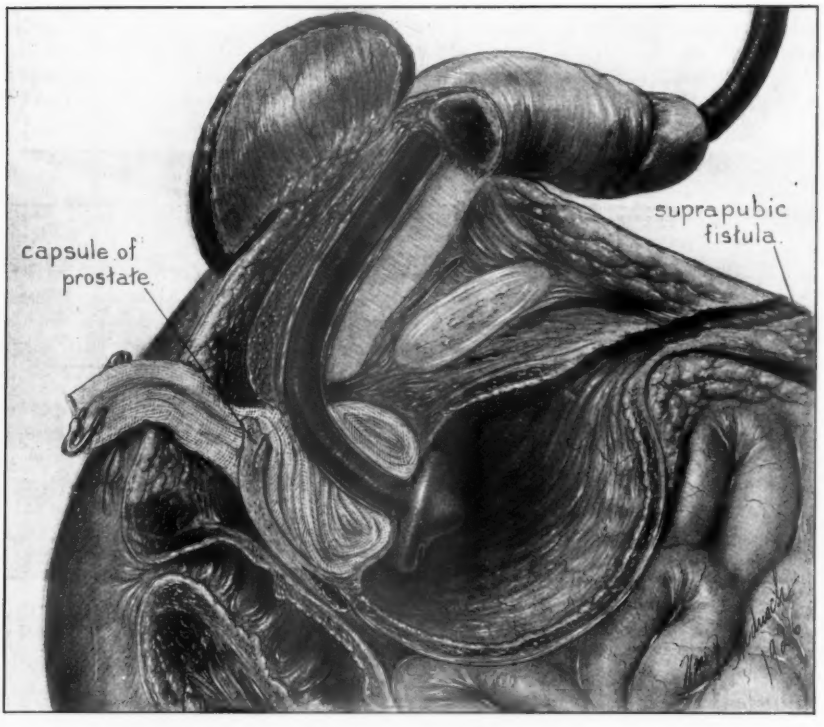


FIG. 6.—Cross section showing prostatic capsule packed with gauze and a Pezer catheter introduced into the bladder through the urethra.

is one such case. We have felt for some time that blood-pressure observations were much more important in the post-operative tests than any other feature of immediate recovery. It has been a surprise and delight to observe this series of cases. In none has the blood-pressure dropped below 100 mm. and in all there has been a tendency to rise at the danger period, about six hours after the operation. We have not found it necessary thus far to stimulate the circulatory system by giving drugs, or by intravenous infusion of gum-glucose, etc.

The general appearance of the patients on the day following operation is surprisingly good. The shock to the entire body mechanism is considerably reduced by avoiding inhalation anæsthesia. There have been no late developments such as

headache, paralysis or other alarming effects. Our patients so far have improved after the first post-operative period, from six to ten hours, without interruption.

Post-operative Care.—The prostatectomy patient must be handled and watched over as carefully as a baby. The patient operated upon under sacral anæsthesia may take fluids without becoming nauseated, not only immediately after operation, but during operation as well.

After operation he should rest quietly in bed, and in almost all cases he will do so, because he remains somewhat under the influence of the anæsthetic for several hours. He should only be disturbed in order to take fluids and to have his blood-pressure taken. The blood-pressure is the most important single test in such cases.

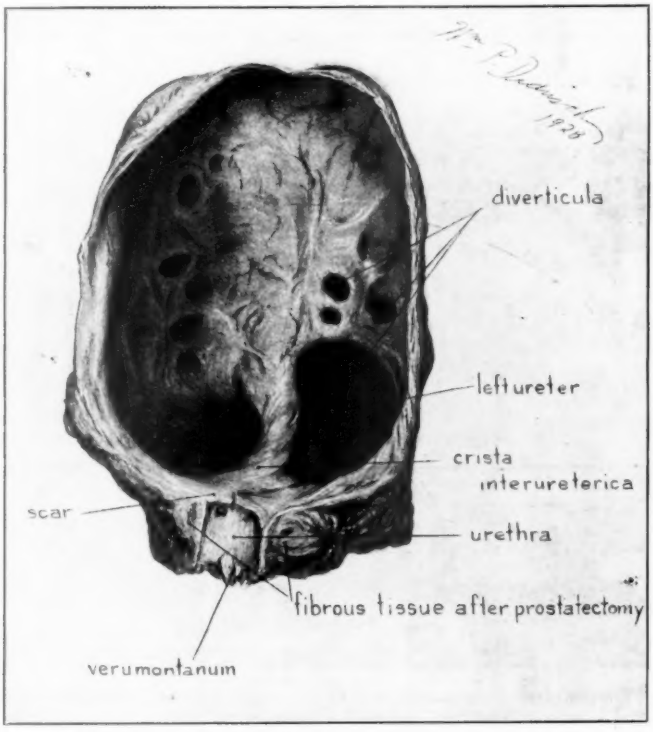


FIG. 7.—The prostatic bed, ten years after prostatectomy. (*Reproduced by kind permission of Professor Christeller, of Virchow's Krankenhaus, Berlin.*)

If the blood-pressure drops unduly and does not rise (premonitory symptoms of shock) gum-glucose solution may be administered after the method described by us in 1921. Transfusion of properly tested blood may be administered in extreme cases. If the bleeding has stopped, the packing is removed from the prostatic capsule after forty-eight hours, the tube is removed from the bladder on the third day, and the patient permitted to sit up in a chair the fourth day. Man is an upright animal and apparently all bodily organs function better when the patient is sitting in a chair than when he is in bed. The patient receives a remarkable psychological impetus when his feet touch the floor and one cannot help but be impressed by the fact that improvement is marked after he is allowed out of bed.

Upon reviewing the results of operations upon the prostate one is forcibly impressed by the fact that age is no contra-indication to operation whatever. In our series of cases there were twenty-six men over 80 years of age, all of whom, except one, lived for varying considerable lengths of time after prostatectomy, and died from some cause in no way connected with the operation.

Almost every patient who recovers from a perineal prostatectomy is able to return to his customary duties. Provided both sphincters are not lacerated beyond repair, there is never a resulting incontinence. Apparently either sphincter may be totally obliterated, provided the other remains intact, without incontinence. Persistent fistula never results if the perineum is reconstructed by one stitch drawing the two parts of the levator ani together. Frequently patients whose prostates have been removed are able to enjoy sexual intercourse. Occasionally the ejaculated mass goes back into the bladder instead of being projected from the urethra.

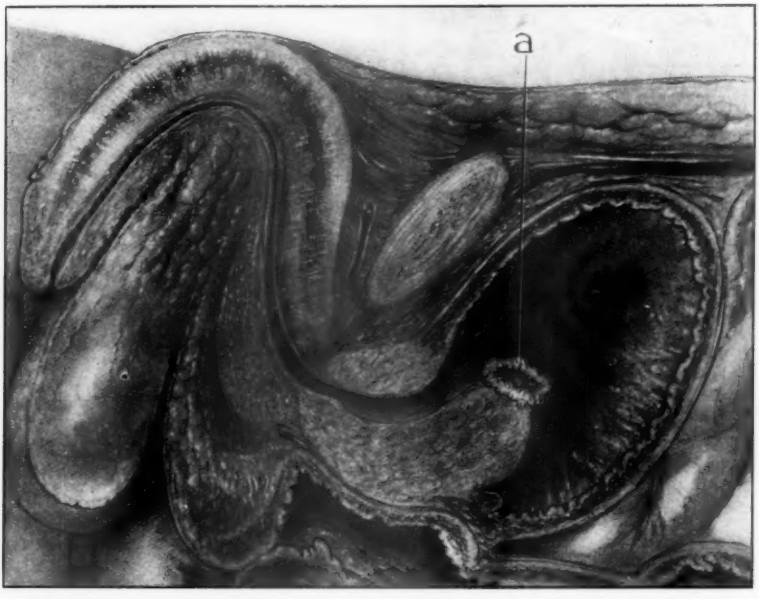


FIG. 8.—Prostate gland showing characteristics of Hodgkin's disease; (a) cauliflower growth following removal of piece for examination.

The bladder, usually infected before operation, almost always afterwards will clear up under appropriate treatment, provided the operation has been successful and there is no residual urine.

Carcinoma of the Prostate.—The accompanying chart shows that there were thirty-three cases of carcinomatous prostate operated upon with a mortality rate of 10 per cent.

It is our practice to remove carcinomatous prostates which cause residual urine, because even the most enthusiastic supporters of radium treatment admit that the application of radium does not relieve obstruction. Therefore we remove the prostate by the methods described above, and as soon as the patient recovers from the immediate effects of the operation, radium is administered to the prostatic

PROSTATIC OPERATIONS UNDER REGIONAL ANÆSTHESIA UP TO JANUARY 1, 1928.

JAMES BUCHANAN BRADY FOUNDATION FOR UROLOGY.

Diagnosis	No. of cases	Average age	Operation	Type of anæsthesia	Post-operative				Other complications	Average length of post-operative stay in hospital	Results			Mortality
					Good	Medium	Poor	Shock			Cures	Improved	Dead	
Adenomatous hypertrophy of prostate gland	297	64·47	Perineal prostatectomy	Sacral parasacral procain 1 per cent.	266	18	3	10	Hæmorrhage, 6	22·76 days	280	—	17	5·7 per cent.
Carcinoma of the prostate	33	67·36	Perineal prostatectomy	Sacral parasacral procain 1 per cent.	23	7	3	*Nil*	*Nil*	21·5 days	—	30	3	10·0 per cent.
Prostatic abscess	40	29·17	Perineal prostatotomy	Sacral parasacral procain 1 per cent.	40	*Nil*	*Nil*	*Nil*	Epididymitis. 2 cases Septicæmia	10·83 days	40	—	*Nil*	*Nil*
Tuberculosis of the prostate gland	2	61	Perineal prostatectomy	Sacral parasacral procain 1 per cent.	2	*Nil*	*Nil*	*Nil*	*Nil*	27·5 days	1	1	*Nil*	*Nil*
Hodgkin's disease of the prostate	1	32	Suprapubic prostatectomy	Sacral parasacral and suprapubic bloc	1	—	—	*Nil*	*Nil*	14 days	—	1	—	*Nil*

bed in the form of emanation seeds, or the element itself is applied. In our experience there is little liability of the growth returning at the site of the prostate. The patients usually live from 2½ to 3½ years, ultimately dying from metastasis.

This method of treating carcinoma is well worth while, as the patient does not die from uræmia and does not undergo the distressful urinary symptoms which he otherwise would surely have.

Prostatic Abscess.—Our series shows forty cases of prostatic abscess operated upon by the perineal route under regional anæsthesia. In a study made some years ago, when the author had the honour of serving on Dr. Keyes' staff at Bellevue, parallel series of cases were operated upon by the perineal and suprapubic route. It was found that the average stay in the hospital, after transvesical prostatic drainage for abscess, was thirty-one days, while, by the perineal route, the average post-operative stay was 16·9 days.

The average age of our patients was 29·17 years. There were two cases of epididymitis and one case of septicæmia, following operation. The latter case recovered after repeated blood transfusions. The average post-operative stay was 10·83 days. None of these patients died.

Conclusions.

(1) The most important item in the care of a case of prostatic disease is the maintenance of suitable drainage until the patient reaches his maximum of renal efficiency.

(2) The best kind of preliminary drainage is by means of suprapubic double tube suction, because such a procedure diminishes the œdema of the prostate and is thus a factor in reducing bleeding.

(3) Sacral and parasacral anæsthesia is the most suitable type for prostatectomy. Its advantages are: (*a*) it is efficient; (*b*) it allows the patient to take fluids up to, during, and immediately after operation; (*c*) the bleeding is much less than with any kind of general anæsthesia; (*d*) anæsthesia persists for several hours after operation, making the administration of morphine—with the resultant intestinal stasis—unnecessary in many instances.

(4) Perineal prostatectomy is preferred because there does not seem to be so much shock as in the suprapubic operation. The post-operative drainage is accomplished by means of a urethral catheter which allows both the perineal and suprapubic wounds to close.

(5) The average post-operative stay in the hospital has been reduced to 22·76 days.

(6) Almost 10 per cent. of the prostates operated upon were carcinomatous.

(7) The mortality for the adenomatous cases was 5·7 per cent., while there were three deaths out of thirty-three cases of carcinoma.

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Section of Comparative Medicine.

President—Major-General Sir JOHN MOORE, K.C.M.G., C.B., F.R.C.V.S.

[*October* 24, 1928.]

PRESIDENT'S ADDRESS.

The Old and the New of Veterinary Science and Practice.

By Major-General Sir JOHN MOORE, K.C.M.G., C.B., F.R.C.V.S.

ABSTRACT.—The paper recalls how matters veterinary were regarded forty-six years ago, what has been achieved since, and future progress is reflected.

The paper is divided into parts relating respectively to: (*a*) medicine; (*b*) surgery (*c*) teaching and research; (*d*) administration.

Formerly, glanders and farcy, and rabies, though acknowledged as contagious and specific, were also believed to be of spontaneous origin. Experiences with regard to these two diseases, and the mallein test for glanders, are related. The discovery of the *Bacillus anthracis* led to the development of veterinary research, but for some time confusion existed. Tuberculosis was believed to be endogenous and the result of the absorption of caseous products of a previous inflammation. Treatment of "milk fever" in cows by udder inflation and biochemistry in relation to that disease are considered.

The advance in veterinary surgery stands out most prominently; in canine practice, operations are now attempted which were never thought possible in the early days. Allusion is made to the recent formidable operations for the cure of "windsucking" in horses, and for traumatic pericarditis in bovines. The powers of observance of the old practitioners in diagnosing lameness, and some of the old methods of treatment for lameness, are supported.

The great progress in veterinary research is referred to, also its advantages from an imperial point of view. The causative agents of those diseases which are at present ultra-visible, particularly foot-and-mouth disease, will probably be found, and better methods of prevention result.

In training, thorough instruction in animal physiology, animal nutrition and biochemistry is advocated, also affiliation of veterinary colleges to universities, the individuality of such colleges, and the one-portal system of qualification being maintained.

HAVING begun my profession in 1882, I presume I may be classed amongst the old of veterinary science, *au fait* with the old ideas and the old order of things; but I have tried to keep in touch with the new. Though we older people have the glories of our past as a solace in our declining years we look to younger generations to show us new ideas and new discoveries and to set up new milestones in the path of science. "Tempora mutantur, nos et mutamur in illis."

For purposes of my address, it would perhaps be best to confine myself to the forty-six years of my own association with the profession: but, on reading some of our old books, one is attracted to the old conceptions of disease, and methods of treatment that at the present day are sources of amusement. The veterinary surgeon of eighty years ago, in the heyday of horses, was certainly unique. If his deductions as to the causation of diseases were wide of the mark, he lacked nothing in dexterity and in heroic treatment on occasions. It has become proverbial that we can bury our mistakes or convert them into by-products.

In speaking of matters old and new, I cannot in this address cover the whole field of veterinary science. I therefore pick out a few instances under the headings of (*a*) medicine, (*b*) surgery, (*c*) teaching and research, (*d*) administration.

(*A.*) *Medicine.*—Few of us can remember in detail what we were taught in our college days. If, therefore, we turn to our textbooks to refresh our memories, as I have done, in the preparation of this address, by reading in parts "The Principles and Practice of Veterinary Medicine" (1882 edition), by my old teacher, William Williams, we may receive rude shocks at the views expressed in those days as to causation of animal diseases. Yet Williams was a sound teacher, a great observer, a deep thinker, and an acknowledged authority considerably in advance of his time. The same may be said of our revered pioneers, George Fleming, William

Dick, Professor Gamgee, William Percival, Professor Coleman and others. Williams, for instance, in defining a contagious disease, says it is a morbid condition of the animal body, induced generally by the operation of a specific virus, termed a poison, derived from an animal similarly diseased, and communicated either by inoculation, contact, or through the medium of the air." He is quite right up to this point, but he continues: "Sometimes, however, contagious diseases originate spontaneously." He instances glanders and farcy, which he describes as "a malignant, contagious and fatal disease, due to the introduction into the animal economy, or of *generation within it* of an animal poison, such generation being the result of degradation of tissue, consequent on the ravages of another disease." He thus affirms the spontaneous origin of glanders and farcy, and further adds that the latter form, farcy, is more frequently developed spontaneously than glanders. Youatt goes so far as to say that the glanders form is hereditary.

Fleming asserts that rabies is capable of spontaneous origin; Dick held that rabies always originated spontaneously. Williams disagrees with Dick on the word "always," and holds that the disease can appear independently of contagion, but admits that its propagation is mostly due to this cause. The circumstances of its spontaneity, however, he confesses are hidden in mystery. I mention these two diseases, as their complete disappearance from Great Britain is the best evidence against spontaneity of origin. The separation of the definite causative organism, *B. mallei*, the discovery of its wonderful and indispensable diagnostic product, mallein, has sounded the knell of the terrible equine malady, glanders, and a fine mark of veterinary scientific progress has been made accordingly. Though matters in respect of rabies still remain hidden, present-day veterinary science and police exert a strangle-hold on it. As these two diseases have been eliminated from the field of the general practitioner in this country, a few words from one who has had general experience of them may be interesting.

During four years and a half assistantship in Manchester I had a unique experience of glanders in the cab and 'bus stables and in those of business firms. I think I can safely say I have seen every manifestation of the disease even to an articular or synovial affection of the menisci of the stifle joints and the colicky symptoms of splenic affection. I recall the grief of a cab proprietor who came to me in tears and begged me to clear out the disease from his premises, giving me permission to do anything I liked to them, which I did most thoroughly. It was then common to allude to the disease as acute and chronic glanders, acute and chronic farcy, cases of the latter at one time being allowed to work. Subsequently, "glanders" covered the whole, an appellation curious even in its pronunciation, but distinguished through its antiquity. I never could get away from glanders; in my first two years of service in India I had outbreaks in two batteries, two Indian cavalry regiments, and in ekka and gharry ponies of civil lines. The procedure in the Army in India in the early 'nineties was the destruction of affected animals, the removal of the unit from horse lines into picket outside cantonment, and constant inspection until four months after the last case. The unit was then declared free, and returned to troop lines. In one battery thirty-four horses out of one hundred and ten were destroyed, the disease having had a free run during my absence for six weeks on special duty. These were pre-mallein days. In the South African War (1899-1902), and on Remount Commissions in the United States, Canada, Austria and elsewhere, officers serving with the army experienced it, sometimes very badly, but were able to keep it in fairly satisfactory control with the use of the ordinary cervical method of mallein testing. During the late war, though we employed thousands of horses and mules, the disease gave little trouble, thanks to a most complete system of administrative and executive control, and the adoption of the intradermo-palpebral method of mallein testing, which could be applied and admit of the animal carrying out duty at the same time. The ability shown by the

veterinary profession in the control of animal diseases and in ministrations to sick and wounded during the war was a fine example of progress from the "old" to the "new" in veterinary science and practice.

In rabies, the veterinary surgeon was called in to destroy the rabid animal—a dangerous practice. My procedure, as a young man, was to present to the dog, over the half-door of the loose box, or from outside the enclosure, a pledget of cotton-wool soaked with Scheele's prussic acid on the end of a stick. The animal usually grabbed it and fell: the remainder of the vial of prussic acid could then be poured into the mouth, gloves, of course, being worn.

Brain diseases of animals occupied much of the attention of the old practitioners and writers, and their views on phrenitis, sleepy staggers, mad staggers and stomach staggers are difficult to reconcile. Treatment was of the heroic order. Here is a specimen from Youatt on phrenitis.

"Now is the surgeon's time, and his courage will be put to the test. He must open, if he can, one or both jugulars: but let him be on his guard, for the paroxysm will return with its former violence, and without the slightest warning. This is a case, and the only case, in which a ligature should be placed round the neck previously to the vein being opened; for this being done, however soon the paroxysm of violence may return, a full abstraction of blood may confidently be relied on."

Blood-letting has its devotees to this day, not perhaps to the extent of eight quarts, but in certain circumstances general or local bleeding is indicated as advantageous; but gone are the fleams, single and composite, and the mallet. I almost had said that gone also are the large and repeated doses of physic administered by our forefathers, but after hearing Major Davenport, R.A.V.C., describe at the National Veterinary Medical Association Congress at Torquay last year, administrations of explosive purgatives, I fancy there has been a return to the wisdom of the olden days.

The discovery by Davaine in 1863 of *B. anthracis* in the blood of animals suffering from splenic fever, the incubation of it outside the animal body by Koch, 1876, and the establishing of its pathogenicity by him, opened the way to brilliant discoveries which almost year by year have thrown more light on the cause and prevention of disease. The rods had been seen before 1863 by several scientists, including Davaine, in the blood of dead animals, and, designated as batonnets, were considered products of putrefaction, anthrax being considered a septicæmia or putrefaction of the blood. Williams is confused. He defines the disease as consisting in a special and primitive alteration in the blood in which an organism termed *B. anthracis* is rapidly developed and propagated; but adheres to an opinion that the disease is often produced by over-feeding, and he "is forced to the conclusion that the disease is due to a virus which renders the blood a fit habitat for this peculiar organism between the spores of which and the virus there seems to be an almost indestructible affinity. He is unconvinced that "quarter ill" differs from anthrax and "until further light is thrown upon the matter the disease will be described as anthrax with external manifestations."

Since then, however, research has given us information so precise that we can now make differential diagnoses in diseases formerly believed to be anthrax or symptomatic thereof.

Without doubt hæmorrhagic septicæmia and anthrax were greatly confused in India in days gone by; this leads me to a question to which I can get no definite reply—Is there any hæmorrhagic septicæmia in this country?

An expedient in preventive treatment of anthrax practised before the days of preventive sera and vaccines was to insert a seton in the dewlap. The old school was greatly impressed by its efficacy; the explanation given was that in inflammation, artificially produced or not, the coagulating properties of the blood are increased. In order to increase its action, dressing the seton with a blistering ointment was recommended. Contrast this with handling of anthrax and other diseases under vaccine therapy.

Previous to the discovery of *B. tuberculosis* by Koch in 1882, the views expressed on the nature and causation of tuberculosis sound extraordinary in view of present knowledge, means for its detection by tuberculin and the possibility for its elimination from our herds under scheduled Acts and Orders. Old pathologists considered tubercle to be a new lymphoid growth resulting from hyperplasia of pre-existent adenoid tissue. Tubercle cells or corpuscles were described; the largest were termed "giant cells" with numerous nuclei and a bright nucleolus. Virchow maintained that tubercles were composed of a retrograde metamorphosis of physiological or morbid tissue elements, that a local process of inflammation led to an exudation of a material which became organized to some extent, and then died or shrivelled, forming a new growth or tubercle. He called the process "tuberculosis" and the name remains to-day. Williams, writing in 1882 before Koch's discovery, remarks:—"Whilst maintaining my opinion that tubercle is endogenous and due to the absorption of the caseous product of a preceding inflammation," . . . "the experiments to prove its contagiousness and even infectiousness do not go beyond this, namely, the introduction of degenerated products of disease into the body of a healthy animal." He says of the tuberculous masses termed by butchers "angle berries" or "grapes," "If we can only train our mind to consider that these masses are mere growths, that in fact they are no more injurious to the quality of the flesh than warts or excrescences on the skin, the feeling of fear may be overcome. I am of opinion that these excrescences are mere results of a previous and perhaps distant inflammation (i.e., capillary bronchitis, pleuro-pneumonia contagiosa, or other inflammatory disease); that the products of such inflammation are thus localized whereby they are prevented from injuring the general economy; that, in fact, they are as much external to the general economy of the animal as so many excrescences in the skin or tumours in any part of the body; and that if they are carefully removed, and the membranes and structures in which they are found and from which they grow, carefully dissected out, the flesh is perfectly good, fit for any man's table; and that it is a pity to destroy valuable and nutritious food because the term tuberculosis has been applied to the tumours."

Needless to say, Williams shaped his opinions in the subsequent years in accordance with the progress of his science, and I believe that if veterinary science as it now stands were properly applied, and the veterinary profession given a free hand, bovine and avian tuberculosis could be eliminated, and a mitigation of the scourge in human beings effected.

Bovine pleuro-pneumonia contagiosa, of which older members of the profession have had considerable experience, does not now exist in this country. Its suppression stands to the credit of administrative procedure in which veterinarians can claim a considerable share.

A paper on progress in veterinary science and practice would not be complete without reference to "milk fever." Rather than go into the syndrome complexity of this peculiar disease in milking cows, and the reasons assigned for their display, I will refer my audience to the valuable articles by Hugh Begg in the *Veterinary Journal*, September, 1928, and Professor Dryerre and Professor Russell Greig in the *Veterinary Record*, September 8, 1928. We had great faith in the ice and cold water pack, and in liquor ammonia fortior as a diffusible stimulant in the practice of Mr. Richard Roberts, of Kendal, with whom I was a pupil in the early 'eighties. Contrast this with the later and more beneficent treatment by sedatives introduced by McConnell, of Wigtown, and the really dramatic results of udder inflation published in 1897, by J. Schmidt, of Kolding. The investigations of Professor Dryerre and Professor Russell Greig, the relationship of calcium deficiency in the etiology of the disease, the explanation of the value of mammary inflation in causing a sustained rise in blood-calcium, and, indeed, the whole evidence pointing to the adoption of calcium therapy in the prevention and cure of milk fever, is a great step forward in veterinary scientific progress.

In many other diseases our progress over the past years has been made manifest. I will merely mention a few: the diseases of sheep due to microbial anaerobes, the contagious abortions, canine distemper, fright disease, and others.

(*B*) *Surgery.*—On the practical side perhaps no greater advance in veterinary science has been made than in surgery. Surgical interference is attempted now, particularly in canine practice, which never entered into the routine of the old practitioner. The preface to surgery, whether human or animal, is anæsthesia, general and local. Pre-chloroform days were barbaric; the law now demands insensibility to pain during major operations. I always associate the late Richard Roberts, of Kendal (afterwards of Tunbridge Wells), as the father of the use of chloroform in our profession. In the early 'eighties he always performed castrations under chloroform, using an ordinary leather muzzle with towels round the top to prevent too great admixture of air, an important matter when dealing quickly with colts in an open field. The outcome of his procedure was a paper read at the Border Counties Veterinary Society on chloroform, from which evolved the Carlisle pattern of muzzle. Other inhalers have been invented, and for general anæsthesia we now have complete and practical means both for large and for small animals, not excluding the ostrich. Local anæsthesia from cocaine, eucaine, novocain, stovaine, hydrochloride of urea and quinine, and other synthetic compounds, is now a common procedure in veterinary practice.

Foremost in veterinary practice, at least in lameness of horses, is the ability to diagnose the seat of lameness and to prescribe rational treatment. The old practitioner was incomparable in detecting lameness. It was said of William Dick that he could tell at a glance even when a horse had moved but a few yards, the seat and the cause from which he was lame. Has the young graduate of the present day the same ability? I doubt it. Here we may concede the palm to the older practitioner. There were many others as brilliant as Dick, and whose certificates of soundness were greatly respected and requested. Their treatments of lameness, too, were, on the whole, resourceful and practical—remembering that the horse was a creature intended for utility—and the majority of the actions of our predecessors, so far as lameness is concerned, hold good to this day. Firing and blistering for sprained tendons and ligaments, if looked at as a permanent bandage, strengthening a part, still may find favour. In the treatment of splint I still have faith in an incision by knife, or rowelling scissors under the splint, separation of the skin and connective tissue over the splint, cutting through the periosteum into the osseous deposit with a sharp scalpel and keeping the wound open by means of a small length of tape, saturated with a mild counter-irritant, until pushed out in the healing process. Of course I presume local anæsthesia for the operation. For obstinate hip-joint lameness I would approve of an old fashioned remedy of two setons over the hip-joint as more successful in counter-irritation than mere blistering. It may sound barbaric, but it has proved efficacious.

Full advantage was taken of the fine field for the surgery of our officers in the large modern veterinary hospitals necessitated by the European war. I am sure that much of the dexterity of our veterinary surgeons of to-day was developed by their war service.

Veterinary surgery has recently excelled itself in an operation for curing wind sucking in horses. The operation is formidable, but results have been satisfactory. One of the animals operated on won a prize in a mounted display at an Aldershot Horse Show. Major C. H. S. Townsend, R.A.V.C., Major W. M. Mitchell, Royal (Dick) Veterinary College, Edinburgh, and others, are to be congratulated on their achievements. The Hobday operation for roaring, too, is a triumph of veterinary skill of recent years. Of operations on the bovine subject, I wish especially to bring to notice the very daring, yet eminently successful, operations for traumatic pericarditis, by Major Leslie Pugh, recorded a year ago. Few of us older

veterinarians would ever have dreamt that such an operation was successfully possible. Another operation in cows, but easy to accomplish, is the removal of a prolapsed uterus. I am inclined to believe that we preceded human surgery in hysterectomy.

Canine surgery during recent years has been brought to a high degree of perfection. Many of the operations in human practice have their counterpart in veterinary. To meet this advance, surgical appliances have been brought into line, and there now appears everything at hand, even to radiation, to render treatment worthy of scientific effort. Time will not permit me to remark on laparotomies, enterotomies for the removal of foreign bodies, hysterectomies, oöphorectomies, prolapses, tumours, amputations, and other operations.

(*C*) *Teaching and Research.*—These are days of research, and teaching must be in conformity to it. Veterinary research is too wide to deal with here, except in skeletal form. It is not yet full grown, but it is the mainspring of our scientific success. It was, and is, beyond the old school of veterinary thought, but it is rightly the dream of the present and rising generations. One thing is quite certain: we shall never combat and defeat disease until we see the face of our enemy, and determine the strength of his sinews. We have done much; with good backing we can do more. There are many animal diseases whose causal agent still remains in obscurity, and until such are defined preventive measures are haphazard and unscientific in application. One is sanguine, however, of ultimate discovery, and that in particular our old and elusive enemy—foot-and-mouth disease—will be arraigned before the Court of Veterinary Research carrying his own gibbet for his Tyburn. His arrest will confer a priceless boon on agriculturist and taxpayer alike.

It may be asked, When the causes of all diseases have been laid bare, what more will there be for the research worker? The reply to that is, There is no finality to research, and, in nature, insurgent diseases appear for us to attack, and other friendly police, or vitamins, anticipate acquaintance. The fascinating study of vitamins in recent years has made clear many factors in relation to health and sickness that were ill-understood in former days of medical and veterinary science. Animal nutrition, animal physiology, and biochemistry in maintenance of animal health and treatment of disease are provinces which veterinary science has barely entered as yet, and I am clear that such subjects come more within the purview of veterinarians than that of any other body of scientists. These subjects should be made a solid basis of training at our schools with the object that we and future generations should be the acknowledged authorities in all matters concerning the preservation of the health of animals, and the mitigation or suppression of their ailments. At the same time I agree with the first President of our Section (the late Sir Clifford Allbutt) that, in the elucidation of disease, team-work is necessary, and that the best results are likely to accrue from a trinity of veterinary, medical and plant pathologists.

(*D*) *Administration.*—Forty-six years ago when I entered the New Veterinary College, Edinburgh, the period of training for the Diploma of the Royal College of Veterinary Surgeons (M.R.C.V.S.) was three years, the Diploma of Fellowship (F.R.C.V.S.) being obtainable five years after. The training now is for four years, and the Diploma of Fellowship is obtainable two years after. The Royal College of Veterinary Surgeons also confers a diploma for Veterinary State Medicine (D.V.S.M.) chiefly taken up by those who are in, or desire, State appointments.

Veterinary colleges, though receiving grants for initial erection, and annual grants from the State towards maintenance, are virtually private institutions. There is a feeling within the profession, and one which I share, that affiliation of veterinary colleges to universities should replace the more or less indirect touch which exists at present in certain instances. Status would be improved and many aspects of training levelled up. Individuality, self-contained measures, and special

training need not be interfered with thereby, but veterinary colleges would have a far better chance of progress if carried along in the swim of university organization. We have lost ground by being a sort of Cinderella; the one-portal system of entry into the profession which is the safeguard of uniformity of qualification will not be broken by university affiliation.

The public support which is being extended to veterinary research, and the placing of it on an imperial basis, thereby enlarging the vision and the scope of the research worker, are great steps forward in the usefulness of veterinary science, and of public recognition. State veterinary service is gaining ground in public health administration; the hole-and-corner procedure is disappearing. It is we who must mend the position, and that we can do in every conceivable aspect of the special rôle we have elected to follow. "Vis unita fortior."

Discussion.—Mr. E. ALFRED WEST said he had the doubtful honour of being able to look back even further than the President, for he had begun his veterinary career fifty-two years ago. His recollection of "the dark ages" confirmed what the President had said. Diagnosis and treatment were largely empiric in pre-bacteriological research days. The speaker had practised in London right through the "glanders era," and he had condemned thousands of animals suffering from this disease. It was common in those times for animals with profuse discharges from both nostrils to be worked at night in the metropolis in buses and cabs. It was indeed a wonder that more human beings did not contract glanders, or at any rate were not recognized as having contracted it.

The sheet anchor of all treatment of lameness was blistering, firing and setoning, and unless one was drastically heroic in treatment, one's reputation suffered. The speaker said that in his pupilage days many men acquired great reputations by their attempts (often successful) to predict the day when the patient would die, and so much interest was evinced in that aspect of the case that the owner seemed to regard the loss of his animal as of slight importance compared with the accuracy of his veterinary surgeon's prophecy.

The enormous strides which had taken place since these early days, and the benefits thereby obtained both for animals and for human beings, could be properly appreciated only by those who had lived through the "old" times to the "new."

Dr. NATHAN RAW said that the study of comparative medicine had a most important bearing on public health and preventive medicine. The great problem of glanders, of such immense importance to an army in the field, had been splendidly handled in the European war by the veterinary surgeons. The importance of tuberculosis could not be over-estimated, and the question was, he regretted to say, still in an unsatisfactory position. The eradication of tuberculosis from dairy cattle was of supreme importance to the public health, as a great amount of the disease was directly conveyed to children from tuberculous milk. He would suggest a joint conference between the medical and veterinary professions, to consider the best means of dealing with the question of infected milk.

Mr. H. E. HAMILTON (Toronto) said that in Canada they were particularly interested in the diseases of poultry. Dr. Weaver, Poultry Pathologist to the Dominion, had stated that the losses in poultry in Canada were estimated at 10,000,000 dollars annually, and that *half of these losses could be prevented.* Probably the same state of affairs obtained in England to lesser or greater degree. He (the speaker) was sure that it would be beneficial to the profession and to the poultry industry if veterinary surgeons would take up this study and apply their peculiar knowledge to the eradication of such diseases as bacillary white diarrhœa, tuberculosis and coccidiosis in commercial flocks. He hoped that when future plans were elaborated for veterinary educational facilities, adequate provision would be made for the training in this important branch.

Section of Pathology.

President—Professor A. E. Boycott.

PRESIDENT'S ADDRESS.

The Transition from Live to Dead: the Nature of Filtrable Viruses.

By A. E. Boycott, D.M.

Rutherford was an example of the danger and folly of cultivating thoughts and reading books to which he was not equal. It is all very well that remarkable persons should occupy themselves with exalted subjects which are out of the ordinary road, but we who are not remarkable make a very great mistake if we have anything to do with them.

—W. Hale White, preface to the second edition of *The Autobiography of Mark Rutherford.*

Pathologists are such practical people that I feel that I am straining the privilege of a presidential address about as far as it will go in attempting to discuss such a topic as the relation between things which we call alive and things which we call dead. But, though we seldom have opportunities of talking about them, we all have our speculative moments when we wonder about things in general and try to put together some sort of lay figure on which we can hang the facts which interest us and see how they fit, and I should like to take this chance of getting rid of some of my own imaginings and sketching the Jemima on which they seem to look fairly presentable. And I do this in a gathering of pathologists because a good deal of light is thrown on the whole question of "live" and "dead" by the "filtrable viruses," "agents," "bacteriophages," and what not, in which we have been so much interested in recent years.

I do not propose to enter at length on the old controversy between vitalism and mechanism. Pathologists might with advantage have taken a greater share in it than they have, for it would take a hardened mechanician to maintain his faith in face of our daily experience of repair adaptation and all the other purposive compensations for injury of which the body is so abundantly capable. Unfortunately our facts have not been widely known to those who have felt inclined to discuss the question. As far as I can see, the attempt to "explain life by chemistry and physics" has completely failed. It was thought at one time that if only the microscope could be made to magnify enough, we should see life going on; the present contempt for histology is I suppose a sort of revenge on the wretched limitations of the instrument. Hope was then transferred to biochemistry, which has done just what the microscope did—it has helped us enormously to understand the mechanisms of live things and not at all to explain life; let us hope that it will not sink to the same degraded position. But if vitalism has had the best of the argument, it has not led to a very profitable or a very satisfactory position. Vitalism is often mysticism, and (which is why mechanism has been so popular) any dualistic interpretation of the world is always repugnant to natural human instincts. But it is possible to escape dualism in another way, and I suggest that the vitalistic controversy in anything like the form it has taken during the last forty years is out of date, that instead of emphasizing the differences between live and dead things we should make as much as we can of their similarities, and that instead of dividing the world into two distinct categories we should regard it as being made up of one series of units with properties which differ more in degree than in kind. This is not the mechanistic view, for we come to it, not by explaining live things by dead things, but by realizing that the characteristics of live organisms appear also in dead matter. While we have been waiting for life to be explained in terms of chemistry and physics, a good deal has been done towards stating chemistry and physics in terms of life. Of course, no "explanation" of either live or dead has

been given; the behaviour of an atom is just as mysterious as the behaviour of a wasp, and neither "explains" the other any more than a trypanosome explains a whale. But it is something of a comfort if we can believe that at bottom they both behave in much the same way; we can have one lay figure instead of two, and if its coat and trousers are not made of exactly the same stuff we may find them in reasonable harmony with one another.

Picking up such rumours as he might of what is going on in other lines than his own, every biologist must have been struck by the curious familiarity of several of the conceptions which in this century have gone to start the revolution in atomic physics which has pulled the universe in pieces and has perhaps not yet quite succeeded in putting it together again. The ideas are familiar because they were originally biological—derived from the study of live things and applied to their explanation. Let me illustrate what I mean by some examples.

(*a*) It is one of the characteristics of life that it is exhibited by discrete units which we know as organisms. As Powell White says, there is no such thing as living matter, there are only live organisms, and in so far as they are alive 0·1 cow or 1·35 cabbage are impossibilities. The enterprising surgeon could, of course, easily make something which was structurally about three-quarters of a cow and, I daresay, even less, but what was left after he had done with it would be either a cow or not a cow—its essential cowness cannot be other than integral. The live world is made up of such discontinuous pieces: so, we now learn, is the dead world. The notion that all matter is particulate is of immemorial antiquity, and as we go further in its ultimate analysis we come always to particles of ever-decreasing size: fractional atomic weights are as impossible as fractional animals: the quantum theory tells us that energy is also parcelled out in bits; light consists of particles and, though the ether dies hard, the belief that there is anywhere a continuum—something without a grained structure—has been almost entirely abandoned. Discontinuities—in the structure of atoms and in the sizes of the stars—are now as characteristic of the dead world as of the live.

(*b*) When Rutherford and Soddy made people believe that one element really could be derived from another, they did for dead things what Darwin had done for live things: indeed they did rather more, for they backed their proposal with experimental proof which neither Darwin nor anyone else had produced in the biological sphere. In neither instance was the idea wholly new; suggestions of various kinds had adumbrated the change. "Evolution" was originally used in reference to the cosmos, but it was from zoology and botany that it spread through the descriptions of all human experience before it was applied to what had been supposed to be the ultimate verities of matter. And now, neglecting the time factor, chemical elements are not necessarily more stable than zoological species. For practical purposes lead is lead and a dog is a dog, but now we have to apply to both the reservation that they have not always been so, and cannot be trusted to be so indefinitely in the future.

The disintegration of the radio-active elements takes place automatically: it cannot be started, stopped, controlled or modified: its progress is simply a question of the lapse of time. The modes by which organic evolution has been supposed to take place are beyond our discussion, but it is not impossible that it follows the same plan. Osborn and other experts hold that the course of any evolutionary sequence of animals is predetermined from the beginning: this "orthogenesis" may be interfered with by circumstances and opportunities, for live organisms are obviously liable to meet conditions in this world which they cannot resist, and which may deflect them from a predestined track or bring them to an end altogether: dead elements meet their difficulties elsewhere in the universe.

(*c*) The classification of the elements which have developed by this evolutionary process recalls the familiar schemes of botanists and zoologists which show at once the affinities of animals and plants to one another and (though here there is of course a certain amount of guess work) their phylogenetic relationships. Animals were

originally classified by characters which we now believe to be largely immaterial—size, shape, habitat and any other obvious features: Mr. Gladstone thought whales were fishes and bats birds, and plenty of people still suspect a slow-worm of being a snake. About 150 years ago comparative anatomy began to get them into more natural groups, and evolution added the criterion of descent in determining the system which prevails at present. Much the same has happened in classifying the elements into something better than a series of arbitrary pigeon-holes. Their discovery was the first step, much more difficult than the apprehension of animal species. The progress of chemistry then showed that they fell into groups akin to vital genera or families or phyla (we cannot guess at what level the analogy is closest), and the discovery of inorganic evolution and isotopes has brought their relationships to a suggestively biological position. Atomic weights are no longer of any great importance: what matters in classifying an element is its atomic number which determines its position in the periodic table and is a summary of its comparative anatomy and a clue to its history. An element (e.g. lead) may arise by more than one line of descent, which is what a biologist would call "evolution by convergence." The isotopes into which Aston has dissected many of the elements correspond to the groups of closely-allied species which embarrass the systematist and with which bacteriologists are familiar enough. Perhaps if they had sugar reactions or could be agglutinated, or indeed had a few more perceptible characters of any sort they might be easier to distinguish.

(*d*) If a man and a bicycle are smashed up together in a common catastrophe, the man mends himself, the bicycle does not. This capacity of self-repair is one of the greatest characteristics of live organisms: indeed, if one wishes to define shortly the subject matter of pathology I doubt if one can do it better than by saying that it is the study of how organisms resist and repair injury. They repair themselves in two ways. In the larger, more complicated animals we find very highly developed a capacity for individual repair which we see daily in the post-mortem room and experience continually in our own persons: it is so common that we are not impressed by it as much as we should be. Simpler things such as bacteria have little of this power of personal repair; indeed, I doubt whether a unicellular organism under natural conditions can effectively repair and recover from a substantial injury any more than can the individual cells of higher animals. But they achieve the same ends by other means, and owing to their numerical abundance and their high capacity for reproduction they can allow the injured individual to perish and readily replace him with a new one. Individually or racially, therefore, organisms repair themselves. Atoms seem to be able to do the same. All gross matter is made up of atoms, each of which has a definite structure according to its species: as nucleus there so many hydrogen atoms with their attendant electrons and outside are so many planetary electrons. Electrons are continually being detached from atoms by various means, e.g. whenever electrical energy is manifested. Presumably an atom of e.g. iron which has lost an electron is no longer of its normal nature and substance, i.e., it has ceased to be perfect iron, and such a process would in the end lead to the iron becoming manifestly something which was not iron unless some restorative process was at work. It seems clear that injured atoms must be able to pick up electrons from somewhere to replace those which have been lost, a method of individual repair which appears to be efficient enough.

(*e*) Another of the great characteristics of live things is their variability. Any measurable quantity of any organism varies, and the values are distributed in some mode akin to the normal curve. Crookes suggested long ago that atoms vary in a similar way, Karl Pearson has imagined a world where contingency replaces cause and effect, and Donnan has emphasized that our chemical and physical constants are statistical, derived from the measurement of an infinite number of individuals, and

summarizing, perhaps, the average values of a variable population : if biological measurements were made on the same scale, zoology and botany and even pathology might be "exact sciences" too. When we say that the atomic weight of one of the chlorines is 35, or that the mass of the hydrogen atom is $1{\cdot}650 \times 10^{-24}$ grm., it may tell us no more about the individual atoms than a statement that the height of the Members of this Section of the Royal Society of Medicine was 5 ft. 8 in. would give us a view of the range of sizes which we represent. Whether atoms and molecules vary like organisms, therefore, we do not know—nor is it easy to imagine how we could find out. The possibilities of variation evidently become greater as structure becomes more complex—as we go, that is, from electrons to elaborate chemical compounds.

(*f*) Cane-sugar boiled with dilute hydrochloric acid is progressively hydrolysed till practically none of it is left. Analysis of the course of the reaction shows that (say) one-fifth of the original quantity is decomposed in the first five minutes, one-fifth of what remains in the next five minutes, one-fifth of what remains in the next five minutes, and so on until the amount left is inappreciable. This strange behaviour is accounted for by assuming that the molecules of cane-sugar go through some sort of regular rhythmical change, so that at any moment only a certain proportion of them are susceptible to the action of the water at the instigation of the acid : there is, I believe, no other justification for the assumption than that it fits the facts, and it cannot fail to remind us of the rhythmical alternations of rest and activity which are common, perhaps universal, in live organisms. If, as Chick has shown, bacteria sometimes succumb to heat or disinfectants on the same kind of plan, it is legitimate to say that they behave like the molecules of cane-sugar. But it is equally correct to say that the molecules of cane-sugar behave like bacteria. We cannot tell which is imitating the other : all we see is that the behaviour of both is similar. The conduct of the bacilli could hardly have been predicted from a knowledge of what happened to the cane-sugar. The natural supposition would have been that the molecules of which each bacillus was made up would have been destroyed logarithmically, so that the death point of all the bacilli would have been reached simultaneously—a reflection which illustrates particularly clearly the considerable truth that the discrete unit which is comparable with the molecule of cane-sugar is the whole bacillus and not one of its constituent molecules.

Now, I do not want to push these analogies between atoms and organisms too far, nor indeed to claim more than that they are suggestive to an imagination which is not afraid to have its wilder moments. Atoms are very much smaller, and necessarily of much simpler structure and functions, and one would no more expect to find in them all the qualities of organisms fully developed than one would look for all that goes to make a human being in the tubercle bacillus. However, it is only because we are used to it that we accept, without emotion, the idea that an amœba is analogous to an elephant : it must have been an amazing notion when it was new.

There are two general objections which will probably occur at once to most biologists : (1) that dead elements do not show the multiplying reproduction characteristic of organisms ; (2) that organic evolution on the whole progresses from the simple towards the complex, whereas what I have called the evolution of the elements proceeds uniformly in the opposite direction. The two difficulties are rather closely related.

Organic reproduction does two things : it produces a fresh version of the old organism and it gives an opportunity for numerical increase : its final effect is to leave organisms very much where they were. Each foxglove plant in my garden goes to immense trouble to produce about 500,000 seeds, and the wasps toil earnestly all the summer to increase from one to about 1,000. But next year there will be just about as many wasps' nests as this and just about as many

self-sown foxglove plants. Darwin taught us the qualitative importance of this superabundance, but, quantitatively, it is made use of only if conditions alter: it then enables organisms to fill up any gap in the environment. If my wife interferes with the natural competition among the young foxgloves we may have more or less than last year; the vacant spaces in Bloomsbury have given us more willow herb than we had before the houses were pulled down, and when some philanthropist enables the University to put up Dr. Maxwell Garnett's skyscraper we shall have less; we make a gap for bacilli in our culture tubes and they multiply as they never did outside. Man alters his own circumstances. These catastrophic alterations in numbers are flaring examples which attract attention. Slower sæcular changes in environment have the same effect, some sorts increase, others diminish, and on the whole there may be a tendency for a few large organisms to be replaced by many small ones. But, taking the facts as a whole, the capacity for reproduction does not result in more organisms than there were before: it merely enables them to adapt themselves to varying conditions. If organisms were less complicated, more stable and enduring, less easily injured, and if natural selection turned out to be a fact of experience without perceptible significance, the reproduction of organisms in general might be reduced to the level at which it runs in men in England to-day—numbers are just maintained. And if they lived longer it might be a still less important feature of their activities: an elephant does not bother about reproduction till it is 40 years old or thereabouts, a bacillus does it at an age of about 25 minutes. It will, however, need a vast increase in longevity if any approximation is to be made to the position in the dead world as we see it on this earth. It is indeed possible that there is here a real qualitative distinction between live and dead, but it seems more likely that the difference is one of mechanism rather than result, and, as we learn from biology, it is results rather than mechanisms which are important. With increasing complexity we get diminishing stability, which is presumably why there is no known element heavier or with a more elaborate structure than uranium. Units which are more complex cannot maintain themselves without the periodical remaking which we call reproduction: those which are less complex do not reproduce, because they have no need to do so.

There is no reason to suppose that anything so like organisms as to deserve the same name exists anywhere in the universe except on the earth; as far as live things are concerned there is no need to look further. But we cannot confine our speculations about dead things within the same limits. The stars are made of much the same elements as the earth, and material transfers take place in both directions: meteorites come and nearly all the hydrogen and methane which arises from the decomposition of cellulose by bacteria and *Streptothrix* flies off to celestial bodies which are dense enough to secure its permanent adherence. The relevant habitat of the elements is therefore the universe and, taking this into consideration, it is not altogether clear that something like reproduction does not go on in dead things.

Though the elements seem inert and stable enough here and nothing much happens to them except the slow decomposition of those which are, in our environment, radio-active, in the immense heat of the stars atoms not only come to pieces and are dissociated into protons and electrons, but their basic structure is destroyed, positive and negative electrons fall into one another and matter is converted into radiation. In the heavens the elements disintegrate more completely than a dead cat does on earth, and unless there is somewhere some reconstruction the cosmos is coming to a material end. Lodge and Millikan think that in the depths of interstellar space, under conditions of intense cold, energy may once again become matter, radiation be reconverted into electrons which in their turn are recombined again into atoms, and so the various elements are reproduced; Jeans doubts any such regeneration. The duty of a pathologist does not call upon him to interpose

his private judgment in so nice and important a controversy, and it would be impudent to say more than that some such process would enable us to have a comfortable faith in the maintenance of the material universe. If reconstitution is shown to take place, one cannot help thinking of the nitrogen cycle, and how it was once held as certain (I was taught it as a student) that combined nitrogen was continually and irretrievably leaving the live world which must therefore inevitably come to an end; we had not appreciated nitrifying bacteria and attached more importance to academic argument than to Moses' directions for fallowing arable land.

If the elements do go through such a cycle, it is possible that what we call their "evolution" is more analogous to the death and reproduction of organisms than to the progressive appearance of more complex forms. Very little of the cycle takes place in our own particular corner of the universe, to which the organismal cycle is limited, and it is conditioned by very different circumstances of time and space, but it has much the same result in that it leaves things where they were. Protozoa are the better for reconstitution without multiplication: perhaps atoms are too. On the other hand, the reconstituted atoms may easily be of different species from those out of whose debris they have been built up, and under conditions where any reconstitution can occur it is possible that atoms are made which are more complex than any of which we have direct knowledge. Perhaps, too, the inorganic cycle is more nearly parallel to the appearance, progressive evolution and final disappearance of a group of animal forms which some writers have imagined to be the birth, growth and death of an organism drawn out on an extended scale. I do not know.

Such are some of the ideas familiar in biology which have appeared in the explanations of our experience of what is not alive. As I have stated them, they are to some extent inconsistent with one another and they lead to no certain conclusion; they furnish, however, an assemblage of concurring and converging probabilities which encourage one to think it possible that things which are alive and things which are not alive constitute in effect one series, beginning with hydrogen atoms and reaching up to man, and perhaps on to angels, not arranged in a continuous linear succession but on a scheme resembling the phylogenetic line of the animal kingdom. The units (or "wholes" as Smuts would call them) which make up the series are of progressively increasing complexity, structural and functional, and must be compared against one another as they stand, irrespective of their composition. A hydrogen atom, a molecule of albumin, a bacillus, a dog are comparable as such, and it is not necessarily of any moment that hydrogen is the basic stuff of all matter, that proteids are essential constituents of all live organisms, or that a mammal is made up of many bits, each of which is more or less like a unicellular organism; in no case is the behaviour of the more complex whole simply the sum of the behaviour of its constituents. Snch a view satisfies our natural antipathy to a dualistic explanation of the universe and makes the old controversy about vitalism and mechanism largely unnecessary. It tells us nothing about the nature of life; by indicating that organisms are analogous to elements, it encourages us to think of life as being as insoluble as gravitation, give up the attempt to make out what it is and, as Lovatt Evans recommends, spend our time more fruitfully in studying its phenomena. If you like to be paradoxical, you can say that live things are dead, or if you prefer it, that dead things are alive. Both at bottom have much the same characters, and it is unlikely that any sharp distinction between them can be drawn.

We pose to ourselves the question: Is the bacteriophage (or Gye's cancer agent, or the virus of plant mosaic, or any other "virus") alive or dead? in the belief that we are asking a crucial question to which there is a definite obtainable answer which would solve our troubles. In doing so we put up one of those false antitheses which so often lead us astray. The difficulty in most scientific work lies in framing the questions rather than in finding the answers, and by the time we are

in a position to know what the crucial question really is we have generally pretty well got the answer. In this case "live or dead" is a stupid question because it does not exhaust the possibilities. Our general notion of the structure of the universe leads us to expect that we shall meet with things that are not so live as a sunflower and not so dead as a brick, and a consideration of what we know about "filtrable viruses" and similar "agents" brings us to the conclusion that they represent part of this intermediate group. Let us see how far they conform with what are, in ordinary language, admittably "live" and "dead."

Size.—Essentially they are very small though just how small it is impossible to say. They must be ultimately particulate because all matter is so arranged, and from the readiness with which they are adsorbed on to appropriate surfaces the particles are presumably much larger than the molecules of simple salts. Passage through filters with pores of different sizes turns out to be a complicated and dubious method of measurement, and the effects of centrifugalization may depend more on the specific gravity than the size of the particles. They are invisible, and ultramicroscopy shows nothing in the infective blood of polyhedral caterpillar disease, at least down to 50 $\mu\mu$ (and probably down to 15 $\mu\mu$), which qualitatively and quantitatively cannot be seen in normal blood. Levaditi says (in error according to Bedson) that herpes virus goes through membranes which hold back complement and tetanus toxin; it is possible to concentrate solutions of hæmoglobin in the centrifuge. Taking one thing with another and reckoning that some viruses are doubtless larger than others, an average diameter of about 25 $\mu\mu$ (0·025 μ) seems a reasonable assumption, about $\frac{1}{10}$ the diameter of the smallest bacillus, about the same size as the colloidal aggregates of dissolved hæmoglobin and with room for 200 to 400 proteid molecules.

Now it is characteristic of all groups of animals and plants that they have upper and lower limits of size.[1] There is no mammal, fish, mollusc or insect which is not perceptible bare eye any more than there is any bacillus which can be seen without a magnifying glass. It is also in a general way true that there is nothing which has the properties which we commonly associate with bacteria which is not at some stage in its life visible with the highest powers of the ordinary microscope or *en masse* in culture, though of course, if rules of this kind were too absolute they would imply a more anthropomorphic world than most people nowadays are prepared to flatter themselves with.

Frank bacteria and protozoa may have minute phases: Leishman showed long ago that the spirochæte of African relapsing fever might in the tick be invisible and filtrable and we cannot reject the evidence that even the tubercle bacillus may exist in a similar state. But no definite bacillus is known which is much smaller than *pneumosintes*, and it seems likely that at a diameter of something like 0·25 μ (250 $\mu\mu$), i.e., somewhere about the limit of direct microscopic vision, there is a break in the series which runs continuously downwards from the largest bacilli (which would be visible naked eye if they were 20 times as big as they are) just as the series of mammals stops at a weight of about 5 grammes and the series of beetles at a length of about 0·5 mm. The largest *Bacillus megaterium* is some 25,000 times the bulk of *Dialister pneumosintes*, which is a relative difference of the same order as that between a pigmy shrew and a big man or between a laboratory guinea-pig and a large elephant. *D. pneumosintes* is about 400 times the bulk of what we imagine to be an average virus, and if there are no large viruses (the organism of cattle pleuro-pneumonia being probably bacillary) there may be more than seems at first sight in our definition of the agents we are discussing, by the facts that they cannot be seen and that they will pass through filters with very small holes—a system of classification which has often been laughed at, though it could be applied well enough to many animal groups.

1 See the table in *Animal Biology*, by J. B. S. Haldane and J. Huxley, 1927, 276, and *Contributions to Medical and Biological Research (Osler Memorial)*, 1919, i, 226.

Composition.—A diameter of 0·025 μ does not give much room or many facilities for complicated vital actions. We do not know what occupies that tiny bulk; we do not even know that viruses are mainly proteid. There would be room for a larger number of simpler molecules though it is doubtful whether in any simulacrum of life this would compensate for the absence of the unique combination of chemical flexibility and physical stability which proteids possess and without which, as far as we know, "life" does not exist. The antigenic quality of viruses is our only evidence that they contain proteid. Clinically and experimentally they confer a resistance to reinfection which is, in comparison with antibacterial immunities, singularly intense and durable, and is associated with antiviral properties in the blood-serum. As against this we have: (1) that antiviral serum contains only a simple neutralizing principle like an antitoxin (and possibly not actually effective *in vitro*) and has no specific agglutinin precipitin or (this is very doubtful) complement-fixing immune body; (2) that it is doubtful (though hardly, I think, more than that at present) whether it is still true to say that all antigens are proteid in nature; (3) that substances like diphtheria toxin and the substance which Murphy has separated from the Rous sarcoma seem to be proteids of rather a special and simple kind. Another point which may be germane is that the dose of virus used for infection makes much less difference in the result than it often does with bacteria. The infective units are evidently present in enormous numbers in e.g. the vesicle fluid in foot-and-mouth disease which may be diluted 1 in 10 million and still carry on infection. There is a minimum infecting dose, which shows that infection is due to something definite and not to magic, but once this is passed the rate at which the resultant illness develops and the degree to which it reaches are not much affected by giving 1,000 or 10,000 times as much. The big doses of bacteria which are often administered to animals contain bacterial substance by whole milligrammes by which the symptoms and course of the infection may be greatly influenced. The absence of such poisoning effects with large doses of virus may, of course, be due to the small quantity of virus substance which is given, but it quite possibly follows from its quality. There is indeed no evidence that viruses contain or produce poisonous substances as do so many bacteria.

We cannot therefore affirm that viruses differ radically in composition from e.g. the typhoid bacillus—nor that they do not. We probably have no business to make an assumption either way.

Metabolism.—The attempts which have been made to demonstrate the production of carbon dioxide by viruses have failed, but the quantities involved are small and the technical difficulties large, so that we cannot regard the evidence as conclusive. It seems, however, that if they have any respiratory exchange it must be at a much slower rate per infective dose than that of ordinary bacteria.

Stability and resistance to harmful agents.—Some viruses at any rate can retain their activity *in vitro* for several years. Some bacteriophages endure for a long time in bacteria-free filtrates; the Rous tumour virus can be kept almost indefinitely in dried tumour tissue. Others are more labile and are difficult to keep over a period of days. There is much the same variability as there is with bacteria and bacterial toxins: viruses as a class are not characteristically unstable, evanescent things.

A good deal has been made from time to time of their resistance to heat and protoplasmic poisons. Here again the results are very various and differ with the sort of virus and the conditions of experiment; there are no general rules. But there are a remarkable number of instances of viruses which have resisted temperatures up to 75° C., and treatment with chloroform, alcohol, ether, toluol, phenol, acids, alkalies, and so forth. Formalin destroys many of them quickly, which is curious, for its action in coagulating proteids is much slower than that of alcohol, which they often resist. As a whole they are certainly more resistant than vegetative bacteria,

but it is not certain that they differ markedly from bacterial spores. In several particulars this resistance recalls that of enzymes, and their peculiarities may be another reason for suspecting that they are not made of quite ordinary proteids. Their is nothing in their size *per se* which should protect them.

Capacity for independent life and multiplication.—There is no convincing evidence that any virus has grown and multiplied in artificial culture, though successes have been reported, and the observations of the Maitlands on vaccinia are difficult to explain away: they would have been more impressive if animal cells could have been kept out of the medium altogether. Living cells are in all cases necessary, which may be supplied by living bacteria, living animals or plants or tissue cultures. That they really do multiply under these conditions seems beyond question: indefinite serial passage of an infective virus (e.g. foot-and-mouth disease) through experimental animals (e.g. guinea-pigs), indefinite subculture of the bacteriophage, quantitative tissue culture of the Rous agent—indeed all the evidence we have is conclusive on that point. Viruses are certainly not enzymes. Apart from living cells they may for a long time survive, i.e., remain in such a state that, on altering the conditions, they can give rise to their characteristic effect—vaccinia, a sarcoma, bacteriolysis, etc. But there is no evidence that they multiply under these conditions, and multiplication at the expense of the environment is probably regarded by most of us as the most important criterion of life.

For their multiplication, young growing cells are especially suitable, and it may be quite necessary. The bacteriophage multiplies only with the multiplication of the associated bacteria, and vaccinia, herpes, Rous sarcoma, etc., develop and multiply especially in connexion with the growth of cells which results from local injury. Cell injury and cell growth are so intimately related that I know of no case where cell growth can certainly be excluded, but at present we cannot be quite certain that it is necessary. It seems also to be true that viruses multiply only in the course of the production of their specific effect.

But though the fact of multiplication is plain, it is by no means proved that it is effected in the way which is familiar in bacteria and living organisms generally. We put in so much virus and we get out more: we have no evidence, nor, I think, the right to assume, that the particles which we get out are the direct descendants of those we put in.

It may be that these facts are best explained by supposing that viruses are obligatory intracellular parasites, and that the difficulty of cultivating them on artificial media will be solved when we can imitate sufficiently closely the essential features of the intracellular environment: pathogenic protozoa were not cultivated at the first trial. Very few bacteria live inside animal cells, and it is perhaps significant that those that do (e.g. *Brucella abortus* and *Bacterium tularense*) are among the smallest of the group. Viruses have, of course, not been seen inside cells, but their dependence on living cells, and the considerable regularity with which their presence is indicated by cytoplasmic and intranuclear "bodies" (some of them of specifically characteristic appearances) make it quite likely that such a position is their natural habitat, in which they multiply and from which they spread, as they do, to other places, liquids and secretions. This habitat might have something to do with the peculiarities of their immunological relations. Living within cells it is perhaps unnecessary for them to produce any definite toxins: mechanical disorganization of the cellular anatomy might well be the effective cause of the injuries they produce. The general symptoms of infections (headache, fever, prostration) are caused, as in bacterial infections, mostly by substances derived from the injured cells of the host, and these would also account for part at any rate of any local inflammatory response.

Such an explanation would do quite well for the viruses that accompany infectious diseases and would cover the facts for the bacteriophage. But

phenomena are known which are surely more or less analogous, and which it is hardly possible to regard as due to parasites of any kind.

There is, for instance, the agent which induces cells to become malignant, indicated years ago by the Imperial Cancer Research Fund (Haaland and Russell) when they showed that close contiguity with malignant epithelial cells might cause normal connective tissue to grow into a transplantable sarcoma—one of the great discoveries of pathology. People had known that they met with tumours occasionally which seemed to be mixtures of carcinoma and sarcoma; they knew too that the cells at the edge of small epitheliomas looked as if they were being transformed and were on the way to become malignant, through the prevalence of a curious dogma to the effect that this could not really be so generally prevented them talking about it. The experimental mouse work explained these appearances: without it they could have carried no serious inference that cancer cells might influence normal cells towards malignancy. Unless we suppose that tumour cells pervert neighbouring normal cells by argument, persuasion, example, or some other sort of immaterial communication, we naturally assume that some substance passes out from the one to affect the other. All attempts to demonstrate this substance in dead tumour cells or in extracts of them uniformly failed until Rous came across his fowl sarcoma and showed that it could be transmitted indefinitely from bird to bird by dried dead cells or by filtrates which contained nothing that could be seen or cultivated. This particular tumour produces the substance in a form so stable that it can be examined and played with when it is detached from live cells. With most transplantable tumours it is present in such small amounts, or more likely in such a labile unstable form, that its clear demonstration is not possible: the carcinoma-sarcoma experiment comes off only with a minority of mouse carcinomas. Gye has shown that its activity may be modified, enhanced or depressed, by various conditions, which helps to explain the difficulties and apparent inconsistencies which are met with in its experimental investigation. But a fair number of tumours have now been transmitted by filtrates, and there is, I think, no reason to doubt that the production of this carcinogenic substance is a common property of all malignant growths. We believe that all pathogenic bacteria, or at any rate all the larger ones, produce extracellular toxins: there is no other way in which they can injure the tissues. But in many instances they are so unstable that it is at the best difficult to demonstrate their presence apart from the bodies of the bacilli, and impossible to investigate them in detail. Nor should we, I think, be too shy of drawing general conclusions from such specially easy and demonstrative examples as Providence has provided for our learning and pushes under our noses, till even our stupidity is bound to take notice: diphtheria and tetanus for toxins, the guinea-pig's peculiar bronchial musculature for anaphylaxis, mice and tar for tumours, radium are such sign-posts; the Rous tumour is another.

Another analogous phenomenon takes us, I think, a step further. The products of autolysis of dead cells in the body, in suitable concentration, stimulate tissue growth. It is a beautiful self-regulating mechanism in which the amount of stimulus is proportionate to the amount of cell destruction, and therefore to the amount of cell growth required, and it is obviously of the highest importance for survival—a far more potent factor in selection and evolution than any disease has ever been. As it normally operates in healing our cut fingers, the final result is simply the restoration of the cells which were destroyed. But if the normal restraint exercised by neighbouring tissues is evaded and use made of tissue cultures, the products of autolysis or metabolism (in the form of extracts of tissues, tumours, or embryos) stimulate growth indefinitely and a much larger quantity of tissue may be obtained than we started with. From the autolysis of this a larger amount of stumulating substance may be obtained, and there seems no reason why this process of multiplication should have any limit: normal tissues in the physical isolation of

tissue cultures are as immortal as malignant tissues in their physiological isolation from the rest of the body.

No one would, I think, pretend that these products of autolysis are alive in any ordinary sense of the word. They have not received nearly as much attention as they deserve, but they are probably of relatively simple and discoverable constitutions. Yet applied to cells they cause growth, and in so doing potentially increase their own quantity; this is very much what the Rous agent does. There are, too, these further minor similarities: the Rous agent stimulates one particular type of cell (white fibrous connective tissue) to malignancy, some extracts of normal tissues stimulate fibroblasts in tissue culture while others act specially in epithelial cells; the activity of the Rous agent may be encouraged or depressed by the simultaneous presence of other tissue extracts, some tissue extracts inhibit growth instead of encouraging it.

But the chief importance of the analogy is, I think, in throwing light on the nature and origin of the Rous virus. If we agree to put the products of autolysis in the category "dead," by what difference are we to separate the Rous virus as being "alive"? It cannot be cultivated apart from live cells, it multiplies only under conditions where its specific activity is displayed, its inactivation by chloroform and other protoplasmic poisons does not take it nearer life than are toxins or enzymes or indeed simple metallic catalysts, and its retention of activity after the drastic methods of purification recently described by Murphy seems to definitely exclude it from "live." As to its origin, all the evidence seems to concur in indicating that the Rous virus arises *de novo* in each tumour. There is no epidemiological evidence that cancer comes into the body from outside; everything we know supports the classical view that it is a local autochthonous disease. Most of the experimental work with the virus has started with an actual tumour, and it is therefore just possible that an agent might be carried along through the whole series which originated somewhere else than in a tumour. But experimental sarcomas produced by embryo extract and indol, arsenic or tar have been transmitted by filtrates, and if others have failed to reproduce Carrel's results I would only remark that in a question like this one positive experiment is worth more than a great many negative ones. Epitheliomas are easily produced in mice by tar and in men by chronic irritation, and if we believe that all malignant tumours contain more or less of a carcinogenic agent akin to the Rous virus, it follows that we can with a considerable degree of certainty stimulate normal tissues to produce virus. It is, therefore, not very remarkable that Murphy, Leitch, and Brebner have at any rate occasionally demonstrated a carcinogenic agent in preparations of normal tissues (testes, pancreas and embryo *plus* placental extract).

It is difficult to escape the conclusion that the Rous virus arises in the tumour. There is no doubt that it is a means by which a tumour may be experimentally dispersed through any number of available animals, and it is apparently responsible for some at any rate of the metastases which occur in the course of the natural disease. But there is no evidence that such a virus ever naturally causes a fresh tumour, and we learn the important lesson that the means by which a disease is propagated may not be the same as that by which it was originally started.

This consideration becomes particularly interesting when we try to bring a frankly infectious disease such as foot-and-mouth disease, measles or smallpox into comparison. Brought up as we all have been in the heyday of bacteriology, it is a little difficult for us to get an unprejudiced view of the situation. Because an agent is constantly associated with and, as we believe, is the cause of a disease very similar to others which we feel assured are caused by bacteria, we naturally assume that its natural history is more or less similar to that of bacteria. We might have been in a better position to take a just view of the facts if we had lived in pre-bacteriological days, or if we could put on some of the complexion of Charles Creighton's outlook and do our best to imitate his learning and industry.

The chief way in which the virus of e.g. foot-and-mouth disease differs from the Rous agent, and, going a step further back, from the products of autolysis (or metabolism) which stimulate growth, is that it seems to spread about pretty easily from one individual to another: chiefly, I think, from the parallel of bacteria we take this to imply the possibility of independent life and probably independent multiplication. But we have no direct evidence of this: all we know is that, like the Rous agent, it can be deliberately dispersed through any number of individuals indefinitely, and that it multiplies only when and where it produces its specific effect. The blister which is determined on the foot of an inoculated guinea-pig by slight local injury is pre-eminently the place in the body where the virus is found in the largest amount, and, trying to be as open-minded as we can, we must allow that this may be due either to the lesion being produced where the agent is present in greatest quantity, or to the agent being produced in greatest quantity where the lesion is. You may say that if the guinea-pig is inoculated with a filtrate, i.e. with nothing but virus, the lesion must be due to the virus; no doubt that is in a general way true, but it does not follow that the whole of what we call the lesion is due to the immediate and direct action of the virus. Local effects at the site of inoculation (if they occur) prove nothing: they may well be determined by the concomitant injury. Putting aside all bacteriological analogy, we have no proof that the particles of virus which we get out of the lesion are directly descended from those we put in. In other words, we have to reopen the question which most of us regard as settled: is the agent the cause of the disease or is the disease the cause of the agent? Another stupid antithesis, for the alternatives are not mutually exclusive: both might be true.

From the time when Pasteur first began to persuade the world that micro-organisms might be something more important and effective than microscopical curiosities, there have never been wanting nonconformists who have held that microbes were the result rather than the cause of putrefaction, fermentation, and disease. It is very difficult—indeed, it seems impossible—to believe in this thesis in respect of bacteria which can be shown to have an independent life by cultivation and which can be inoculated into an animal with the production of a definite disease (e.g., tuberculosis); the bacteria which we get out of the experimental lesion may without undue credulity be supposed to be the direct descendants of those which we have put in to produce it. But, as Hamer and Crookshank remind us, we have quite possibly gone too far in identifying a "disease" with its accompanying microbe and defining diseases in terms of what we believe to be their causative agents. If it is sound to do this (as it certainly appears to be) with some epidemic diseases, it does not follow that the method can properly be applied to all of them. After all "similarity" between diseases is liable to be superficial: most of the clinical symptoms of infections are due to the reaction of the body, and on *a priori* grounds one would expect resemblance between diseases of quite diverse ætiology. I conclude, therefore, that we have to admit the possibility that, as in the Rous sarcoma, the viruses which we associate with certain diseases are not their original causes though they may be the means by which they are propagated and carried on.

You will probably say—and I think with a good deal of justification—that it is contrary to all common sense to suggest seriously that the viruses of diseases like smallpox, measles or rabies arise anew in each infected person. And it may indeed be nonsense. It is evidently more conformable with our general experience and with the epidemiological dogma to which we subscribe to lay stress on the definite way in which each case can be traced to a preceding case and that to another and so on, explaining such examples of apparently spontaneous origin as we meet with by carriers and the imperfections of our data rather than by the concurrence of a favourable epidemic constitution of the atmosphere. With that point of view I quite agree: the evidence that in an epidemic something is passed on from one case to the next seems extremely strong. But at the same time I cannot

altogether get rid of the uneasy suspicions which intrude when I think of e.g. foot-and-mouth disease, distemper or labial herpes.

Distemper seems to be everywhere where there are susceptible animals, and if the stock of dogs at Mill Hill can be kept free from it indefinitely it will be a point of much more than technical interest. As to foot-and-mouth disease, in which no material connexion between one outbreak and another can be discovered, I think that the unbiassed man in the street would say that the facts showed either that the virus was universally dispersed, possibly in some common animal (such as the hedgehog[1]) other than the cow, or that the disease was continually beginning afresh. Labial herpes seems in much the same position. Epidemics may be found by ransacking the literature but they are certainly not common. Not only has herpes no connexion with itself but it has a definite association with other diseases—pneumonia and severe catarrhs: its possible relation to human encephalitis does not help us—both are blind men. It is possible that the virus is an offshoot from the pneumococcus, though when Perdrau looked for it in pneumonic lungs he found instead another "agent" which could be transmitted through rabbits in series.

I daresay, however, that some simple explanation will be found for these epidemiological difficulties and that any suspicions that we may have about the origin of these viruses will be allayed. Viruses can remain dormant in live animals for a long time and carriers might be activated by a variety of incidents. But what are we to make of such a phenomenon as virus III? Virus III is made manifest by inoculating a filtrate of an emulsion of a rabbit's testis into the testis of another rabbit. This procedure is sometimes followed by an inflammatory reaction and the production of intranuclear "bodies," and if this inflamed testis is emulsified and the filtrate inoculated into another fresh rabbit the inflammatory condition is reproduced: thereafter the "disease" can be carried on indefinitely. It is not fatal, and after bad attack has subsided a rabbit is refractory to further inoculations and his blood-serum can prevent infection with active virus. If we knew nothing of bacteriology, should we not conclude that the virus had been generated by our procedures from the tissues of the normal testis? The only evidence to the contrary is analogy, and the slender fact that the phenomenon comes off more easily in New York than in London rabbits. I do not know how many people have tried similar experiments with other apparently normal tissues: if they had been positive we should certainly have heard about them; Leitch's, Brebner's, and Murphy's successes with sarcoma have already been mentioned and bacteriolysins transmissible in series have been extracted from normal organs.

It might be expected that what we know of immunity to these viruses would throw some light on their origin and nature, but as a matter of fact it does not seem to give us much help; as far as it goes, it is perhaps against their autochthonous origin. Two points are certainly clear. In susceptibility to reinoculation and in the neutralizing properties of the blood-serum, the immune reactions are at least as sharply specific as they are with most bacteria; some viruses show immunological races as bacteria do. The facts of natural immunity are also very similar: a virus may affect one, two, or several species of host and have special affinities for certain tissues. We might use this analogy, and the general proposition that immune reactions occur only if the antigen and the reacting animal are of different species to argue that viruses must come from outside the affected animal, and to say e.g. that if virus III originates from rabbit tissues it ought not to stimulate a rabbit to an antiresponse as it does. The argument seems to be rather a strong one, but it is not conclusive. It is easy to suppose that the virus, whatever its origin, would not have on it the stamp of complete rabbitness: considering its

[1] Mr. Charles Oldham tells me that at the end of the 18th and beginning of the 19th century churchwardens in Hertfordshire put as high a price (4d.) on the head of a hedgehog as on that of a polecat. "Urchins" were supposed to do something to cows which diminished the yield of milk and this was translated into a belief, still extant, that they sucked the cow's udders when they were lying down. Such expenses were not lightly incurred in those days.

size and its other peculiarities it would perhaps be rather remarkable if it had. We know, too, now that the general immunological rule about specific differences and specific identities has many exceptions. The lens of the rabbit's eye is antigenic to the rabbit and in common with such proteids as casein and egg-albumin it is not species specific; a mother reacts to the blood-corpuscles of her fœtus if they happen to belong to a different blood-group; the development of one tar cancer makes all the rest of a mouse's skin refractory to the development of another, though whether the resistance is to the mouse's own malignant tissues or to a virus which has developed in them we do not know. One can hardly, then, I think, be sure that a virus has an extraneous origin because an animal treats it as an antigen.

Whatever filtrable virus we look at we meet with the same difficulties. A good many people are willing to believe that the bacteriophage is generated by its bacillus—which is probably the truth. And they would explain the way in which each bacteriophage more or less fits its own bacillus by its having originated from that bacillus. Others see in their multiplicity evidence that bacteriophages are really live organisms with the characteristic variability and adaptability. It is perhaps more than a coincidence that it is in another group of plants that the same difficulty has arisen: the agents of plant mosaic diseases have never been found apart from affected plants; they have not been cultivated; no one can be sure whether there is one virus or many viruses. Lysozyme is another phenomenon about which one would like to know more. It is widely distributed in animals and plants and is abundant in egg white; withstands drying, alcohol, chloroform, etc.; acts on dead as well as live bacteria, and would pass for an enzyme were it not increased in amount by dissolving *Micrococcus lysodeikticus*. Such multiplication during the exhibition of its activity seems to connect it with the viruses, but Fleming says that it cannot be carried on by serial cultures.

If viruses do originate in tissue cells, what are we to imagine that they are? Béchamp's ghost would answer "microzymes, as I told you seventy years ago." Altmann would say bioblasts, others micellæ and even mitochondria, and all the people who have imagined that cells are made up of much smaller essential elementary live particles would see in the present development the fulfilment of their prophecies. They cannot all have been exactly right; bioblasts are quite big, and mitochondria (which some have supposed to be symbiotic organisms) are also visible, and not only to the elect. But it may well be that they were making as shrewd guesses at the truth as Prout did when he suggested that all elements were ultimately compounded of hydrogen. Till Harrison did it we had not suspected that the cells of warm-blooded animals could be cultivated *in vitro*. If they can live and multiply, divorced from their proper community, is it altogether impossible that parts of cells might have something of a separate existence also just as electrons may operate apart from atoms? Granting that they might, why should they have such injurious effects? To which there are two answers; first, we apprehend only such disembodied parts of cells as produce some definite effect which we can observe, and, as it happens we have perceived only those which do damage: second, believing as the fundamental proposition of morbid anatomy that structure and function go hand in hand, we should naturally expect such gross aberration of structure to result in such a departure from the normal course of function as, in this so nicely adapted world, would manifest itself as injurious.

What to make of all this confused mass of facts and speculation I do not know. We seem to have a fairly definite group of things which: (*a*) are very small; (*b*) can multiply; (*c*) have no independent life; (*d*) are of uncertain origin. Of their multiplication we know that the association of live cells is necessary, and that it occurs when the specific effect of the agent is manifested: we do not know that direct multiplication is possible at all. Of their origin, we have strong grounds for thinking that some are derived from live cells and we cannot exclude this ancestry

for any of them. They seem, too, to form a series: (1) the growth-promoting substances from tissues show indirect multiplication but make no other suggestion of life; (2) lysozyme would pass for an enzyme except that it can multiply; (3) the Rous agent and the bacteriophage arise repeatedly in malignant tumours and bacteria respectively, and may be in some sense alive, but they are not independent species of animals or plants; (4) the pathogenic viruses represent a further step towards being wholly alive. Taking one thing with another, I am inclined to think that they are both the cause and the result of their diseases as Sanfelice suggested for epithelioma contagiosum. Somehow or other a virus arises in an animal or plant and by its action on the tissues causes them to produce more of itself. Some viruses (e.g., smallpox) acquire a considerable capacity of spreading from infected to normal individuals and the majority of cases of the disease are so caused; the virus is on the way towards independence. Others (e.g., herpes) have little or no power of dispersion and most cases are due to the virus arising *de novo* under the appropriate stimulus (whatever that may be). You may say that if that is so it is strange that one case of herpes is so like another and that epidemic virus diseases are so uniform in their characters and so "true to type." It is, indeed, rather curious, but the circumstances which lead to the generation of a virus are presumably often repeating themselves, the possibilities of parts of cells having a separate existence are very likely limited, and after all the specific characters of infectious diseases are not always very sharply defined. However, these are difficulties which I am not prepared to solve; my object has been to ask questions rather than to answer them.

Section of Odontology.

President—Mr. GEORGE NORTHCROFT, O.B.E., L.D.S.E.

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PRESIDENT'S ADDRESS.

The Desirability of the Establishment of a Rationalized Standard for the Prevention of Dental Disease in Children.

By GEORGE NORTHCROFT, O.B.E., L.D.S.E.

IN the midst of our busy professional lives, unhampered by the elaborate specializing which characterizes many of the practices in the Western Hemisphere, it would seem well to pause awhile and take stock of the position we have obtained or strive to attain in regard to the rising generation, and what we are doing, or conceive should be done, for the better dental health of the children of this country.

We can hardly be satisfied with things as they are: caries and sepsis are still rampant, and still we hear the parrot-cry about the uselessness of deciduous teeth and their unvarying regularity.

Looking back, however, over a long professional career, I can discern the prospect of a day when a rational hygiene will be vigorously promoted and balanced diets be insisted upon, and when parents eager to do the best for their children, can be referred to the findings of scientifically conducted investigations which will have solved for the public good many of these problems now so difficult and contradictory. These solutions may lead to the elimination of those curses of modern civilization, dental caries and oral sepsis.

A consideration as to the best methods by which we can promote the prevention of dental disease necessarily involves the study of past efforts and those that are at present stimulating the activities of the profession.

Even in the eighteenth century such outstanding men as Fauchard, Berdmore, Lecluse and others, insisted on adequate attention to the deciduous dentition. Berdmore especially notes the disorderliness that arises in the permanent dentition when the deciduous teeth suffer from neglect. In 1754 Lecluse wrote that "it is, above all, necessary that the dentists should impress upon young persons the advantages and inconveniences that arise from a mouth well or ill-kept," adding that "the habit of oral cleanliness, once acquired, lasts a lifetime and costs nothing."

But while the profession then was aware of the necessity of oral hygiene, and appreciated the unsightliness of irregular teeth, they looked on themselves rather as dental therapists for their individual patients, and seem not to have concerned themselves with the larger aspects of prevention. There was at that time no coördinated movement by the profession advocating the national importance of the care of the teeth of children. Judging from the ravages of caries and the number of malocclusions among children to-day, such wisdom as our predecessors chose to impart seems to have fallen on deaf ears.

It is interesting to note in passing that the first number of the *Dental Cosmos*, in August, 1859, contained an article on the Preservation of Deciduous Teeth. In 1885 William Fisher, of Dundee, read a paper at the Fifth Meeting of the British Dental Association drawing attention to the deplorable lack of attention to the teeth of children of school age, advocating compulsory examination, the elimination of caries before a pupil was allowed to enter school, and instruction in preventive oral hygiene. His contention was that caries should be regarded as similar to scabies, ophthalmia, ringworm or the exanthemata, the presence of which prevented entry into school. He also wished to see the appointment of dental surgeons to all schools under State control, who should, among other things, give lectures on dental subjects.

One of the earliest of such appointments was made by the guardians of the school for pauper children at Anerley, where 850 boys and girls between the ages of

3 and 16 were boarded, lodged, clothed, fed, and educated out of the Poor Rates, and a half-time qualified dental surgeon was paid £60 per annum for his services!

Fisher's suggestions brought home to the Association the reasonableness of obtaining reliable statistics for the purpose of "acquiring a more exact knowledge of the condition of children's teeth at various ages," and "showing by means of individual facts so acquired, the disabilities under which children frequently suffer in their growth and development, and the important bearing the condition has upon the future health of the individual," and, it might have been added, on the prosperity of the Nation.

A series of seven reports was drawn up by a committee of the B.D.A. covering the years 1891-1897. At the end of that time eleven schools in London under the Local Government Board were supplying dental treatment to their pupils, the first starting in 1884 and the last in 1896. Eight years after the last report, in 1905, an Interdepartmental Committee reported on the Physical Deterioration of the Nation, and when dental deterioration was considered, use was made of these statistics and it was recommended to the Board of Education that:—

(1) The teaching of the elements of hygiene should be compulsory in schools, and in this teaching the care of the teeth should have special attention.

(2) Daily cleansing of the teeth should be enforced by parents and teachers.

(3) Systematic examination of the teeth of children by competent dentists employed by school authorities, should be practised to prevent caries extending, to stop carious teeth, and to remedy defects of the teeth.

Also instructions should be given to mothers as to the proper food for infants, so that as far as possible, dentition might be normal, and conditions removed which might cause dental disease so profound as to affect the whole system.

In Germany in 1894, Röse had drawn attention to the deplorable state of the teeth of the children of Freiburg. Ten years later the First International Congress on School Hygiene met at Nuremburg, and at the Second held in London in 1907, Dr. Jessen pleaded for the establishment of Municipal School Dental Hospitals.

In 1906 and 1907, about the same time as the Dental Clinic for School Children was opened at Cambridge, Jessen was carrying out similar pioneer work at Strassburg. In four years' time 119 clinics had been opened in Germany, most of them supported by a municipality, and free.

A school clinic was also founded in Vienna, about which far too little is generally known.

The outcome of these efforts was the establishment of School Dental Clinics all over England, and the formation of the School Dentists' Society, also the commencement of the Oral Hygiene Movement in the United States, culminating in the Forsyth Dental Infirmary in 1910, the Rochester Dispensary in 1917, and followed by the free clinic at the Hockser Foundation for Children in New York in 1920.

In the light of the foregoing facts it is a little obscure why the Editor of the *Journal of the American Dental Association* should say in the number for July, 1928, that the first organized effort on behalf of Children's Dentistry was the resolution passed by the Massachusetts Dental Hygiene Council: "that the only hope of real progress lies in the prevention or early control of dental disease, and to accomplish this attention must be given to the teeth of young children," and it is equally astonishing to read in the *Dental Cosmos* for September, 1928, that "at present there are comparatively few pedodontia clinics in the United States, or, for that matter, in the world."

Thus, we see that the public conscience has been slowly roused to the necessity of doing something for the amelioration of dental conditions in children, and it naturally looks for guidance to those who should know most about the subject. Surgery will slow down or mitigate a seemingly inevitable process,

cleanliness will help, the skill of the orthodontist can only assist further, and we must, after all, acknowledge that we have not as yet solved the problem of prevention, but are eager to do so and to discover the practical methods must be the ideal for which to work. In order to do this one should go back further in the process of development than hitherto, and forge a link between the prenatal and Infant Welfare Centres and the School Dental Clinics.

This can only be done by the creation of an experimental clinic for children of pre-school age. To emphasize the necessity of such a course I may repeat some cogent remarks from Fones' "Preventive Dentistry."

"The prevention of dental diseases is dependent upon the integrity of two tissues, the gingiva and the enamel." "The majority of all pathological conditions affecting the dental tissues are preventable, especially if preventive measures are applied during infancy and childhood." "Dental prophylaxis is that scientific effort, either operative or therapeutic, which tends to prevent diseases of the teeth and the surrounding tissues." "Statistics of the mouth conditions of children entering public schools at 5 years of age show that prophylactic measures to prevent dental caries of the deciduous teeth are then about two years late." "It cannot be denied that the ideal time of attacking the problem of physical defects is in the prenatal and preschool periods."

In addition to the above and further stressing the desirability of discovering what standardized methods of prevention should be adopted, one need only refer to the thoughtful and painstaking work of Russel F. Rypins, who states that the hypothetical immunity resulting from changed habits of diet has not been verified by sufficient observation.

If founded on the right lines, under the guidance of established research workers, assisted by skilled clinicians, such a clinic could, as a living demonstration, speed up the working out of problems at present only vaguely understood and teach approved methods for the prevention of dental disease.

It is to be hoped that some sympathetic philanthropist may undertake the launching of this invaluable scheme. Then not only may the question be approached by methods already established in many countries, but our knowledge may be advanced still further upon definite lines from an altogether new angle.

When the analysed results of such an experimental clinic are available, in a few years the problem of the prevention of dental disease in the young will be solved, and the consequent improvement in health and nervous stability will enable the "men of the future" better to withstand the strain of modern industrial and commercial life.

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The Radiographic Appearance of Chronic General Periodontitis.

By D. A. IMRIE, M.D. (Lausanne), L.R.C.P. & S.Ed., L.D.S., D.M.R.E.

THE skiagrams [shown on the screen: not reproduced] were made at the suggestion of Sir Frank Colyer from dried specimens in the Odontological Museum of the Royal Society of Medicine, which is housed at the Royal College of Surgeons, where the skiagrams will be displayed for future study.

The interpretation of dental radiograms is not easy, largely, I think, owing to the fact that here is the only instance in radiography where the film is not flat when being activated. There is of necessity a considerable degree of distortion of the images of the objects under examination. This distortion is increased also by the fact that it is unusual for the central ray to be at right angles to the film. Radiologists would welcome more "subsequent histories," either "immediate," as when a tooth, said to be absorbing, is found to be unabsorbed, or "remote," as when teeth said to be infected have been removed, and the patient has regained health. Most of the radiograms shown are of dried specimens, and are therefore not the exact parallel of similar radiograms of the living subject. They are unnaturally clear, owing to the fact that the X-rays were not required to pass through any soft tissues. In the making of dental radiograms the amount of extraneous soft tissue through which the X-rays pass is minimal, and, with the added advantage that the film can be placed in very close proximity to the bone, dental radiograms should surpass in clarity of bone structure those of any other part of the skeleton, with the possible exception of the digits. This point is often obvious when studying a series of dental radiograms. The upper central incisors are less clear, owing to the intrusion of the nose into the beam of X-rays. The effect of the interposition of other tissues is the production of scattered radiations, which literally fog the clarity of the image. The accurate placing of the film against the teeth and alveolar bone is easy with dried specimens, and there is no difficulty owing to the patient's intolerance of the film. The haziness of the trabecular shadows is of primary importance. It is the accepted and proved indication of inflammation in radiography of other parts of the skelton, when these parts are the subject of osteomyelitis, tuberculosis, etc. In fact the infection of the alveolar processes of the jaws is an osteomyelitis; generally of a subacute or chronic nature, it is true, but owing to the grave interpretation of the word it is rarely used in this special branch of radiology. Here I would claim, however, that as a result of the relatively greater clearness of dental radiograms, minute changes in bony structure can be detected more readily and earlier than they can be in bone more deeply situated. I am aware of several instances in which osteitis deformans, or Paget's disease, has been detected in the alveolar bone when changes in the skull and other bones were still too indefinite to warrant the diagnosis. It will also be necessary to refer to "areas of rarefaction." Such a description may not be accurate in every instance, for occasionally such areas are really areas of bone destruction. The term has acquired a significance now generally understood to indicate changes the result of relatively acute inflammation. The term "sclerosis" is here used to indicate the result of inflammation. The bone trabeculæ become thickened. As a result the delicate lacework of the cancellous bone is lost, and the cancellous spaces, normally angular, become rounded. To quote the neat and apt description from "Chronic Infection of the Jaws": the structure is changed from the Gothic to the Norman."

It is regarded as an indication of a relatively greater resistance of the host to the infection than is present when areas of rarefaction result. A similar result is seen in sarcomata of bone in radiography. If the growth be rapid, bone destruction only is seen. When the growth is slow, the structure again reacts by laying down new bone; often irregularly and often also beyond the normal limits of the bone. In one of the specimens on view there is apparently a proliferation of bone beyond the normal

limits about the necks of the molar teeth. This proliferation is not well shown in the radiograms, owing to the fact that the X-rays pass transversely through the proliferation, which is about two millimetres thick. The haziness of the trabecular shadows is not well shown in the dried specimens. This is due no doubt to the fact that the engorgement of the blood-channels and spaces of the bone, present in life, is now absent. These spaces are now filled with air, which offers little or no resistance to the passage of the X-rays and so causes no scattering. However, there will be shown on the screen one or two series of radiograms from the living subject, which are similar to the dried specimens, in which this haziness is well marked.

The infection of the alveolar bone takes place by two main channels:—

(1) By spread from the periradicular tissues, periapical infection and the like. This mode of infection I do not propose to touch upon, for the reason mentioned, that the periodontal membrane is non-extant in the dried specimens from which my pictures are taken.

(2) By spread from the surface or margin of the alveolar process. This is the more common, and also the more important in general periodontal disease.

The host's reaction to the infection is shown on the radiograms by the resistance, as measured by the sclerosis that is set up. In the most resistant, a rampart of sclerosed bone is laid down at the surface. In this manner, the delicate cancellous tissue below is protected from invasion, and the infection may be regarded as superficial. The products of the war being waged between the host and the invader are thrown off into the oral cavity, and, in the main, are safely dealt with by the digestive juices. This is the type of infection often encountered in the aged, who may lose all their teeth, and yet apparently suffer little in the process so far as the general systemic reaction shows. Let me here stress the point, however, that this loss of teeth by the aged is not a natural and physiological process. I will throw upon the screen a series of radiograms of an individual aged 70, in which there is little trace of destruction of the marginal bone. When, owing to the relative potency of the infection, the host is unable to throw up this rampart of sclerosed bone against the invader, the appearances are different. The radiograms show the summits of the interdental septa to be destroyed, and "open." By close scrutiny of the deeper bone, the trabecular shadows are seen to be altered, in places they are thickened, and in others broken, and there is a general fogginess of the picture. In these cases there is frequently some systemic reaction to the infection, often of a serious nature, and, if the mass of the infection be great, the patient may be very ill. When one considers the size and extent of the alveolar processes of the jaws, this mass can be great; certainly it may be a much greater volume of unhealthy tissue than the other commonly suspected source of infection—the tonsils. It must not be inferred that the quantity of the infection is the all-important factor. Much more important may be the quality or type of that infection. There are many cases on record where the infection around a single tooth has caused profound systemic disturbance, the removal of which has had a salutary effect. The absorption from a focus in the unyielding bone is probably greater than from one in the softer tissues. There is another group of cases, which may be taken together, and roughly generalized under the heading of the "rheumatics." They may be placed mid-way between the two foregoing groups. With this type, the common finding is a reaction in the deeper bone in the nature of a sclerosis, but as a rule the haziness is less pronounced; the infection is probably relatively less acute.

The radiograms which have been made of the dried specimens are not the exact parallel of those of the living subject, but are unnaturally brilliant and clear. They are most advantageously examined on a viewing box, with the aid of magnifying glass. They do not show their finer points well on the screen, and they reproduce so unsatisfactorily in the blocks of the printing press that I have refrained from reproducing them at all.

Section of the History of Medicine.

President—Dr. HERBERT R. SPENCER.

[*October* 3, 1928.]

The Beginnings of the Literary Renaissance of Surgery in England.

By Sir D'ARCY POWER, K.B.E., F.R.C.S.

THE Wars of the Roses so troubled England during the fifteenth century that surgery became little more than a handicraft and there is no surgical literature. The process of reconstruction occupied the first half of the sixteenth century and began with an attempt to unite the Fellowship of Surgeons with the Company of Barbers. The former body, impoverished and small in numbers, represented the Consulting Surgeons of to-day—men who had served their apprenticeship in war and found their occupation gone when peace came; the latter, numerous and rich, were the general practitioners of the day, and had obtained a Charter as a Livery Company of the City of London. They performed their duties well, but were commercially-minded and had no wish to ally themselves with impoverished surgeons who were, perhaps, more highly skilled in their science and had certainly seen more of the world; the very wideness of their outlook might make them inconvenient partners for they would be sure to advocate radical changes in a business which was doing well on the old lines.

Better counsels prevailed and relationships changed during the next forty years under men like George Keble, to whom Clowes was apprenticed, Thomas Vicary, Richard Ferris, Sir John Aylef, James Monford and Robert Balthrop. The Surgeons and the Barbers were united into one Body Corporate in 1540, and the revival of Surgery as a profession began at once. The revival proceeded along two main lines, the one administrative, the other literary. It is with the literary side alone that I am concerned to-day.

The cry of the surgical reformers on the literary side was "Back to the Fathers." This same cry was used again in the early nineteenth century when, at the beginning of the Oxford movement, Newman set himself to read and to publish a new edition of St. Ignatius and St. Justin, urging his pupils to follow his example. So, after the Union of the Barbers and Surgeons the leaders of thought began to make a new surgical literature. Some, like Vicary, published without comment the writings of more than a hundred and fifty years earlier; others, like Hall, compared several manuscripts and published a revised version of his author; others again, like Read, Hall's son-in-law, issued an old writer with comments, or printed an unpublished manuscript. Read went further and obtained a translation of parts of the surgery written by Francis Arcaeus (1493-1574), then a modern surgeon; in this he followed the good example of Richard Jonas, who translated and printed Rösslin's *Rosengarten* from the *De partu hominis*. The way being thus prepared, Gale, Clowes and others wrote their own experiences in the form of text-books and monographs which are the foundations of English surgical literature.

The first known edition of Vicary's book is a duodecimo published by Henry Bamforde in 1577. It is called *The Englishman's Treasure*, and was issued posthumously, for Vicary died late in 1561 or early in 1562. There must, however, have been an earlier edition, for John Hall says in his *Treatise on Anatomy* (published in 1565) that he was following—

"the example of good Maister Vicarie, late Sargeante chyrurgien to the queenes highness; who was the first that euer wrote a treatyse of Anatomye in English (to the profite of his brethren chirurgiens and the helpe of young studientes) as farre as I can learne."

The book ran through many editions between 1577 and 1651, being reprinted on each occasion without change. In 1888 it was edited with notes by Dr. Furnivall and Mr. Percy Furnivall and appeared as an extra volume of the *Early English*

Text Society. Dr. J. F. Payne examined it with care, and I published an account of it in the *Transactions of the Bibliographical Society* (*The Library*, vol. ii, 1921-2, pp. 82-88). I see no reason to alter my conclusions then expressed that "There is no doubt Vicary compiled this *Treatise on Anatomy* and issued it with the full knowledge that it was already out of date, because he thought it would be useful to the students of the United Company of Barbers and Surgeons which had been founded in 1540. There is little doubt that he pursued, and perhaps originated, the plan which Hall imitated. He borrowed a manuscript and copied it with such alterations as his limited knowledge of anatomy allowed. He did not know or did not think it worth while to incorporate the work of Vesalius, or even of Geminus who was one of his colleagues, as surgeon to King Edward VI. Vicary worked from a single manuscript, and he printed it in an abridged form."

John Hall proceeded more methodically when he published part of Lanfranc's *Chirurgia Parva*, for he says it

"was translated out of French into the olde Saxony englishe, about two hundred yeres past. Which I haue nowe not only reduced to our vsuall speache, by changying or newe translating suche wordes, as nowe be inueterate and growne out of knowledge by processe of tyme, but also my labours in this behalf with other copies both in Frenche and latin; namely with maister Bacter, for his latine copie and Simon Hudie for his french copie, and other English copies; of the which I had one of John Chamber, and another of John Yates both very ancient with other mo; whose good helpe hath not a little farthered me in these things to the intent that it might perfectly come forth to a public profite which to do I was constreigned not only because I would not truste too muche to myne owne rude judgement; but also that by the authoritie of dyvers men of knowledge, this excellent worke (as it is worthy) may be the more effectually alowed and accepted."

Nothing is now known of Bacter or Hudie, those early bibliophiles of the Tudor period. John Chamber appears in a new light. He has long been known as the first person mentioned in the Charter of the College of Physicians (1518), and Holbein's picture of Henry VIII and the Barber Surgeons has made us familiar with his appearance. He was born in Northumberland in 1470, was educated at Merton College, Oxford, of which he was Warden from 1525 to 1544, and died in 1549. He was buried in St. Margaret's, Westminster. He was physician both to Henry VII and to Henry VIII, and, taking orders in the old Faith, he subscribed to the Reformed Church in 1536 as Dean of the Collegiate Chapel of St. Stephen's, Westminster. He was one of the physicians in attendance on Queen Jane at the birth of Edward VI, and in a letter written by him to the Privy Council concerning the Queen's critical state he signs himself "Priest." He was also in attendance on Anne Boleyn at the birth of Elizabeth. It is pleasant to think of him as assisting at the birth of a great Queen, as well as that of the College of Physicians and of the Barber Surgeons. There is now evidence that he helped at the re-birth of surgical literature.

John Yates was an active member of the Barber Surgeons Company; senior Warden in 1578; a little too free of speech, for in 1566 "he was dismist for revealing of secrets belonging to the mistery"; and an examiner in surgery in 1570. He is the chief speaker in that pleasant dialogue by Thomas Gale, entitled the *Institution of a Surgeon*.

Hall's *Treatise on Anatomy* is worthy of more attention than it has received. The date of publication is 1565. This is probably about the time it was written, for, in quoting Geminus, he says: "Whereof I gather that Geminus accompted but viii [bones going to make up the upper jaw] though he after (forgetting himselfe) wher he numbereth generally all the bones affirmeth xii." Geminus, therefore, was already dead, and we know that he died in 1560. The *Treatise* is written in English; it is an original work, not a compilation; it is a simple statement of anatomy as Hall knew it. It was up to date, for he quotes Carolus Stephanus, Vesalius, and Mundinus. It was intended for the use of the apprentices and

members of the recently-formed United Company of Barbers and Surgeons, who were obliged to attend the lectures on Anatomy and Surgery given by Dr. Caius until 1563, and by Dr. William Cunningham until 1567. The *Treatise* was therefore a textbook for those unskilled in the Latin tongue. It represents accurately the information given in the lectures of the day and indicates the character of the examination which every apprentice had to pass before he obtained his licence to practise "in London and seven miles round." I have constructed an examination paper which could be answered from the treatise, though it should be premised that the questions would have been asked *coram publico* and the examination would have been wholly oral.

(1) From what are the words Chirurgion and Cancer derived? Give the Greek or Latin equivalents for each.

(2) Mention the complexions of the body. How would you know whether a man was of a sanguine or saturnine complexion?

(3) How many bones are there in the human body?

(4) What are the uses of the Liver? Where does it lie in the body?

(5) Define an imposthume? How can it be cured?

(6) What does a surgeon mean when he speaks of an algebra? How should the condition be treated when it occurs in the upper arm?

(7) Mention in order the structures which would be divided to expose the great ventricle of the brain, beginning from the skin.

(8) Who were Galen, Avicenna, Lanfrancus, Guido Cauliacus, Johannes Vigensis, Andreas Vesalius, Thomas Geminus and Carolus Stephanus? What discoveries or methods of treatment are connected with their names?

N.B.—Questions 7 and 8 need only be answered by those who wish to obtain the Company's great diploma.

Hall knew his limitations in anatomy, and camouflaged his ignorance cleverly under the plea of brevity. Speaking of the deeper parts of the brain, he says:

"But for the copious declaration of the causes of these and other names, with many other circumstances hereunto belonging, for briefnesse sake here I leave the loving reader, unto the perfect enumerations of Galenus, Vesalius and Carolus Stephanus, where thy desires may be satisfied at the full" (p. 39).

In regard to the movements of the heart he was much in the same state as was Harvey when he wrote:

"When first I applied my mind to observation from the many dissections of Living Creatures as they came to hand, that by that means I might find out the use of the motion of the heart, I straightways found it a thing hard to be attained and full of difficulty, so with Fracastorius, I did almost believe that the motion of the heart was known to God alone, for neither could I rightly distinguish which way the diastole and systole came to be, nor when nor where the dilatation and constriction had its existence."

Hall, in a like manner, says:—

"Here also might be to great purpose declared the great secrets of these first movings which are in the heart called in Greek Systole and Diastole, in Latine Contractio and Dilatatio which are compared in this orbicle (of divers learned men) to the *primum mobile* or first mover in the great orb. For these movings are the first cause of all other movings, as of pulses in the arteries and so of all the rest. But because these secrets pass the capacity of the common sort, and also that I should break my purpose of briefness, I omit them and will now speak of the lungs or lights" (p. 68).

These passages show how near the Elizabethan anatomists had come to the circulation of the blood if only they could have thought and experimented as Harvey did half a century later. Harvey reflects the language of the schools when he uses Systole and Contractio indifferently just as Hall did.

Each part of the *Anatomy* ends with "A Conclusion" written in the pleasing Elizabethan style. Here is one:—

" Lo, thus have I ended this second treatise, although grossly and unlearnedly ; trusting notwithstanding that it will be gently borne withal. Truly, my masters and brethren, these are but ears of corn that I gleaned in times past after the bindings of the plentiful sheaves of others, trusting to God that hereafter (though not yet) I shall be able to bind sheaves of mine own and scatter some ears for such as must glean and gather their handfuls of other men's leavings. Neither wish I that we should any longer live than we should show ourselves profitable one to another. In the which, as we may learn in holy writ and as the divines continually blow in our ears, we shall be known to be the servants of our Lord and Saviour Jesus Christ, who taught his disciples to love [one] another. Saying, by this ye shall be known to be my disciples in that ye love one another. The which love God graunt us all. Amen."

A much more charming finish than the bald *Finis* of a modern book.

John Hall died in 1568 and I have often wondered whether the book was written in as entirely an altruistic spirit as he would have us believe. He was a young and ambitious surgeon who died at the age of 39 probably of some acute disease. He lived at Maidstone and it is possible that he was thinking of settling in London for he was already in friendly correspondence with Drs. Coldwell and Cunningham, distinguished London Physicians of the time. The anatomical treatise he may have thought would pave the way for his appointment as Reader of Anatomy at the Barber Surgeons Hall, a post which would at once have given him a first-rate position in the profession. If this were his object he was disappointed, for the Minutes of the Barber-Surgeons Company report that on "January xivth, 1566-7, here was Mr. doctor Julyo and he made a request that he might have the work of the anatomy these three or four years so that the college of physicians should not put him from us." The request was granted and Dr. Julyo replaced Dr. Cunningham who had been lecturer since 1563. Dr. Julyo was Giulio Borgarucci (the elder brother of Prospero Borgarucci, Professor of Anatomy at Padua) who had fled from Italy because he was a Protestant. He was thought to have an evil knowledge of poisons but he had powerful influence for he was physician to the Earl of Leicester and was appointed physician to the Royal Household in 1573.

The *Treatise on Anatomy* may after all have served its purpose, though not in the way hoped for by the writer. Dr. Smith resigned in 1577 and "Mr. Thomas Hall [John Hall's younger brother] of this company is granted to dissect the Anatomies, private or public, for a term of ten years that shall happen to be within this house and that there shall be yearly four, one private Anatomy at the least and one public if some reasonable cause be not found to the contrary. The same private always to be dissected before the public And the same to be always fetched from the place of execution by the Master and Stewards for the time being." Thomas Hall had evidently determined to become Lecturer on Anatomy at the Barber Surgeons Hall for he had been to Oxford and had graduated B.A. probably from Broadgates Hall (now Pembroke College) in January, 1567-8, and had taken his M.B., in July 1572, and with it a licence to practise. He then made assurance doubly sure by enrolling himself a Liveryman of the United Company, which he would have had no difficulty in doing as his brother had many friends on the Court and finally he attached himself to the College of Physicians, becoming a Fellow in 1585.

John Read, who had married John Hall's daughter Cicely, was the last to follow his father-in-law's example. He was a keen reformer and anxious "that all surgeons ought to have some knowledge of physic and that the Barber's craft ought to be a distinct mistery from Surgery." Read published in 1588 an early fifteenth century translation of Arderne's *Treatises on Fistula* and issued with it two books of the Surgery of Francis Arcaeus, a Spaniard, who died some time after 1574, and a very interesting and practical Tract upon the treatment of Stricture of the Urethra, which he calls, of course, by the name then current of "Caruncle or Carnosity growing in the yard or neck of the Bladder."

Read, therefore, followed faithfully the lines of John Hall, but times were

changing and there was no longer need to reprint the works of the older English Surgeons, nor seemingly were the surgeons in this country interested in their fellow-workers on the Continent. Men like Gale and Clowes were all-sufficient to themselves and were content to record their own experiences.

Had John Hall lived, he would in all probability have published another book on the lines of his previous work. The manuscript volume in the Bodleian Library (Bodl. MS. 2073, Ms. Bodl. 178) appears to have been in preparation for the Press. Headings have been added, lines have been struck out, and explanations have been introduced. There is no question that the book belonged to John Hall. It contains, in addition to some minor matters such as Prescriptions with English translations and a practical treatise on the compounding of medicines for the use of Surgeons, *A treatise on the French Pox written in Latin by Benedictus Victorius* (1481-1561) *translated into English by John Hall and the cure of the pox by fuming after Nicholas Massa* (*d.* 1569). The two treatises by the Italian Surgeons are greatly abridged. They are followed by a very long manual of medicine. It is incomplete at the beginning and is written throughout on parchment and is approximately of the second half of the fifteenth century. It is in English and treats of man's body in general, of diseases of different parts and organs, eyes, ears, nose, throat, lungs, brain, stomach, etc., under the headings of Causes, Signs and Curation. Finally, there are interspersed in the book letters or Consultations with Dr. Cunningham and Dr. Coldwell about patients whom Hall had been treating and about whose diagnosis he had been in difficulties.

So far I have spoken of books written by surgeons for surgeons, but there is evidence that a book on Surgery was published in the ordinary course of trade in 1543; there must therefore have been a demand for a standard text-book, though it was probably too expensive to command a large sale. It is the first edition of the English translation of John of Vigo. The translator does not give his name but "Bartholomew Traheron wisheth true health and prosperity to Richard Tracey" so perhaps he is responsible for it; Edward Whitchurch printed it with a privilege for seven years. Everyone connected with the book is interesting.

I need say little of so well known a man as John of Vigo. He was born at Rapallo about 1460. His skill in surgery was first recognized during the siege of Saluzzo in 1485 and 1486. Cardinal Giuliano della Rovere, afterwards Pope Julius II, attached him as chief surgeon to his train and thus afforded him such an insight into Italian life as enabled him to write his masterly account of the new disease known as the French pox. His fame was established in 1514 by the publication at Rome of his *Practica in Arte Chirurgica Copiosa Continens Novem Libros*, a work which ran through many editions and was translated into French, Spanish, Italian, English and German within a few years of its issue and into Portuguese many years later.

Bartholomew Traheron seems to have been born in Cornwall and was early left an orphan. He was educated at Oxford either at Hart Hall or at Exeter College where he attained some eminence in Greek and Latin and was persecuted for religion's sake by Dr. John London, the pestilent warden of New College, about whom Foxe has much to say in his Book of Martyrs. Traheron migrated, therefore, to Cambridge where he took the degree of B.D. in 1533 being then a friar minorite. He was maintained at the Universities by the liberality of Richard Tracy to whom he dedicates this translation of Vigo and whom he calls, perhaps metaphorically, his father-in-law.

Tracy was of Toddington in Gloucestershire. He was admitted B.A. of Oxford on June 27, 1515, and after studying at the Inner Temple, became M.P. for Wootton Bassett from 1529-36. When Mary came to the throne he was obliged to flee the country on account of his faith.

Traheron, the translator, moved by Tracy, "forsook the puddle of sophisters to fetch water from the pure fountains of the Scripture," as he expresses it. In other words he became a Protestant. He travelled through Germany into Italy, where he doubtless became acquainted with Vigo's book. He was at Zürich in 1537, at Strasburg in 1538. In March, 1538, he was in London in the service of Lord Cromwell. He retired from Court life in 1542 with the avowed purpose of marrying a rich wife, the daughter of a country gentleman who favoured godly doctrine, and of keeping a school for small boys in a country town, but there is no evidence that he succeeded in either of these projects.

King Edward VI appointed him keeper of his library at Westminster upon December 14, 1549, at the annual stipend of 20 marks. He was elected about the same time a burgess in Parliament, where he made strenuous efforts to prevent ambiguity about the Lord's supper in the Liturgy then established. He was living at Oxford in 1550, when he was appointed tutor to the young Duke of Suffolk. King Edward, however, finding him a person of merit caused him to be elected Dean of Chichester, but the Royal mandate was obeyed only after much delay and difficulty on January 8, 1551-2, and he resigned the deanery about December 1552. In September, 1552, he was granted a canonry of Windsor, a similar grant being made him in the following year. At the accession of Queen Mary he relinquished all his preferments, for he was an intemperate writer, and, like Knox and Goodman, regarded government by women as monstrous. He went to Germany a voluntary exile, and in 1555 was acting as divinity reader to fellow exiles at Frankfort. Holinshed states that he died abroad at the latter end of Queen Mary's reign, but Anthony Wood believed that he lived for some years longer. During his exile he seems to have sometimes adopted the name of Pilkington.

The publisher of this edition of Vigo is no less interesting than the translator. He was Edward Whitchurch, who, with his partner, Richard Grafton, printed and published Matthew's Bible and the Great Bible. Soon after the execution of Thomas Cromwell (Earl of Essex) on July 28, 1540, Grafton was imprisoned in the Fleet for six weeks on the charge that he had printed the Bible without notes. After this each partner printed separately, though they were still connected by some exclusive privileges, and this first English edition of Guido appears to be one of the earliest of Whitchurch's separate productions. Whitchurch, like Grafton, was brought up a merchant and was a citizen of London. After the martyrdom of Cranmer he married Ann, widow and second wife of the Archbishop. Whitchurch, lived first at the "Wheel and Two Buckets" in St. Martin's-le-Grand, secondly on the south side of Aldermary Churchyard, and lastly, at the "Swan" in Fleet Street, over against the Conduit, perhaps the old place of business of Wynkyn de Worde.

Section of Anæsthetics.

President—Mr. ASHLEY S. DALY, L.R.C.P., M.R.C.S.

[*November* 2, 1928.]

Endotracheal Anæsthesia.

By I. W. MAGILL, M.B.

ABSTRACT.—(1) With certain exceptions, endotracheal anæsthesia is the best method for operations on the head and neck and for any other operation in which there may be a difficulty in controlling the patient's air-way. (2) Expiration should be provided for, in endotracheal anæsthesia, either by means of a second tube or by a tube of calibre sufficient to permit to-and-fro respiration. (3) Cocainization of the upper air-passages has decided advantages in endotracheal anæsthesia. (4) "Blind" intubation through the nose renders the method possible in cases where it is impossible to use a speculum. (5) The insufflation method is not specially indicated in abdominal surgery. (6) The routine use of endotracheal anæsthesia in teaching-hospitals for every class of case is detrimental to the production of sound anæsthetic knowledge in students who are likely to become general practitioners.

THE history of endotracheal anæsthesia is sufficiently well known to require only brief comment. The name Elsberg will always be associated with the first application of the endotracheal insufflation principle to the human subject in the year 1909 [1].

Within recent years the supply of this form of anæsthesia might almost be said to exceed the demand—or shall we say the necessity—for it is worth remembering that before the advent of endotracheal anæsthesia, many satisfactory anæsthetics had been administered without interfering with the patient's larynx and without the aid of complicated apparatus.

Advantages of the Method.—(1) The anæsthetist has complete control over the patient's airway under most conditions. (2) The anæsthetist is able to keep himself and his apparatus clear of the field of operation, and consequently does not impede the surgeon, or interfere with his aseptic technique. (3) The surgeon can be protected from the anæsthetic-laden expirations of the patient, an important factor in a long operation in the region of the mouth or nose. (4) The patient is protected against the entry of blood into the trachea. (5) There is, or should be, no tax upon the respiratory mechanism of the patient. (6) The method is dosimetric.

Disadvantages:—(1) The necessity of an apparatus and a comprehensive equipment of catheters and tubes for patients of different ages.

(2) A certain amount of delay before an operation can be begun.

Both these factors arise chiefly in private practice.

Indications for Employment of the Method.—With the advantages I have mentioned in hand it would at first seem that the method is indicated in:—

(1) All operations upon the head and neck not actually involving the vocal cords.

(2) All operations in which the position of the patient entails a difficulty in controlling the air-way, and

(3) All operations in which the manipulations of the surgeon may be likely to cause respiratory embarrassment.

Contra-indications.—(1) In delicate operations (e.g., upon the eye), the possibility of the patient coughing at any stage of the procedure must be ruled out, and for this reason endotracheal anæsthesia is inadvisable.

As an example, I cite the case of a woman, aged 80, who had undergone one operation for cataract under local anæsthesia. For the same operation on the other eye a general anæsthetic was demanded, and the surgeon stipulated that vomiting would be a most undesirable after-effect. This patient was easily controlled with nitrous oxide and oxygen delivered through a funnel air-way. and I am confident that the result would not have been so satisfactory with intubation.

(3) In exophthalmic or toxic goitre, a combination of local anæsthesia with nitrous oxide and oxygen from a gas-bag is infinitely better than endotracheal

anæsthesia, as far as the after-effects are concerned, and in cases of this type it will be agreed, I think, that the method which insures the best post-operative condition of the patient is to be preferred to a method which provides greater comfort for the surgeon and easier control for the anæsthetist.

(3) Operations on the thorax, such as thoracoplasty and pulmonary decortication, give better results when nitrous oxide or ethylene and oxygen are administered by means of a face-piece. The condition of the patient after operation is ample compensation for the difficulty sometimes entailed in maintaining air-tight apposition of the mask. Moreover, an efficient apparatus provides for positive ventilation of the lungs without the necessity for intubation.

(4) It is obvious, also, that there are many operations of a trivial nature, or of short duration, which may cause some inconvenience to both surgeon and anæsthetist, owing to their situation. Such cases can be dealt with satisfactorily by simple methods in most instances.

Technique of Endotracheal Anæsthesia. (1) *Preliminary Medication.*—Atropine is administered to all patients and morphia in a suitable dose, when indicated. In nervous individuals and excitable children, paraldehyde administered *per rectum* one hour before operation gives excellent results. Provided the dose has been correctly gauged and the patient undisturbed after the injection, the operation can be performed without any memory of the induction, and the sleep which continues after the operation is over has none of the undesirable respiratory effects associated with morphia.

(2) *Cocainization of the Larynx and Fauces.*—The practice of using cocaine to diminish the activity of the reflexes in the upper air passages in general anæsthesia was suggested originally by Rosenberg [2], and I have found it a valuable aid in endotracheal administration. The effect is easily obtained with an infinitesimal quantity of a 20 per cent. solution from a nebulizer, and the advantages are:—

(*a*) Induction is rendered easier: (*b*) intubation can be carried out without the necessity of very deep anæsthesia; (*c*) diminution of the cough reflex permits a light maintenance level; (*d*) post-anæsthetic sore throat is less frequent; (*e*) the vocal cords being at rest, muscular relaxation can be obtained with comparatively light anæsthesia.

In this connexion, the relationship between muscular relaxation and the behaviour of the glottis is worthy of a digression. In his Presidential Address to the Section in November, 1926, Dr. Mennell drew attention to the fact that abdominal relaxation could not be obtained until the adductor muscles of the larynx had been paralysed, or, in other words, until the larynx was at rest and the glottis fully open. This relaxation of the vocal cords and the abdominal muscles arises in deep anæsthesia, but it can undoubtedly be brought about by cocainization of the larynx without recourse to ether saturation.

Risks in Connexion with Cocainization.—Cocainization of the larynx in the manner described is obviously open to much hostile criticism.

(*a*) The risk of cocaine poisoning and the question of idiosyncrasy must always be carefully weighed. But when we consider that, notwithstanding its reputed toxicity, cocaine is liberally used as a routine in operations upon the nose, this factor need not be an unsurmountable obstacle to its use.

(*b*) The temporary abolition of the protective cough reflex may be considered dangerous, but in any case it is surely desirable to keep this reflex in abeyance during the operation. Moreover, the method of anæsthesia itself protects the patient from the possible entry of blood into the trachea, and in practice the cough reflex returns before the catheter is withdrawn at the end of the operation.

(3) Induction of anæsthesia follows ordinary lines and need not detain us.

(4) Intubation can be carried out through the mouth or through the nose, and one or both channels may be available according to the site of the operation. When the anæsthetist has a free choice, it is often easier, as will be shown later,

to intubate through the nose. Whatever route may be selected, free egress for expiration must be provided [3] in all circumstances, especially in operations in which the endotracheal method is of greatest value. In operations involving the buccal cavity, or the lower jaw, for instance, the use of a single catheter leaves the care of the expiration to chance. A pharyngeal air-way may be all that is required in many cases, but this will not completely obviate the possible occurrence of obstruction to expiration during the operation. This difficulty with regard to expiration in endotracheal anæsthesia was solved for me somewhat dramatically in 1922, in the case of a man about to undergo an operation for the repair of extensive deformity of the lower jaw. A catheter had already been passed into the trachea through the nose, but owing to the deformity and contracture of the lower jaw, expiration became laboured. Attempts to find a free channel by means of rubber tubes passed into the pharynx were quite unsuccessful until one of these tubes accidentally entered the trachea along with the catheter. The relief which followed left no doubt in my mind as to future procedure in such cases. Since then it has been my custom when using the insufflation method through a catheter, to pass into the trachea, in addition, a firm-walled rubber tube of sufficient calibre to provide adequately for expiration [3]. I have called this the "double tube" method for lack of a better name.

The advantages of the second tube are :—

(1) Free expiration is assured under all conditions ; (2) The pharynx can be securely packed with gauze, a proceeding which is not practicable when a catheter alone is used for insufflation ; (3) The patient's head can be moved into any position without obstructing the air-way ; (4) The anæsthetic-laden exhalations of the patient can be diverted from the surgeon ; (5) There is no bubbling of blood to obscure the operative field in intra-oral operations ; (6) It is unnecessary to use high pressure to keep the larynx free from blood ; (7) Deliberate occlusion of the expiratory tube can be practised when positive intrapulmonary pressure is required.

Criticism.—This method of dealing with expiration has been adversely criticized since the return flow from the trachea is no longer available for driving blood from the neighbourhood of the larynx. This is true ; but the answer rests with the manner of packing the pharynx already described.

Intubation with One Large Tube.—Another method of securing absolutely free respiration is to employ a rubber tube of sufficient calibre to permit of to-and-fro respiration after the manner of Kahn's apparatus, and certain advantages may be claimed for it :—

(1) Endotracheal anæsthesia can be carried out on ordinary inhalation principles. (2) Elaborate apparatus is unnecessary. (3) It is the easiest way to administer endotracheal chloroform. (4) It permits re-breathing and is the best method of giving nitrous oxide and oxygen by the endotracheal route when ether and chloroform are contra-indicated.

Intubation with one large-bore rubber tube was first performed by me in 1920, and the occasion is not without interest from the anæsthetic standpoint. The patient, a woman aged 27, had sustained a severe injury to her nose. She had already been anæsthetized eight times, and her hatred of chloroform and ether was such that she refused to undergo further operative treatment unless under a guarantee that these anæsthetics would not be used. Further, she refused local anæsthesia. An injection of a quarter of a grain of morphia was given and she was intubated under nitrous oxide anæsthesia. The tube was connected to a gas bag and anæsthesia was maintained for 1¼ hours with no other agents than nitrous oxide and oxygen. Fortunately the result was satisfactory to both surgeon and patient.

This method has been previously referred to in a conjoint paper read before the Section by Dr. Rowbotham and myself in February, 1921 [5].

Technique of Intubation.—(1) *Intubation through the mouth* with the aid of an efficient speculum is a simple manœuvre already familiar, and the introduction of a

rubber expiratory tube is no more difficult than passing the catheter. If desired, the catheter and expiratory tube can be fastened together by means of a piece of silk and passed as one unit. Double channel catheters so far produced for endotracheal anæsthesia, are too cumbersome to be of service.

The difficulty of obtaining suitable tubes for expiration in children under two years of age, led me to produce for this purpose a thin-walled metal tube for attachment to the catheter [6]. This arrangement worked satisfactorily but I have since abandoned it in young children in favour of a tube sufficiently large to permit to-and-fro respiration into a gas bag.

(2) *Intubation through the Nose.*—The method of passing a catheter into the trachea through the nose by means of a special forceps used through a direct-vision laryngoscope has been described by me elsewhere [7]. With the aid of the curved stilette, however, the catheter can be made to enter the trachea through the nose, in many cases without opening the patient's mouth [4], and if a rubber expiratory tube be passed through the other nostril without a stilette, it will often be guided into the trachea by the catheter which is already in position.

Strangely enough, if one large rubber tube, say of three-eighths of an inch calibre, be passed through the nose in the adult, provided there is no nasal deformity, it will enter the trachea in similar fashion.

This method of "blind" intubation has many advantages: (1) It renders endotracheal anæsthesia possible when the mouth cannot be opened; I have performed it in many cases of complete trismus. (2) It obviates the risk of injury to the teeth and to dental bridge work by the speculum. (3) It can be carried out without the necessity for complete muscular relaxation of the lower jaw.

The ease with which "blind" intubation can be carried out is purely a matter of anatomical consideration. In the normal subject, if the head be placed in the position which gives the freest nasal air-way—that is to say in a position of slight extension—the course of the air-way from the nostril to the glottis is a gentle curve, and a rubber tube with a similar curve, or a catheter mounted on a curved stilette, can naturally be made to follow that course and enter the glottis. In fact, with the head in the correct position in relation to the body, the tube will enter the glottis more readily than the œsophagus.

Objections to Nasal Intubation.—Objections have naturally been raised to passing tubes into the trachea through the nose on account of the likelihood of introducing septic material into the lungs. I have yet to record a case, however, in which any such complication has arisen. It is highly probable, moreover, that micro-organisms present in the nasal secretions are also already present in the trachea.

The Maintenance of Anæsthesia.—Intubation having been carried out as described, the catheter is connected to an apparatus capable of the delivery of air or nitrous oxide and oxygen under sufficient pressure, with whatever regulated amount of ether may be necessary. I prefer nitrous oxide and oxygen to air, as the amount of ether used is much less, and can, in favourable cases, be dispensed with. I believe that it is advantageous also to be able to vary the amount of ether independently of the flow of the gases. This is best accomplished by means of some form of drop feed and vaporization by hot water.

Heating the Anæsthetic.—The question of warming—or attempting to warm—the anæsthetic to room temperature before it reaches the patient, does not seem worthy of consideration according to some authorities [8]. The observations of Shipway and Pembrey, however, show conclusively that warming the anæsthetic vapour, to room temperature at least, is both practicable and advantageous. I am convinced that the practice is good, especially when nitrous oxide and oxygen are employed to vaporize ether without rebreathing.

Choice of Anæsthetic.—At present, generally speaking, a combination of nitrous oxide, oxygen and ether best satisfies the requirements of surgeons in this country,

and the smaller the quantity of ether, the better the post-operative condition of the patient. The use of endotracheal nitrous oxide oxygen without ether has still some disadvantages: (1) Heavy preliminary medication is essential if an undesirable degree of suboxygenation is to be avoided; (2) the narrow margin between anæsthesia and recovery is a disadvantage with a tube in the trachea; (3) muscular relaxation is difficult to obtain when it is necessary.

Chloroform.—In robust or alcoholic individuals of the labourer type, chloroform and to-and-fro breathing through a wide-bore rubber tube give the best results.

Endotracheal Anæsthesia in Abdominal Operations.—The lessened respiratory excursion obtainable by endotracheal insufflation has led to the adoption of the method by some anæsthetists for all abdominal operations, and I understand that actual apnœa has been produced in the presence of deep anæsthesia in some cases, in order to provide the best operating conditions for the surgeon. Whether such a procedure is beneficial to the patient, however, is open to criticism. In my opinion, such interference with diaphragmatic activity is likely to lead to stagnation of the circulation at the bases of the lungs, with consequent tendency to post-operative pneumonia. In any case, sufficiently quiet respiration of natural amplitude can be obtained by open methods for most abdominal operations.

When extreme difficulty arises in controlling the air-way, I should not hesitate to intubate and, in that case, the method of using a rubber tube wide enough to permit to-and-fro respiration gives better results than insufflation through a catheter.

Use of the Method as a Routine in Hospital.—Notwithstanding the obvious advantages of endotracheal administration, I am of the opinion that its use for any and every case is a disadvantage in the teaching of students.

The student of to-day is the general practitioner of to-morrow, and if he is equipped with no knowledge other than the control of his patient by means of an elaborate apparatus, he will be at a loss when called upon to administer an anæsthetic in private practice with a piece of lint and a bottle of chloroform. However high our ideals in anæsthesia may be, this practical point is of importance. I have heard that in the teaching of students "the rag and the bottle" was the slogan of the late S. R. Wilson. It is for you to judge whether the opinion of such a master carries any weight.

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Discussion.—Mr. H. E. G. BOYLE said that in the history of this subject the names of Meltzer and Auer should be included. Further, the first three papers on the subject read before the Society were by Dr. Kelly, himself (Mr. Boyle) and Dr. Shipway.

Endotracheal anæsthesia should not be employed as a method for all cases.

In certain cases it was of great benefit, but in others it was unnecessary; for example, in tonsil operations, since a perfectly good anæsthesia could be produced by simpler means.

He knew of cases in which the beginning of the operation had been delayed for a considerable time, owing to difficulty in inserting the catheter into the larynx; this was annoying to the surgeon, especially to one who had never tried intubation. He welcomed Mr. Magill's valuable suggestion of cocainizing the larynx.

With regard to giving paraldehyde, or, as he had heard it described, "stealing a day out of the patient's life," he did not quite approve of the method, for he thought that the odour of paraldehyde was so horrible that it was adding an unnecessary unpleasantness to what was already rather an ordeal for most patients.

With reference to double intubation, except when there was going to be a great deal of blood in the mouth he did not usually practise it, and preferred a small catheter and an air-way.

He did not agree with Mr. Magill about endotracheal anæsthesia in abdominal surgery; for he had found it of great value, and since he had been using it for this type of case,

the work of the surgeon had been made much easier; but he greatly deprecated the production of apnœa; he much preferred to be able to see that his patient was breathing.

Students should be taught the technique of endotracheal anæsthesia, but they should also be taught the other and simpler methods as well.

Mr. C. LANGTON HEWER said that there were two points upon which he was not in complete agreement with Mr. Magill.

Prolonged operations upon the upper abdomen, such as partial gastrectomy, were simplified by the use of endotracheal anæsthesia. The diminished respiratory excursion, the absence of any laryngeal spasm, and the complete muscular relaxation were of great assistance to the surgeon, while, if "de-etherization" with carbon-dioxide oxygen was practised at the end of the operation, the risk of post-operative pulmonary complications did not seem to be increased.

The question of warming the insufflated gases was a difficult one, but he (Mr. Hewer) thought that if the temperature was raised appreciably, the patient lost more heat by sweating than he gained from the warmed vapour.

Dr. H. P. FAIRLIE cited four cases of respiratory complications following endotracheal administration of ether, and asked Mr. Magill his experience of the incidence of such complications.

Mr. T. POMFRET KILNER said he could bear testimony to the excellence of Mr. Magill's double-tube method of endotracheal anæsthesia for operations about the head and neck. In the specialty which he (the speaker) practised almost all the cases fell into this class. He had had a wide experience of the method in some hundreds of cases of his own, while, in his work in association with Mr. H. D. Gillies, he had observed many hundreds more.

Mr. Magill would be the first to acknowledge that the development of his method owed much to experience gained at the special Face and Jaw Hospital at Sidcup and he (Mr. Kilner) would like to say how much the development of plastic surgery owed to the perfection of Magill's method of anæsthesia.

Various non-endotracheal methods had been used at first, and, though these enabled much satisfactory surgery to be performed, difficulties were frequent; single-tube endotracheal anæsthesia had proved a boon and gave much greater freedom and scope to the surgeon; but it was not until the double-tube method was introduced that the surgeon, working in the nose and mouth regions, was freed completely from all anæsthetic anxiety and given an unrestricted operative field.

He could not understand how anyone who had seen the double-tube method employed could advocate the single-tube method. The perfect inspiratory and expiratory air-ways which it provided, no matter how great the deformity or into what awkward positions the head was placed, the carrying away of the ether-laden expiration from the surgeon (a matter of real import to those working directly over the face) and the complete prevention of "spluttering" of blood seemed to him advantages which could not be provided by any other method.

In the large number of cases which he had personally observed, he recalled one only in which the patient had suffered from post-anæsthetic laryngitis and this was of a trivial type.

He felt so convinced of the advantages of the method that he would not have been surprised to hear Mr. Magill advocating it for all cases; he had asked for it for members of his own family, and, when the time came for him to have an anæsthetic himself, he would demand it.

Mr. MAGILL (in reply) said that in his experience the risk of damage to the nose or vocal cords by the use of rubber tubes of three-eighths of an inch calibre was negligible. He agreed that it was not always necessary to employ two tubes, but the "two-tube" method gave better results in intra-oral operations of long duration such as the removal of impacted wisdom teeth.

In reply to Dr. Fairlie, respiratory complications as a result of intubation were fortunately very rare. He was accustomed to intubate, when necessary, in patients of all ages.

The excursion of the pressure-gauge was not materially altered by the use of two tubes. The pressure was always an indication of peripheral resistance and depended largely upon the size of the catheter. The pressure was therefore relatively higher in children than in adults.

The success of "blind" intubation depended upon the position of the patient's head. The epiglottis could often be negotiated, when it obstructed the manœuvre, by rotation of the catheter or rubber tube.

Section of Dermatology.

President—Dr. J. M. H. MacLeod.

[*October* 18, 1928.]

CASES.

Sarcomatosis Cutis.—H. C. Semon, M.D.—This patient (aged 42) a foreman in the building trade, was first seen in October, 1924, for a bluish, subcutaneous, painless tumour, about the size of a pigeon's egg, in the left flank. It was freely movable over the ribs, and there was no enlargement of the axillary glands. It was excised, and on microscopical examination it proved to be a round-celled sarcoma (section exhibited). After X-ray irradiation and observation over a period of about a year, the patient was lost sight of, but returned a month ago with a cutaneous lymphatic invasion of the front of his chest, and the interscapular region, present about three months. There are one or two areas, notably above the left nipple, where the extending neoplasm has assumed the dimensions of a tumour, about the size and not unlike the appearance of a mulberry. The condition as a whole, although the hard fibrous element is absent, reminds one strongly of the stage in the development of carcinoma termed "cancer en cuirasse." The glands and viscera show no evidence of invasion as yet. The blood-count is normal. A biopsy was refused. The interval between the appearance of the presumed primary growth and that of a rapidly growing local extension in the skin is unusually long.

Dr. A. Whitfield said he had had a parallel case, but did not know the patient's subsequent history. Many years ago Dr. Pringle and he saw a doctor who had been struck on the chest by a tennis ball. A hæmatoma formed, which did not resolve, and later it turned into a new growth, and there were about 70 nodules in different parts of the body. After radiotherapy the original tumour melted away, and whereas previously two or three fresh tumours per week had appeared, from the time of the applications to the original tumour, no fresh ones came, though those which were present did not disappear quickly. With continued radiotherapy all the tumours were destroyed and the patient went on with his duty in the Army from 1912 to 1917. In 1917, however, he had come to him (Dr. Whitfield) with three new tumours, and also some glandular enlargement. Probably the patient had since died. He (the speaker) suggested that in Dr. Semon's case the testicles should be examined.

Simple Achromic Nævus, of Blotchy Distribution, on the Front of the Chest.—F. Parkes Weber, M.D.—This patient, aged 39, has many small areas of relatively or completely unpigmented skin, irregularly grouped on the front of the chest, chiefly on the left side. I regard the condition as one of congenital leucoderma, and use the term simple achromic or hypochromic nævus in contrast with the ordinary simple pigmentary (hyperchromic) nævus. When the skin of the front of the chest is rubbed it immediately becomes red, owing to hyperæmia, and the pale spots can then no longer be distinguished from the rest of the skin. The condition cannot therefore be confused with the rare so-called "nævus anæmicus," which, according to H. Vörner (*Arch. f. Derm. u. Syph.*, Leipzig, 1906, lxxxii pp. 391-398), is due to the local cutaneous blood-vessels being limited to capillaries.

Four Cases of Lupus Erythematosus treated with Gold Preparations.—H. Haldin-Davis, M.D.—(1) W. B., male, aged 56. First seen in May, 1927, with lupus erythematosus of about three years' standing. It affected both cheeks, the end of the nose, which was much deformed, and the upper lip. Treatment was begun with krysolgan in doses from 0·015 gr. up to 1·5 gr. administered by intravenous injection. After ten injections there was considerable improvement. Relapse took place after an interval of four months. Treatment with krysolgan was resumed in October, but without much benefit. In December treatment with sanocrysin was begun, the initial doses being 0·05 grm., subsequently gradually raised to 0·5 grm. After eight doses, in February, 1928, cure was apparently effected, and treatment was stopped. In April,

1928, however, some recrudescence had taken place and treatment with sanocrysin was begun again and continued until July. He had eight more injections. The lesions all disappeared, for the most part without scarring. He has put on about 10 lb. in weight and the cure appears to be permanent.

(II) E. B., male, aged 38. This patient had had lupus erythematosus for over twenty years, and had been treated at several hospitals. Treatment with krysolgan was commenced in March, 1927. Injections up to 1·5 gr. were given intravenously. Some improvement was obtained, but after a time the condition became stationary. The disease affected the face, especially the right cheek, on which there was a large area of disease about 2½ in. square, the nose, the forehead and the skin behind both ears. In January, 1928, sanocrysin was given, the initial dose injected being 0·1 grm., and the dose was gradually raised to 1 grm. Altogether, up to September, 1928, he had fifteen injections. The improvement obtained has been very remarkable. With the exception of the large patch on the right cheek, all the lesions have disappeared, leaving pale and satisfactory scars, and that on the cheek is now very much reduced and no longer angry-looking and conspicuous. But the sanocrysin had had to be discontinued owing to a sore mouth and tongue. He is now being treated with neosilversalvarsan. Improvement continues, but not perhaps so rapidly as with sanocrysin.

(III) Mrs. R., aged 36, was first seen in June, 1928, for lupus erythematosus of ten years' duration. The disease was extensive, there was a large butterfly patch stretching nearly over the whole of both cheeks and over the bridge of the nose. There were other outlying patches in front of both ears and also on the scalp. This patient was treated with sanocrysin alone. She had only five injections, beginning with 0·05 grm.

Improvement was manifest from the first. The fourth injection given on July 17 was 0·5 grm. This was followed by a slight pruritic, papular rash on the arms, which soon disappeared, and a second dose of the same size was given on July 31. This was followed by considerable disturbance, headache, loss of sleep and a generalized dermatitis affecting particularly the arms and legs and to a lesser extent the trunk. The dermatitis passed off in about a month, while the improvement in the facial lesions continued and all the lesions eventually disappeared. She has now had no treatment for nearly three months and the improvement is maintained.

(IV).—Mrs. A., aged 56. This patient was first seen in March, 1927. She had an extensive eruption of ten years' duration, stretching in a continuous sheet over both cheeks, including the nose, down on to the chin and up on to the forehead. It was red and scaly, and apparently a case of severe lupus erythematosus. Treatment was begun with krysolgan and considerable improvement followed. The redness and swelling of the skin diminished, but then numerous nodules characteristic of lupus vulgaris were recognizable. The patient was given general light baths and some local applications with the Finsen lamp. Treatment with sanocrysin was begun in February, 1928, and since then great improvement has been obtained. Many nodules have disappeared, leaving pale scar tissue. After each injection of sanocrysin (0·5 grm.) there is some local reaction succeeded by improvement.

Discussion.—Dr. J. M. H. MACLEOD (President) said that he was especially glad to have seen the excellent result in one of the cases, which he (himself) had tried to cure for years by other methods. There was great enthusiasm in America for this form of treatment. Through the kindness of Dr. Howard Morrow, he (the President) had obtained a consignment of the American sanocrysin for hospital use, and would report later as to its efficacy.

Dr. H. W. BARBER said he had treated a large number of cases of lupus erythematosus with krysolgan, and a smaller number with sanocrysin. Some of his results with the latter, given in doses of 0·25—0·75 grm. had been dramatic, even after only three or four doses. Those with krysolgan had been good on the whole, but a large number of injections were necessary.

He had found that toxic reactions, which were common with sanocrysin, could be prevented by the administration of glucose, two or three ounces per diem in divided

doses, for three days before each injection. Some cases did not respond either to krysolgan or to sanocrysin. He believed that it was the tuberculous cases that usually did well, whereas those of streptococcal origin were often entirely refractory.

Dr. J. A. DRAKE said that the only gold preparation he had used was aurophos, and after this he had seen alarming manifestations. In one case a generalized erythrodermia exfoliativa had developed, persisted for months and had not disappeared. In a female patient an acute purpura had developed on the trunk and the mucous membranes, and she was ill for some time. In another case in the wards, one of pulmonary tuberculosis, in which the physician had given sanocrysin, a pustular dermatitis of the scalp had developed, and had proved resistant to treatment until the sanocrysin had been stopped; the condition then had cleared up completely. After a time he (Dr. Drake) had thought that the sanocrysin might be given again, but its administration had been followed by a relapse of the pustular dermatitis.

Dr. W. J. O'DONOVAN said that he had had one case treated with sanocrysin, in which acute exfoliative dermatitis, similar to salvarsan dermatitis, had developed, and three and a half months' hospital treatment had been necessary.

A unique case was that of a Jewess, R. R., aged 30, who had attended skin clinics from 1914 onwards. When seen in 1926 the scalp, the left shoulder and both cheeks showed extensive deep scarring and active lesions of lupus erythematosus. The Wassermann reaction was negative, the von Pirquet weak-positive. She was admitted to hospital in good general health on August 16, 1927. After the first injection of krysolgan (September 8, 1927) headache and pyrexia developed. No reaction followed four further injections, but on October 21 there was a fine exfoliation of the trunk. Later incontinence and diarrhœa had developed, with rapid loss of subcutaneous fat, and the patient had died on November 7, 1927. The post-mortem examination showed bronchial pneumonia with hæmorrhagic ulcerative colitis.

A striking contrast was afforded by an elderly woman now in his ward with acute and chronic lupus erythematosus of the face, hands and forearms, which, after several weeks in bed, without any response, had after one injection of 0·3 krysolgan showed immediate and progressive improvement.

Dr. G. B. DOWLING said he had treated over thirty out-patient cases with gold preparations, and his results supported the remarks of Dr. Barber. He agreed that if good results were to come from sanocrysin they would be evident early. The best were obtained in cases of the chronic fixed type, though one or two of the early mild variety had cleared up quickly under the treatment. Among several minor toxic effects he had had two cases of erythrodermia, of which both had recovered, though one developed eventually a persistent lupus erythematosus disseminatus. He thought that he had probably given too large doses in these cases. The majority of the others improved, though he was not prepared to say that they got quite well. Those which improved began to do so after about six injections or earlier. He did not think now that it was necessary to go beyond a dosage of 0·3 of a grm., a dose that could safely be used for out-patients.

Dr. H. MACCORMAC said he had treated about thirty cases of lupus erythematosus and lupus vulgaris with these preparations, using both krysolgan and sanocrysin. On the whole the results he had obtained were disappointing, although in some cases apparent cure had been effected. He believed it important – although this had not been mentioned in the discussion—to employ some form of local treatment while the injections were being given, and his best results had been obtained in this way. He also had on several occasions experienced alarming reactions, from which the patient had made satisfactory recovery, apparently without any subsequent ill-effects.

Dr. S. E. DORE said his own experience with these preparations had been too small to make his remarks of any value to others. He had seen two cases definitely made worse by the treatment.

Dr. A. M. H. GRAY said much stress had been laid on the difference between the two drugs concerned, and not very fairly, because it was obvious that those who gave krysolgan gave it in doses up to 1·5 of a grain, i.e., one-tenth of a gramme, whereas sanocrysin was given up to ten times that dosage. He had not had experience with sanocrysin. He had used krysolgan and also triphal. During the last two years he had treated practically all his cases of lupus erythematosus, both in hospital and in private practice, with one or other of those drugs, and he had seen little difference in the action of the two. Dr. Goldsmith, he

believed, considered that triphal was the more potent of the two. So far, he had not seen toxic manifestations, certainly nothing more severe than slight malaise which had cleared up when treatment had been withheld for a week or two. The majority of the cases improved considerably, and in some the eruption had disappeared. Unquestionably there was a tendency to the recurrence of the disease, and the treatment needed to be repeated. He had not, so far, given larger doses than those recommended, but thought further benefit might follow an increase in the dosage, though there might also be additional risk. Some of the cases, he believed, were cured, though it was as yet too early to be certain of that. He had not seen any other form of treatment in lupus erythematosus which could be compared with this gold treatment. Sometimes three or four courses were required before the disease was finally cured.

Dr. HALDIN DAVIS (in reply) said that sanocrysin was an inorganic compound, namely gold-sodium-thiosulphate, while krysolgan was organic, the gold being attached to a benzine ring in company with amino groups.

Experience with these preparations showed that the prognosis in cases of lupus erythematosus was now much more hopeful than it was a few years ago. Whether they would prove of much value in lupus vulgaris was more doubtful.

As to the disasters reported with sanocrysin, he thought that some might be due to the doses having been given in too quick succession. He liked to see some redness take place in the lesions without any general reaction. Great benefit usually followed the subsidence of local reaction, and he waited until the effect of one dose was exhausted before giving another. Sometimes the interval was as long as a month. He was aware that it was a potent drug, but he used it in the dosage recommended for pulmonary tuberculosis. If it could be given in gramme doses to patients suffering from that disease, those afflicted with lupus erythematosus should be able to tolerate the same amount.

Lichen Plano-pilaris.—H. HALDIN-DAVIS, M.D.—This patient is a boy with an extraordinary eruption, for which he has been invalided out of the army. It is widely spread, but irregularly distributed over the trunk and limbs and is invading the face. To some extent it resembles lichen planus, but where it has partially faded it has left some follicular pigmentation. The suggestion has been made that it may be secondary syphilis.

Subsequent Note.—The Wassermann reaction has been found to be negative. The patient has general glandular enlargement. The blood-count shows over six million red-blood corpuscles, but only 70 per cent. hæmoglobin; a few myelocytes are present.

Pityriasis Lichenoides et Varioliformis Acuta.—A. M. H. GRAY, C.B.E., M.D.—In November, 1926, I showed a boy aged 13, with an eruption which appeared to correspond to a number of cases which had been published mostly in Vienna. The original case was published by Mucha in 1916, and since that date three have been described by Rusch, two by Oppenheim, one by Krüger, two by Habermann and one by Almkvist. Cases have since been published by Haxthausen and Buschke.

Almkvist, who has written a critical survey of the cases published up to 1926, summarizes the characteristic features of the disease as follows:—

(1) A papular eruption, which more or less resembles a papular syphilide or a pityriasis lichenoides chronica, sometimes itches but does not always exhibit this symptom. (2) Among the papules there may occur more inflammatory papules, surrounded by a red halo, sometimes vesicles or pustules, necrosing in the centre, which when the crust falls off leave varioliform scarring—resembling a form of malignant syphilis. (3) In comparison with ordinary pityriasis lichenoides it runs an acute course, the skin changes subsiding in six weeks or, at least, after some months, with no special tendency to recur.

The case I showed previously ran a course of about six months, and has had no recurrence.

The patient I show to-day is a police constable, aged 31, who came to University College Hospital September 27, 1928, with an extensive eruption on the trunk and limbs. He stated that the rash had first appeared suddenly in the night some fourteen days previously, on the chest, abdomen and thighs. There was no itching, no sore throat and no feeling of malaise. He had been, and was otherwise, in perfectly good health and was continuing to carry on his duties. He gave no history of previous venereal disease and denied any recent exposure to infection. On examination he presented a symmetrical, thickly-set eruption of oval pea-sized macules and papules on the trunk and limbs. These lesions were most numerous on the abdomen and chest and the flexor aspect of the limbs and front of the axillæ; three or four only were present on the face. The individual lesions varied considerably in appearance, some were of pale pink colour, others pinkish-brown, while others showed miliary hæmorrhages into them, giving them a dark brownish-red coloration. A few showed a flaccid bulla, while others were covered with a dark hæmorrhagic crust.

The cervical, axillary, epitrochlear and inguinal glands were distinctly enlarged. The throat was slightly congested but there was no other abnormality in the mouth. There was no evidence of an old or recent primary sore.

The patient otherwise appeared in robust health.

The Wassermann reaction tested at the first visit proved negative. On October 3, an intravenous injection of 0·3 grm. N.A.B. was given, and October 10 another Wassermann test was made and also proved negative. The injection did not appear to influence the eruption in any way.

Since the first attendance, further lesions have appeared and many of those previously present have undergone necrosis. Some scabs have fallen off, leaving varioliform scars. A considerable number of lesions have now appeared on the face.

It has not been possible so far to make a biopsy but this will be done later. Nor have tuberculin tests been made. In the last case shown by me, the boy gave a definite positive tuberculin reaction and some suspicious infiltrating shadows were seen by X-ray examination in the upper part of both lungs.

This case was originally thought by the patient's doctor to be one of varicella, as was the case with my previous patient, but both this and variola can be excluded by the type of lesion and by the duration of the affection. The case bore an extraordinary resemblance to a malignant secondary syphilide except for the fact that the patient was not at all ill.

Punctate Melanodermia.—H. CORSI, F.R.C.S.—Patient, female, aged 25. The condition started six or seven years ago as a dark spot and has gradually spread. There are no symptoms.

On the left shin is a thyroid-shaped area 4 in. by 5 in., consisting of purplish brown spots which over most of the area have coalesced to form a uniformly pigmented surface. This is definitely depressed below the level of the normal skin. The central portion appears as if missed out by the disease process.

Wassermann and Sigma reactions negative. Von Pirquet: doubtful positive.

Pyodermatitis Vegetans.—G. B. DOWLING, M.D.—The patient, a man aged 50, has had for four months a vegetating granulomatous lesion, about 2½ in. in diameter, on the back of the right wrist. It is said to have followed a series of boils in the same area, and apparently developed from one of them. The clinical appearances resemble those of lupus verrucosus; trichophytosis and granuloma pyogenicum have also been considered as alternative diagnoses. A biopsy has been made, and a guinea-pig inoculated with part of the excised portion. Histologically the appearances are those of a vegetating granuloma with intra-epithelial abscesses; one giant-cell has been found, but no caseation or other tuberculous formation. No tubercle bacilli have been seen in stained sections. Cultures of the pus from the

lesion gave growths of staphylococci and *B. pyocyaneus*. In direct smears Dr. Elworthy found acid-fast organisms occurring in groups, which he considers may belong to the streptothrix group of fungi, but it cannot be said whether they are causal or an accidental contamination. The lesion has improved under fomentations of perchloride of mercury. I consider the diagnosis to be granuloma pyogenicum.

Discussion.—Dr. J. M. H. MACLEOD (President) said that the rapidity with which this lesion had grown, its acuteness, and the inflammatory halo around it were against the probability of its being verrucose lupus, and he thought it was most likely of staphylococcic origin.

Dr. SYDNEY THOMSON said he had seen four or five cases like this during the previous two months, and in each the bacteriology had been the same; in those cases an acid-fast bacillus had not been present. The papillomatous processes were usually more translucent and œdematous-looking than in the present case. While the laboratory report was awaited, treatment with 1 in 4,000 perchloride had been carried on, and within three weeks the condition had completely cleared up in all the cases.

Section of Epidemiology and State Medicine.

President—Dr. F. E. Fremantle, O.B.E.

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Individuality and Epidemic Disease.

By H. P. Newsholme, M.D., F.R.C.P.

(*Medical Officer of Health, Birmingham.*)

The classification of infectious diseases into definite species, while it has led to a vast extension in our knowledge of them, exposes us nevertheless to a danger which has been by no means completely avoided: the danger of regarding the particular deviation from health as defined and done with as soon as its particular classificatory tag has been attached. Yet it is conceivable that within the species of infectious disease to which it has been relegated, the individual may show special qualities capable of throwing light on the character of the whole species.

It is to this point of the individuality to be traced in particular epidemics of infectious disease that I invite your attention. I shall attempt in the barest outline to sketch the features giving individuality to a few outbreaks of infectious disease which have come to my notice, and to see whether these features can throw any fresh light on the problems of epidemic infection. In doing so I propose to use as illustrations outbreaks homely in type and almost domestic in scale, rather than to deal with widespread epidemics in which individuality may become submerged.

The first series consists of some twenty cases of acute febrile illness occurring in quick succession among girls in a private school. All the girls showed, in varying degree, sudden onset with headache, general aching, fever, sore throat and some reddening of the pharynx. The condition was at the time generally prevalent in the district; and in the school group, as in the district as a whole, it was given the label of influenza. Recovery was almost without exception rapid and uneventful within a few days of the onset. In the school group there was, however, one tragic exception: that of a girl who had an attack to all appearance similar to that experienced by her fellows, with convalescence beginning within some three days, when she was allowed downstairs. She was, however, then quickly noticed to become rather emotional; this was followed by complaint of headache. This in turn was succeeded within twenty-fours hours by stiffness of the jaw, then paralysis in sequence of the muscles of the face and of the pharynx, with salivation and failure of deglutition and rapid failure of the heart, with clogged lungs and running pulse, ending fatally within a few hours of the onset of the paralysis. The condition was labelled clinically as fulminating influenza, as acute polio-encephalitis, and as acute bulbar paralysis by as many medical observers. Autopsy revealed macroscopically no notable cerebral lesion.

Thus, in this otherwise colourless outbreak of an acute infectious condition, leaving in most instances no after-effects, one patient developed a rapidly progressive fatal involvement of the nervous system. That in itself is an arresting feature. It may or may not be significant that the patient was an only child, who had lost one parent in early life, and who had been subjected therefore to an environment discouraging balanced development; that she was intellectually above the average, giving evidence by habit spasms and by general bearing of an appreciable degree of nervous tension.

A second series consists of some forty cases of chickenpox involving boys in a private school. The chickenpox was to all appearance of the ordinary mild type, giving a colourless outbreak, except in the following particulars. In the first place, a convalescent boy, seen for a reason unconnected with the chickenpox, was noticed to be mentally dull, and slow in responding to questions; while at the same time he

was seen to have a peculiarly stumbling gait in walking, which did not appear accounted for by his previous short detention in bed. In the second place, a casual remark of the head of the school indicated that several of the boys, on returning to their classes after recovery from chickenpox, had shown a similar mental lethargy, to an extent making their masters complain that they could now drive nothing into these boys' heads. Such a statement is obviously vague, and may be of no significance; in conjunction, however, with the circumstances of the more extreme case mentioned above, the chance of some significance must not be set aside without examination. I am unable to give more than the vaguest information as to the previous degree of mental activity shown by the general group of mentally lethargic boys. Inquiry was not made at the time; but so far as could be ascertained from inquiry made some months later, the group contained some seven or eight boys, of whom one had previously been mentally backward, two "delicate," and one "of good mental capacity but lazy." In respect, however, of the particular boy who, with pronounced dullness, had a stumbling gait, other particulars were obtained. He was a boy who had been slow in mental growth, though the dullness following the attack of chickenpox was in marked exaggeration of his normal sluggishness. Further, he was a boy who had more than once been called to book at school for walking clumsily and tripping, while his mother stated that since early childhood he had always been careless in walking, not looking where he was stepping, and falling over his own feet with unusual frequency. In this case, therefore, one effect of the chickenpox on this boy was apparently to emphasize defects hitherto relatively trifling in degree.

Thus this series of cases of chickenpox, which might otherwise have been set aside as of no interest, gained a character of its own by reason of at least one, perhaps more, of its victims developing peculiarities which were exaggerations to the point of caricature of their customary physical or mental idiosyncrasies. In the one case in particular a boy of slow mentality became temporarily still slower in mind, and his gait, somewhat clumsy at any time, became for a period pronouncedly unsteady.

A third series of cases affected a single household. During a period when influenza was prevalent in the district, the members of this household were invaded in rapid succession by an acute febrile illness with onset indistinguishable from that of scores of influenzal cases around. The illness was therefore not unreasonably labelled as influenza. But the variation in its manifestation in the several members of the family was notable, though it is not unfair to assume that one strain of infection was concerned in these diverse results. In a child, aged 5, it produced transient mental irritability, and some unsteadiness in walking for a day or two; in another, aged 7, it gave naso-pharyngeal catarrh and ear-ache; in a child, aged 2, nasal catarrh and serous conjunctivitis; a boy, aged 10, developed a sore throat; one parent had pains in the joints, rheumatic in type, with stiffness of the neck, general fatigue and sleeplessness: the other parent had a sore throat and swollen submaxillary glands; a maid had a sore throat, and swollen glands in the neck and axillæ, with much depression; two cats in the house had violent suffocative coughs and great limpness.

A virus which we may presume to have been one and the same in all cases thus gave a considerable variety of symptoms in different members of this household. In several of the cases the symptoms were those which an observer knowing the individual characteristics might have foretold with considerable accuracy as centring on the most probable points of attack.

Turn now to an outbreak of epidemic disease in a community lower than man. An outbreak of acute encephalitis among dogs was recorded some two years ago by L. P. Pugh,[1] involving a large number of dogs. The majority showed typical

[1] Pugh, L. P., "Epidemic Encephalitis in Dogs," *Lancet*, 1926 (ii), 950.

encephalitis, with persistent sleepiness, ocular palsies, hiccough, changes in disposition, and "cuff" lesions in the basal ganglia. Some, however, showed conditions of a differing symptomatology, indistinguishable from nervous distemper. Thus, an infection which is to be presumed to be one and the same, gave rise in different individuals to a marked divergence of symptoms. I am given to understand further that it is the highly nervous or the spoilt dog that is most liable to manifest the severer forms of encephalitis.

Before passing to the more general discussion, I will close this necessarily incomplete sketch of a few small outbreaks of infectious disease by relating the recent experience of the head of a fairly large public school. This headmaster told me that until lately, in spite of many waves of catarrhal infections in his school, he had never had to arrange for a mastoid operation on any of his boarders; but that three such cases had now appeared in quick succession. In the second place, he commented on the fact that his school had been extensively invaded by a particularly mild form of measles requiring but trifling hospitalization; while at precisely the same time another public school, not far distant, and dealing with boys of like age and social status, had been involved in an outbreak of a far severer form of the same disease. Here we appear to have individuality, on the one hand, in illnesses occurring in a school at different periods; and on the other, in a particular species of illness when its features in two different schools at the same period of time are compared.

I would safeguard the more general discussion to which I now come by saying that no one can be more aware than I myself am of the scantiness and the sketchy character of the illustrations which have been placed before you. Any deductions must clearly be speculative: yet such speculation may be by no means without value.

The preceding examples suggest that in certain groups of cases of infectious disease, individual cases, with aberrant symptoms, arose to all appearance from an infection identical with that producing the ordinary type to which the majority conformed. There was, further, more than a hint in some instances that the individuals showing these abnormal symptoms were not themselves altogether normal and stable, but showed vagaries of temperament or of physical habit which might *prima facie* render them susceptible to the particular type of illness to which they fell victims. Is this association between specially accentuated symptoms and a special diathesis or temperament pure coincidence? Is there any basis for the opinion current among the staffs of fever hospitals that it is frequently the child of parents themselves most difficult to handle who develops the most undesirable complications: the child, that is, who besides being perhaps delicate has also been subjected to an unbalanced environment tending to physical and mental instability? The suggestion which I wish to make and to amplify is that the relation is not fortuitous; but that, on the contrary, the qualities of the individual invaded, as well as those of the invading organism, are concerned in determining the character of the symptomatology in the individual case. The extent to which the general type of the epidemic may be affected by the same factor, and the agencies which may cause variation in the qualities of the individual, will need some consideration. Is it, for example, possible for external agencies to produce a temporary physical or mental bias in a number of persons at a given time, in such a way as to give a particular colour to an outbreak which, had the same invading organism involved the same group under different circumstances, might have given an outbreak of different symptomatology? We are familiar with the odd way in which at one time influenza is of nervous type, at perhaps the next wave, of pulmonary type; how diphtheria may in one outbreak cause a far higher proportion of paralyses than in another; how scarlet fever may at one outbreak cause a relatively large proportion of cases of otitis media, in another of nephritis, and so forth. Is it

possible that the population invaded may in an appreciable degree share with the virus the responsibility for such variations; and that that population plays this part through subjection to factors definable or indefinable in the environment, using that word in its most comprehensive sense?

I am for the moment proposing to assume that the individual can, by reason of personal peculiarities or idiosyncrasies, play a part in moulding the symptomatology of the disease imposed on him by an infecting organism. In attempting to suggest a mechanism by which the individual could exert such an influence, I find myself falling back on a conception of function as habit in action. Man's mind and body function by virtue of innumerable sets of habits acquired during the process of evolution. The better grounded the habits, the more stable is the individual, the more able he is to meet the shocks of life's emergencies on the one hand, the more free to progress further along the track of development on the other. This applies not only to the body as a whole, but also to the organs, the tissues and the cells of which it is composed; each functions by means of a complex of habits, inherited from a more or a less remote ancestry, or acquired by itself. On the stability of the organ-habits, the tissue-habits, the cell-habits, will depend the capacity of these structures to react to the strain imposed on them, for example, by an invading organism.

Habits of mind and of body may be of average development; they may be poorly developed; they may have attained a delicacy beyond the normal, being interwoven perhaps with other habits in a way not yet reached in the average individual. Poorly developed habits may give way under conditions of stress, causing disorganization of that particular function. Habits which have attained special delicacy and complexity of function might be expected similarly to give way earlier than those of average complexity and stability.

Consider now the impact of a virus (in the sense of an infective micro-organism, whether microscopic or ultra-microscopic) let loose among a community containing numbers of individuals with notable weaknesses of cell-habit, and hence of function, in one direction or another:—

(1) Were the virus of no definite specificity, and of relatively low virulence, it might nevertheless cause a degree of disturbance of equilibrium, and produce stress, sufficient to upset the most faulty cell-habits of a number of the most unstable members of the community. This would be represented by an increase in general sickness and mortality, showing no marked trend in any one direction, since the faulty cell-habits of such a mixed community might be regarded as distributed with average evenness over a full range of organs, tissues, and cells.

(2) Assume now that the infecting virus, instead of being colourless, had a specific bias towards a particular symptomatology, but only a relatively feeble virulence. (By a virulent virus I imply a virus capable of breaking down even relatively stable cell-habits in the person attacked.) Such a virus would cause breakdown of equilibrium chiefly in those of the most unstable members of the community who had too feeble or too delicately poised cell-habits in respect of functions coming particularly within the ambit of action of this specific virus. Sporadic cases of *specific* infectious disease would result.

(3) The greater the virulence of the virus, the larger the proportion of the population likely to be made victims, and the more widespread the epidemic of specific infection. Even those persons with more stable cell-habits in respect of functions liable to disturbance by the virus, might now feel the stress sufficiently to become disequilibrated and subjected to the disease.

Thus the virus may be regarded as on the one hand bringing to light faulty cell-habits in the individual infected, and on the other having the particular symptoms towards which its specific bias trends, underlined and thrown into relief in members of the community whose faulty cell-habits are in relation to cells rendering them specially liable to a like defect of function.

But it is possible to imagine factors other than a virus of a particular type bringing to light the individuals with corresponding weaknesses of cell-habit. The habit of cell-function might be disturbed not only by an invading virus, but by conditions of the environment—heat, cold, humidity, continuous wind, and so forth —which by imposing strain on the system might dislocate the weak cell-habits of the most unstable members of the community, in numbers growing with prolongation of the period of stress. Exposure to an infection at the later stages of such a period of environmental stress might act as the last straw in breaking down the resistance and in showing up the particular weak cell-habits under greatest tension, while adding to the list those cell-habits now subjected to special strain through the specific qualities of the infecting virus. Moreover, the agency causing stress might be related to the mental equally with the physical environment. Prolonged emotional stress in the individual, the suspense and tension involved on a national scale by war, by industrial strife, by widespread plagues or catastrophes, might so impose a strain as again to render the community fatigued, and thus make the weaker cell-habits of the more unstable members of that community specially liable to break down when exposed to attack by a virus; particularly if that virus by reason of its specific symptomatology tends to attack the weak cell-habits in question, whether these are of the fatigued nervous system or of the somatic system worn down in its turn.

Finally, it is possible to imagine that certain types of environmental stress may accentuate particular weaknesses of cell-function in the population affected, giving a bias in favour of a particular symptomatology if disequilibrium passes into disease. It is possible to imagine, for example, that prolonged tropical heat might give rise to a set of weakened cell-functions rather different from that produced by extended emotional strain—the former conceivably causing, by its disturbance of peripheral cutaneous sensibility, an alteration in spinal and splanchnic reflex control and cell-function, the latter perhaps giving a disturbance of balance between the intellectual and emotional functions, and rendering the individual liable to infections of the highest parts of the nervous system.

We have reached the following point in our speculation: that the individual through his own idiosyncrasies may emphasize, and may colour, the symptomatology of the infection imposed on him: that the more pronounced the virulence of the virus, the less the relative importance of the individual's idiosyncrasies: and that these idiosyncrasies, arising from too weak or too delicately poised habits of cell-function, may become actual instead of remaining potential, through the agency of a variety of physical and mental factors in his environment.

Let me try to move a step onward in the speculation by seeking out its bearing on two points: (*a*) the pre-epidemic rise in general invalidity and mortality, but with little or no specific bias, and (*b*) the change, as the epidemic becomes declared, from the frequently anomalous cases of the earliest stages to the steadily clearer and more definite, and often more infectious, cases as the peak of the outbreak is approached: the progression, in other words, towards an increasing specificity and often an increasing virulence, with emergence and growth of the epidemic.

We have seen that a pre-epidemic rise in general invalidity and mortality might be explained—though this is not necessarily the only possible explanation—by the influence of a virus of feeble specificity, causing disorganization of equilibrium of the most unstable members of the community, bringing to light weak cell-habits which, in the absence of selection through a specific bias in the infecting virus, range over the whole gamut of invalidity to which the community is subject. A predominance of a particular type of invalidity might, on such a view, depend on some degree of specific action by the virus, or on some degree of direction by factors in the physical or mental environment of the patient.

How is the increase of specificity with progress of the outbreak to be explained? We have seen that, from a speculative standpoint, the virus may be conceived as producing a particular symptomatology, not only because of its own specific bias,

but also because the effect is reinforced by weak cell-function of the particular tissues most subject to attack by it, in the members of the community who develop the most marked and definite attacks of the disease. Does not the increase in specificity with progress of the epidemic suggest that the bias of the virus has somehow been increased and made more specific by its interaction with the individual? Is this a natural assumption, and is it supported by any experimental evidence?

First, as to whether the assumption is a reasonable and natural one. The body cells have acquired, under the process which we label as immunity, the capacity to react to, to be specially sensitive towards, an invading micro-organism and its products: their specificity of reaction to that micro-organism is increased by the fact of previous contact with it. But the body cells are essentially of the same stuff and of the same ancestry as the bacterial cell; and that which is true of one might reasonably be expected to be true of the others. We might expect on theoretical grounds that the bacterial cell, which by its initial bias is already sensitized against particular body cells, by contact with these might become more sensitized, with a keener reaction against the corresponding cells when next brought into contact with them in a subsequent host; that is to say, it might be expected to develop increased specificity of action with progress of an epidemic. Is there any experimental evidence of this? Let me quote the following summary by the late Professor Adami of certain American work, though prefacing it with the observation that, so I am informed, these experiments have not found favour with the majority of bacteriologists, although somewhat similar results have been obtained by a few other observers:—

"... As Professor Welch, of Johns Hopkins, pointed out some years ago in his Huxley Lecture, there is evidence that if man can adapt himself to altered environment, so can microbes; that if man can protect himself against the toxic action of the bacteria with which he is surrounded, so on their part bacteria can protect themselves against man by increasing their virulence and altering their properties according to circumstances.

"It is in this connexion that the recent series of studies by Professor Rosenow upon the elective localization of streptococci are of extraordinary interest. Briefly, by employing deep tubes of fluid culture medium, which permit streptococci to grow at the oxygen tension most favourable to the particular strain, and by inoculating young growths which still retain the final qualities impressed on them by development in particular tissues and localities, he has been able to show that strains obtained from the gall-bladder in cases of cholecystitis, when inoculated intravenously into animals of the laboratory, are peculiarly apt to localize themselves in the walls of the gall-bladder and there set up cholecystitis: isolated from the joints in man, to set up arthritis, from duodenal ulcers to set up gastric and duodenal ulcers, from multiple neuritis in man to be recovered from the nerve-trunks in the inoculated animal, in myalgia from the muscles. Growth, that is, in a particular tissue in man endows the streptococcus with an elective affinity for the same tissue in other animals. With the abundant material to work upon, first at the Memorial Institute, Chicago, and later at the Mayo Foundation, Rochester, Illinois, injecting various strains of streptococci into animals of the laboratory, and making routine cultures from various tissues, he found that the average incidence of the cocci in or obtained from muscle was 27; with cultures from cases of myalgia this rose to 93 per cent. Cultures of cocci are rarely obtained from the spinal cord of the inoculated animals (4 per cent.); thirty-six animals inoculated with cultures from a case of sporadic anterior poliomyelitis yielded 78 per cent. of positive results, i.e., of growths of streptococci obtainable from the spinal cord. With these examples before us is it possible for medical men not to believe in 'direct adaptation'? According to the environment so do bacteria assume special properties."[1]

I have no comment to make on the statement that this work is not viewed favourably by the majority of bacteriologists, except that the agreement between the experimental results and our theoretical deductions during the preceding discussion

[1] Adami, J. G., "Medical Contributions to the Study of Evolution," 1918, p. 43.

makes me hope that the investigation will be tested further, without favour and without prejudice.

The speculation which started from a consideration of the peculiar features of a few small outbreaks of infectious disease and of the peculiar features of the individual victims most markedly affected in those outbreaks, has led to a recognition of the possibility of specific infection involving two factors; a specificity depending on the invading micro-organism and a specificity depending on the qualities of the individual attacked—qualities of defect, partly inherited, partly produced, more or less temporarily, by variations in his physical or mental environment. The more virulent the virus, the more it will be able to impose its specific bias on the host, and the less it will depend on the idiosyncrasies of the latter for the production of a result capable of recognition as a picture of a specific disease. The less virulent the micro-organism the less it will impose its specificity and the more it will be dependent on the particular bias imposed on the invaded community by conditions of the environment. If this were so we should expect careful history-taking in the sporadic case, suggesting mild virulence, to give a clue to personal factors which provide the necessary weakness of cell-function allowing the virus to become effective. The point is not one on which much evidence can be provided, merely from lack of regard, hitherto, of the possibility. I can quote only a series of ten cases of acute anterior poliomyelitis, occurring sporadically over a period of some four months, and merging into a period at which the disease became somewhat prevalent, though scarcely of epidemic proportions. In these sporadic cases the history of personal or of inherited idiosyncrasies was noted in brief. Of the ten cases, four showed in notable degree a prior neurotic element, either in the patient or in his parents; two followed on severe falls, causing great nervous irritability, in young children; one was a mongol, with marked deficiency, therefore, in the cell-function concerned in the highest nervous control; one with polio-encephalitis had complete deafness, and this child had previously had altogether exceptionally acute hearing—that is, an over-delicate habit had been dislocated; while one case showed an unusual degree of over-control by a stubborn will, which refused to acknowledge the fact of illness. Thus nine out of the ten of these sporadic cases of anterior poliomyelitis showed initial weakness, or else over-development, of nervous cell-functions which might have a bearing on the development of a specific bias arising from an infection by itself insufficiently specific to produce a clear epidemic of this particular disease in the community as a whole.

I will close with only one further reference to the transformation not uncommonly to be noted, from an indeterminate into a specific infection with the progress of an epidemic. The hypothesis which we have formulated would allow us to conceive of such a transformation taking more than one course, according in part to the initial bias of the virus, in part to the influence of the population infected: in the latter either by an habitual bias, or by a more fugitive bias imposed by the environment, physical or mental, of the moment. Thus an epidemic which started as a comparatively indeterminate brand of "influenza" might conceivably be converted into one of "poliomyelitis" or of "encephalitis" by reason of the special bias of the population at the time of the initial invasion, according as the potential rather than the actual specific quality of the virus at the beginning of the outbreak became emphasized by successive action in a number of hosts over an ever-increasing range. The direction taken—that is, the type of the epidemic—would in such event be determined by the resultant of the original bias given by the infecting virus, and of the bias existing in the invaded community.

The limits inherent in such a paper prevent any fuller discussion of the problem at issue. I hope that it may at any rate have fulfilled in some degree its purpose of drawing attention to the clues towards a fuller apprehension of disease processes which may still remain to be unravelled, from the details even of the more modest outbreaks of infectious disease.

Discussion.—Sir WILLIAM HAMER said: Exceptional happenings force unusual effects of infection upon our attention. Thus, in the fortnight after a Christmas party, in 1902, nineteen of the twenty-two persons who had been present succumbed to influenza. The same virus was presumably at fault, yet the symptoms were widely varied. The experience prompted an analysis of records of illness among members of a family living in London over a period of fifteen years, and this revealed seven examples of outbreaks of a similar type to that noted above. All these outbreaks occurred at times when influenza was epidemic in London, and here again the symptoms showed great variation. In the winter of 1904-5, Dunn and Gordon's "Epidemic simulating influenza" occurred. I attended a Conference of Medical Officers of Health of East Herts, at which the varied manifestations of influenza in this outbreak were discussed. I can therefore appreciate the force of the remark recently made here by Professor Boycott: "Brought up as we all have been in the heyday of bacteriology it is a little difficult for us to get an unprejudiced view of the situation." A paper such as that now under discussion is, however, a great help, and it lures us on to the study of Creighton's "History," of the "epidemic potential" of Peters and Goodall, of the "velocity of infection" of Sheldon F. Dudley, of the "quantum theory" of Lieut.-Col. Gill, of the "epidemic waves" of Ross and Brownlee, of the Ministry of Health Report, of the Essays of Crookshank and his collaborators on both sides of the Atlantic, and of the experimental work of Topley and Greenwood. All these workers owe much to Sydenham, and Dr. Newsholme now enrols himself in the brotherhood, for his "reciprocal action between virus and victim" was the text of the "Medical Observations" and his "gradual emergence of more specific types of disease" might well form a subject-heading for paras. 54-57 of Sydenham's Letter to Dr. Brady. Dr. Newsholme's paper is most stimulating and suggestive.

Sir GEORGE BUCHANAN said that a philosophical and speculative paper of this kind should be considered with such parallel speculations as those of Professor Boycott in his recent presidential address to the Pathological Section, or even with those expressed by Mr. Rudyard Kipling at the Society's recent Annual Dinner. Generalizations about the group of diseases we arbitrarily class as infectious were usually perilous, and this was illustrated when an explanation was sought for particular individuals being severely attacked or having unusual complications. If the epidemic was small-pox or measles, the natural or artificially conferred immunity was the dominant, and almost the sole, consideration. The infected person might be exposed to tropical heat or arctic cold, have a "nervous make-up," or any other idiosyncrasy, be starved or shell-shocked; it was all of no consequence if he had his immunity. By contrast, in the case of a disease like pneumonia, such circumstances were all-important. In the nervous system infections such as encephalitis, poliomyelitis and cerebro-spinal fever it was a perpetual puzzle to know how, when the infection was passing through the whole community and leaving almost every one little or none the worse, a small minority of individuals were heavily hit and their illness alone demonstrated the general infection which was about. He doubted the explanation that these stricken individuals had a specially unfavourable environment or must have had some exceptional exposure or some pre-existing nervous complaint. In each of these three diseases there were endless reports and records of attacks in children or adults who had been in first-rate health, and were of good physique, and mentally normal and stable. Was not an anatomical explanation quite as likely, e.g., that the individual had exceptionally large sinuses or other peculiarities that enabled a local infection of nose, pharynx or middle ear to spread rapidly and massively to the central nervous system?

The recent inquiry into post-vaccinal encephalitis was instructive in this respect. Here there could be no question of vaccinia, if vaccinia was regarded as the infecting agent, having become intensified or changed by passage through particular human contacts and carriers—there was no human passage at all. Nor was it a case where a particular vaccinia strain had been altered by passage through some particular calves or other animals, as the post-vaccinal encephalitis was associated with various vaccines of quite different origin. It was difficult to explain why only one out of many thousand vaccinated children was affected by this condition, and then only in particular geographical regions. The Departmental Committee, seizing on the facts of its local incidence and other considerations, had been driven to the hypothesis of the coincidence of vaccinia with a normally harmless, unknown, virus infection, one which probably would be capable not only of determining encephalitis when it coincided, at the appropriate stage, with vaccination, but also when it occurred simultaneously with any acute infection. If this kind of assumption could be accepted, for working purposes, to explain the form of encephalitis, it might also be invoked to furnish a reason for some of the cases to which Dr. Newsholme had alluded—at least it seemed as good a speculation as any other.

War Section.

President—Lieut.-General Sir MATTHEW FELL, K.C.B.

[*November* 12, 1928.]

Severe Malaria among British Troops in the East African Campaign.

By W. D. KEYWORTH, M.D. (Major, I.M.S.).

ABSTRACT.—(1) A study of the East African Campaign shows how helpless we are on field service as regards prevention of malaria in a malarious country. (2) Not only prophylaxis but also the symptoms, diagnosis and treatment of the disease are modified by the conditions of field service. (3) The accounts given of the disease in its different aspects do not represent any scientific advance but are included merely to illustrate the points considered under (1) and (2) and to show the responsible and difficult nature of the work that medical officers on field service may be called upon to do.

The problems presented by malaria on field service are quite different from those under peace conditions. Although it is now over nine years since the end of the Great War, certain peculiarities of the East African Campaign, worthy of careful study, have never been discussed. The official medical history of the campaign is very meagre, and few other articles have been written. I was on field service in the country for eighteen months and was, during a great part of that time, attached to some of the larger hospitals situated on or near the East African coast. I was thus able to see the conditions of shipping and disembarkation, as well as to treat the men when they returned to the coast, having left their units inland on account of sickness.

The chief points to which I wish to draw attention are:—

(1) Field service conditions are incompatible with the taking of adequate precaution against malaria.

(2) In the East African Campaign there were several factors which contributed to the outbreak of malaria; these were the equatorial climate, the long and malarious coastline, numerous ports, and the fact that the troops were unused to tropical warfare and tropical conditions.

(3) The incidence of malaria was extremely heavy and the general health of the troops was, in consequence, poor.

(4) The large number of quinine-resisting cases, of cases showing intense malarial infection, and of cases of various severe types of malaria.

(5) The poor facilities for scientific diagnosis compared with the vast amount of clinical material available.

Although East Africa was one of the subsidiary theatres of war, about 300,000 allied troops were employed there. In the matter of sickness ratio, East Africa was by far the worst of all the war areas.

The admissions for disease of all kinds were 2,880 and 1,278 per 1,000 in 1917 and 1918 respectively, the former figure being several times higher than the figure for Macedonia, which area shows the next highest admission rate.

Malaria was the chief cause of disability and accounted for 57 per cent. of the total admissions. Between June, 1916, and October, 1917, there were no less than 107,000 admissions for this disease. Apparently figures are not available for the whole period of the war, but probably the total admissions for malaria amounted to at least a quarter of a million, 100,000 of these occurring among British troops, while African and Indian troops accounted for the remainder. On the average, every European was admitted to hospital two or three times annually on account of malaria.

Disability due to the disease was, I found, much greater than the number of admissions shows. Many men ignored slight attacks of fever and only came to hospital when a more severe attack occurred. Others spent a fortnight in hospital followed by a few weeks on duty, and this process was repeated, and varied with an occasional trip to Nairobi or South Africa, till the man was finally sent to England as an invalid. The majority were not so much upset in health as in the cases quoted, and, even though they had malarial attacks from time to time, their general health was but little affected. This paper constitutes an attempt to explain the causes of the appalling incidence of malaria in this campaign.

Climate.

The part of East Africa with which this article is concerned lies in the equatorial zone roughly between the latitudes 5° and 15° south. The climatic conditions on the coast, therefore, are similar to those found on the West Coast of Africa, at Singapore, and on the Isthmus of Panama, though these areas all lie north of the equator. These coastal areas are all similar in this respect—that there is a warm steamy climate throughout the year with very little seasonal variation of temperature. As the temperature is always above 60° F., the breeding of anopheline mosquitoes occurs all the year round. Ample facilities for the breeding of mosquitoes exist in the neighbourhood of the various ports, as these are of necessity situated at the mouths of rivers and creeks whose shores are fringed with mangrove swamps. On the coast no extremes of heat or cold are found, the maximum day temperature, throughout the year, being about 100° F.

In East Africa the larger hospitals, for obvious reasons, were situated at these unhealthy ports.

Military Operations: Prophylaxis.

During 1916 and 1917 the main German column which had been operating in the north-east corner of their territory, gradually retired towards their southern frontier. In November, 1917, the column crossed the River Rovuma, between Portuguese and German East Africa, and by June, 1918, had penetrated into Portuguese territory as far as the neighbourhood of Quilimane. Having attracted our forces thither the German Commander turned his steps northwards again, and, in September of the same year, he re-entered German territory. In November he invaded Northern Rhodesia and a few days later he surrendered in that country under the terms of the Armistice.

He thus compelled us to keep ports open, for disembarkation of troops, along nearly 1,000 miles of coastline. His object, of course, was to keep engaged in Africa as large an Allied force as possible and thereby indirectly to influence the course of affairs in Europe. In this project he, from his point of view, admirably succeeded. If we view the situation from the sanitary standpoint, malaria became, in a sense, his best ally.

The usual method by which we reinforced our troops was as follows: British troops were sent out from England by the Cape route to Durban in transports plying between England and that port. At Durban these troops went into camp to await onward transport to Dar-es-Salaam, or other East African port. The next stage of their journey was in transports plying up and down the East African coast, between Durban and Dar-es-Salaam, and putting in as required in other ports, viz., Mombasa, Zanzibar, Kilwa, Lindi, Port Amelia, etc. These coasting vessels were actually old "tramp" steamers which had been adapted to the purpose of transporting troops. They were small, ill-equipped and always overcrowded.

Troops travelling in them suffered extreme discomfort throughout the voyage. Beds or cots were not fitted, so that the men could not use their mosquito nets; frequently they had not even brought mosquito nets. All they could do to obtain rest was to lie down on the floor of the various troop decks, and often they had not room to lie down at all.

These coasting vessels were never out of the tropical regions, except at the southernmost part of their voyage, and even the climate of Durban is hot for several months of the year. The steamers were mostly employed in the area between 5° and 15° south latitude. South African troops were also conveyed from Durban to East Africa in these vessels. Indian troops after reaching Dar-es-Salaam from Bombay and African troops from Mombasa and Zanzibar, were similarly shipped to their final port of disembarkation in these vessels, which were thus utilized for British, Indian and African troops as occasion required.

Dar-es-Salaam, the capital of the country, became our headquarters and base of operations, but from time to time another port might become an advanced base. Reinforcements thus camped at Dar-es-Salaam and often visited several other ports before they were able to leave the unhealthy coastal area and join their own columns in the African bush. In this way other ports as far south as Beira were opened up, often at short notice. The arrangements there were necessarily imperfect and the military situation naturally did not allow extensive camps being put up or ports rendered mosquito-free before the troops arrived. Port after port in Portuguese East Africa became actual or virtual "advanced bases" in rapid succession, and no one could possibly tell when a port partially given up might not suddenly be brought into use again.

During all this time, whether in steamers or in camps situated on the coast, the risk of the men becoming infected with malaria was very great. Indeed, it would be surprising if they escaped infection on shore, as there they found themselves in close proximity to an African population infected with, though relatively immune to, malaria. Even when men are living in buildings, under settled conditions, can speak the language, can have beds and mosquito nets and can rest during the periods of mosquito activity, the prevention of malaria is difficult and often fails. When troops are on the move and "kit" is cut down to the minimum, the difficulties are almost insuperable and sooner or later infection is sure to occur.

The following instance shows how troops may become infected while on board ship.

In December, 1917, a transport arrived at Lindi, with reinforcements which had come from England via Durban in the manner already described. Some of the troops went into camp at Lindi, while others proceeded inland next day to join their units. Of the former, some were brought to hospital three days later and found to be suffering from severe primary malaria. Of those who proceeded inland, a journey of several days' duration, many never reached their units, or, if they did reach them, were passed back, suffering from malaria, through the various hospitals, till they rejoined their fellow travellers from the same ship in hospital at Lindi. Undoubtedly a proportion of men from this ship failed to join their units; some had to be sent to Nairobi, some to South Africa, and some even to England. These men had never been in the tropics before the incident mentioned. The ship in which they had travelled from Durban had been anchored for two nights in Kilwa harbour, but the troops had not been allowed ashore. The explanation which was accepted at the time to account for their infection, was that the ship had been visited by anopheline mosquitoes while it was anchored in Kilwa harbour. This is a possible explanation, but it assumes an incubation period of only five days. It seems to me much more probable that the troops were all infected on board ship while on the voyage from Durban, in spite of an issue of quinine at the rate of 10 gr. daily to every man.

The following is an instance of a unit evidently infected while on shore, in spite of all the usual precautions. The unit referred to consisted of the entire staff of an expanded British casualty clearing station. Quinine in solution, at the rate of 10 gr. daily, was given on parade to every member of the unit, and this had been carried out from the day that the unit had arrived in the country. The use of mosquito nets was also rigidly enforced. Breeding places of mosquitoes were dealt with as far as possible, and all the usual precautions were taken. In spite of these

measures there was a great deal of malaria among the members of the unit, and, further, among the individuals affected, the malarial attacks usually proved most resistant to quinine. Many men of the unit were sent as invalids out of the country, owing to the fact that quinine did not control their malarial attacks at all, even while they were under treatment in hospital. Accordingly, I examined the blood of every member of the unit (120) by the thick-drop method and found malarial parasites present in 75 per cent. In fact, practically every man was suffering from malaria and spent his time alternately between duty and hospital. Once such a stage of infection is reached, it becomes impossible to distinguish between quinine treatment and quinine prophylaxis. Attempts at preventing malaria by the administration of quinine are now considered to be useless by most authorities, but, at the time of the war, great reliance was placed on this measure. It is now generally supposed that quinine given for this purpose fails to prevent malaria because it does not act on the trophozöite form of the parasite.

It is evident, therefore, that our troops, in spite of any precautions we were able to take, suffered severely from malaria.

Notwithstanding all that is known about malaria, we have as yet no simple and reliable means of preventing the disease. The means at our disposal at present are imperfect and uncertain. Our last line of defence is the mosquito net, but even this is not infallible and in any case cannot be used throughout the entire period of mosquito activity. Under field service conditions many necessary measures cannot be carried out, and attempts at malaria prevention naturally fail, as they did in the campaign under consideration.

Microscopical Examination of the Blood.

Facilities for obtaining a diagnosis by laboratory methods were crude in the extreme; this state of affairs, also, can be traced to the facts that East Africa was regarded as a subsidiary theatre of war, and that the distance from England was so great. At Lindi, for instance, there was a staff of ten medical officers for the care of 1,500 sick in hospital. There was only one microscope, and no means of doing cultural work or carrying out agglutination tests, except by sending specimens to Mingoyo, a distance of ten miles by river. At Mingoyo was situated a mobile laboratory where simple cultural work could be done, the staff consisting of one medical officer and one African attendant. In advance of this point, along a line of communications about 200 miles in length, there was, I believe, not a single microscope, and consequently the diagnosis of fever cases was made on purely clinical grounds,

It is admitted that laboratories fitted out for cultural work are not, on the whole, mobile units, and unless they are accommodated in huts or buildings the results are not likely to be satisfactory. The roads that existed in Africa were far too rough for motor laboratories, and in the area in advance of the roads columns usually moved on foot along narrow paths through the African bush, where a motor vehicle could not follow them. The examination of the blood for malaria parasites, on the other hand, can be carried out if necessary in a small tent, but even for this work a building is a great convenience. The need of such units as the malaria-diagnosis units provided in Palestine, was badly felt in East Africa, and, owing to the lack of such facilities there must have been thousands of mistakes in diagnosis. The difficulty of examining blood-films, or blood-drops, under field service conditions can, I think, be exaggerated. The Germans, for instance, regarded a portable microscope and simple blood-stains as an essential part of the equipment of their column, and their medical officers were usually able, even in the African bush, to examine the blood of fever cases when they reached bivouac for the night; and they showed their equipment to us at the end of the campaign.

The sick were brought down daily by river convoy to Lindi, and those requiring transfer were sent on to Dar-es-Salaam by hospital ship about once a week, there

being a complete turnover about once a fortnight. The clinical material was vast. I endeavoured to do the microscopic work of a large hospital in addition to my own clinical work (100 beds). The microscopic work was often done at night with the aid of a hurricane oil lamp, as this was the only means of illumination. Only the simplest stains were available. Blood was examined by the thick-drop method and diluted Giemsa stain, diluted Leishman's stain, or even diluted methylene-blue being used for both dehæmoglobinization and staining. Even under these difficulties I was able to examine from twenty to thirty slides daily. I had had in India several years' experience of the thick-drop method of blood examination, and thus was able to appreciate its advantages and its fallacies. I found that the use of this method saved time and facilitated diagnosis, especially in cases where quinine had already been administered. Further, the difficulty as regards strict cleanliness of slides and reliable methyl alcohol was so great that the use of the thin films was given up, except for special cases where a blood-picture was required.

Benign tertian and subtertian parasites occurred in about equal numbers, and parasites were usually found in the first field or first few fields examined. Heavy infections were common, the slides showing from forty to fifty parasites in every field under the $\frac{1}{12}$th in. objective; specimens showing anything from two hundred to four hundred parasites in each field were met with occasionally. It was noted that men whose peripheral blood contained large numbers of parasites were often anæmic and debilitated, but that they did not always present acute symptoms. Presumably, if left undiagnosed, from lack of blood examination, and therefore untreated, such men would sooner or later become gravely ill. On the other hand, in the cases which were clinically of a severe nature, and especially in the cases with cerebral symptoms, it was noticed that parasites were often very difficult to find in the peripheral blood, doubtless owing to the fact that they were concentrated in the capillaries of the brain or other viscera.

When we examined, by the thick-drop method, the blood of Europeans in poor health, we found either malarial parasites or a reticular appearance of the red cells which formed the background of the field. This appearance is often, incorrectly I believe, called "basophile degeneration." We came to regard this reticular appearance as evidence of latent malaria, and considered it to have the same significance in the microscopic examination of the blood as splenic enlargement has in the clinical examination. Blood-films, stained as controls in some of the cases in which this change had been observed, showed polychromasia of red cells and the presence of "brassy bodies." Unfortunately, I was not able to carry the research any further, but propose to do so at an early date. In the meantime, I suggest that this reticular appearance of red cells seen in thick-drop examinations corresponds to the appearance known as Schaudinn's figures found about malignant malaria parasites in the red corpuscles when the thin film method is used. Malaria pigment was present in such quantity in the blood-drop, in some of the severe cases, as to give under the microscope the appearance of a dirty slide. The first time I saw such a slide I discarded it on the assumption that it had been in some way powdered with fine coal dust. I then prepared a series of slides from the same case, taking especial care as regards cleanliness of slides and stain. I obtained an identical result in each slide and realized, therefore, that the appearance seen was due to pigment present in the blood.

Owing to the fact that parasites are often most difficult to find in the most urgent and dangerous cases, we felt greatly the need of some more delicate test for malaria such as culture of parasites, concentration of parasites and complement deviation; but, as has been shown, such methods were not available for us.

Diagnosis and Clinical Features.

The onset of some other illness appeared to bring about a malarial relapse, with the result that cases of enteric fever, or of dysentery complicated by malaria, were

common. The mere finding of malaria parasites was thus often misleading, as the blood was not sent at a sufficiently early stage for cultural examination of the enteric group of organisms. The cases were diagnosed as malaria, the more serious disease being overlooked. The patient might then be regarded as fit to move and be transferred to another hospital. Many such cases must have ended fatally. The following is an instance of a case in which an incorrect diagnosis was made by me in the early stage of the disease:—

In December, 1917, a young South African who had not been protected, by inoculation, against the enteric group of diseases, was admitted to hospital on account of high fever. He was anæmic and the spleen was enlarged nearly to the level of the navel. Subtertian rings and crescents were found in the blood in moderate numbers. Malaria was diagnosed and it was supposed that an acute attack had supervened on a chronic infection. Like many Rhodesians, this patient had suffered from malaria for some years before joining the East African force. Under vigorous quinine treatment the parasites disappeared from the peripheral blood and the spleen shrank to about half its former size; the temperature, however, remained high and continuous; in fact, clinically, the case became one of enteric fever, and this diagnosis (enteric) was confirmed by cultural examination of the stools in the third week of the disease.

In this case gradual recovery occurred. In other cases, in which the original diagnosis was malaria, persistent diarrhœa and high or irregular fever developed. Two such cases under my care ended fatally, and the amended diagnosis of enteric fever was confirmed at autopsy. In addition, there were numerous cases in which the fever was high and continuous for five, six or seven days. The actual malarial attack in many cases of benign tertian infection was severe—high continuous fever lasting five or six days, with vomiting and much constitutional disturbance. The cases in which the parasites of malignant tertian malaria were present were, in the absence of pernicious symptoms, often milder as regards the actual malarial attack.

Dysentery was commonly associated with malaria, but the diagnosis of the double disease did not usually present such difficulty as did the combination of malaria and an enteric group infection. Hæmorrhagic malaria may be mistaken for dysentery. In the following case I made this mistake, and, in consequence, anti-malarial treatment was postponed till it was too late to avert a fatal issue.

A West Indian soldier was admitted with symptoms of acute dysentery. His general condition was very grave and the stools, which he passed almost continuously, consisted entirely of blood and mucus. No amœbæ were found in them, and cultural examination of the stools for dysenteric organisms was not carried out, as I had no means of doing this locally. For some reason an examination of the blood for malarial parasites was not made on the day of admission. Next day an overwhelming infection with subtertian parasites was found. He was then sinking, and an intravenous injection of quinine did not save his life. It is probable that this man would have died even if his case had been correctly diagnosed immediately on admission.

Vomiting, pain referred to the spleen, and jaundice were all common symptoms. Vomiting was often so troublesome that the patient was unable to take quinine by the mouth; also, if vomiting were persistent, the patients rapidly became exhausted. A large proportion of patients suffered from pain in the splenic region during the acute febrile attack, the pain being often so severe that a hypodermic injection of morphia was required for its relief. Many cases were admitted in which a diagnosis of gastritis had been made previously, the symptoms being epigastric pain, vomiting and anæmia. Apparently, owing to the absence of fever, malaria had not been suspected, and the blood, in consequence, had not been examined. When an examination of the blood was made, a subtertian malarial infection was found, and the symptoms readily yielded to the intramuscular administration of quinine.

Acute abdominal symptoms were present in many cases both of benign tertian and subtertian infection, and the differential diagnosis from appendicitis and other acute

abdominal conditions was difficult, especially when the symptoms were on the right side. In doubtful cases an intramuscular injection of quinine, followed by careful observation for a few hours, was preferred to immediate operation. We had also to distinguish cases of dysenteric localized peritonitis, of which the following is an instance:—

A nursing orderly was admitted complaining of severe pain referred to the left side of the abdomen, and, on examination, this area was found to be rigid and tender. No splenic enlargement was made out, though the man had had several attacks of malaria. Careful inquiry elicited the additional information that a few days earlier there had been an attack of diarrhœa or dysentery so slight that the man had not sought admission to hospital. He had merely given himself a dose of castor oil and had continued at work. The stools on examination were found to contain mucus, but no amœbæ were detected in them on microscopic examination. A provisional diagnosis of peritonitis about a deep dysenteric ulcer was made, and emetine was administered hypodermically, commencing from the day of admission. During the next few days a large tender swelling developed in the left lumbar region, but this gradually subsided and complete recovery occurred.

In this case, owing to the pain being left-sided, the diagnosis was fairly simple.

Malarial attacks simulating acute pneumonia were very common, but usually gave rise to little anxiety, as the attacks quickly yielded to the administration of quinine. On the other hand, in some cases the brunt of the disease fell on the heart. During the course of what appeared to be an ordinary malarial attack, the heart suddenly failed. Cases of this type gave great anxiety and many ended fatally. Dr. L. S. Dudgeon and Dr. C. Clarke in an excellent study of the histology of malaria carried out in Macedonia, found fatty degeneration of the heart-muscles and blocking of the capillaries, with infected red blood-corpuscles.

Blackwater fever was common, but calls for no comment.

Treatment.—The sulphate and the bihydrochloride of quinine were the only drugs available for the routine treatment of malarial cases. The usual method of treatment consisted in the oral administration of 30 gr. of the sulphate daily. But when persistent vomiting was present, and when the attack did not yield to quinine given by the mouth, some other mode of administration was necessary. No one could have gone to Africa more prejudiced against the intramuscular injection of quinine than I was after eight years' military work in India. The danger of tetanus following these injections had been thoroughly impressed on me, as on every military medical officer in India. Colonel Semple's work, published in 1911, had been entirely accepted and, in consequence, throughout the Army the intramuscular injection of quinine had been strictly forbidden. As the result of Major MacGilchrist's work, we were further taught that quinine when administered intramuscularly was not absorbed and that, therefore, the method was pharmacologically unsound. Quinine, we were told, should be given orally or, in special cases, intravenously. In India, and especially while serving in the N.W. Frontier in 1915, I had seen among Indians cases of malaria not benefited by quinine orally administered, and the men so affected had usually been invalided to their homes. Such cases were comparatively few in number.

In Africa I found malaria to be a much more severe disease, and quinine, when given by the mouth, commonly failed to control it, even when enormous doses—up to 80 gr. daily—were used, as they were by some officers. While in East Africa I gave more than 3,000 intramuscular injections of quinine. At first I used ready-made quinine phials and boiled the needle and syringe, but soon resorted to preparing my own solution and sterilizing the needle and syringe by chemical means.

I found it best to use quinine bihydrochloride, 1 to 3 solution, in distilled water. I boiled about 250 c.c. of this solution in a glass flask on three consecutive days and then stored it in rubber-capped serum bottles, leaving a spare hypodermic needle pushed through the cap of each bottle to allow equalization of pressure when the bottle was reheated or solution extracted. The 2 c.c. all-glass syringe with needle was sterilized by sucking up into it first

pure liquid carbolic acid (or lysol), then methylated spirit, and finally sterilized water out of three ground-glass stoppered bottles kept for the purpose. Before a series of injections was begun the syringe and needle were left to soak for ten minutes in the carbolic acid bottle.

The injections were usually made into the buttock 2 in. or 2½ in. below the crest of the ilium; 1½ c.c., containing about 8 gr., was injected out of the syringe, the needle being pushed well into the muscles. Other sites, such as the deltoid muscle or the posterior axillary fold, were sometimes utilized.

One injection was usually given daily for the first three days, and thereafter the treatment was continued by the mouth. Another single injection was usually given on the ninth or tenth day, as at about this stage a relapse commonly occurred. Two or three times a week, according to circumstances, the carbolic acid and spirit were thrown away and fresh supplies substituted. By this method, gradually evolved, all dangers of sepsis (including exogenous tetanus) were elimininated and only the irritating or necrosing action of quinine on the tissues remained. I found that, when everything was ready, I could give about twenty injections hourly by going round from bed to bed, an orderly following me with a tray bearing the necessary bottles.

It may well be asked why sterilization by chemical means should be preferred to boiling both syringe and needle. The answer is that we found the sterilization of needles by boiling to be quite impracticable. The life of a needle repeatedly boiled is very short, glass syringes sooner or later crack, and replacement of needles and syringes was not always possible. Sterilized chemically, both remained serviceable much longer. The whole arrangement, of course, was devised solely for the purpose of giving a large number of injections daily under the special conditions prevailing.

Pain and local swelling occurred in some of the cases, and I consider them inevitable, as post-mortem findings and experimental work both show that local necrosis is produced in the tissues injected. If these symptoms are very troublesome, cold compresses are applied and resolution usually follows. The temptation to incise the swelling is great, but this should not be done in doubtful cases before palliative measures have been tried, as the wound once produced heals slowly, owing to the devitalizing effect of the quinine. I saw in Nairobi two cases showing the unfortunate effect which quinine injections occasionally have. In both cases the buttock had been incised some months before. Large gaping wounds, exposing the iliac bone, remained, and tracking into the muscles was still occurring in spite of the wound being widely open. Even at that stage further incisions became necessary from time to time. The occurrence of such cases was used, by some officers, as an argument for the prohibition of intramuscular injections of quinine; but we must set against this the opinion of most medical officers that hundreds of lives were saved by this method of treatment.

I saw no cases of tetanus following quinine injections. Operation was only called for on one patient to whom I had given an injection of quinine, and in that case no pus was found. If the patients have some focus of infection—such as severe pyorrhœa or furunculosis—I think the advisability of giving quinine by some other route should be considered, as the possibility of autogenous infection must always be borne in mind.

The value of, and danger attaching to, intramuscular injections of quinine have, of course, been the subject of discussion for many years, and a large amount of literature has accumulated. I do not propose to enter into this discussion. I only wish to point out that under the conditions of field service in East Africa, we found that the intramuscular was the best route for administering quinine to patients for whom quinine orally administered was unsuitable. Experienced officers working in other theatres of war came to the same conclusion. For the sterilization of syringe and needles chemical means were found necessary in other areas also.

Intravenous Administration of Quinine.—With this method of treatment I was disappointed. I gave quinine in this way in six cases, all of a desperate nature, and

each of the patients so treated, rapidly collapsed and died. The quinine was given in various dilutions, adrenalin being added to the solution. I can only suggest, by way of explanation, that my patients were exhausted as the result of field service and privation, and that they would probably have died whatever treatment was tried. But my impression at the time was that intravenous injection hastened the end, and consequently I discontinued it. Other writers too, are not in favour of the intravenous injection of quinine in field units, as the patients are exhausted and the cardiac condition is likely to be at its worst. Under peace conditions I have great faith in the intravenous administration of quinine, and its use for severe cases is now well established.

Rectal Administration of Quinine.—Some medical officers, who had had experience of treating malaria on the West Coast of Africa, were strongly in favour of this method. They recommended the rectal administration of 30 gr. of the bihydrochloride dissolved in a few ounces of saline or starch solution. I tried it in a few cases and was disappointed. Either the dose was rejected, or, if it was retained, no benefit accrued. The method was inconvenient and interfered with the work of the ward. We were poorly supplied with trained nurses and with orderlies possessing the necessary experience. For eighty beds, full of acute cases under my care, I seldom had the assistance of more than one nursing sister and four nursing orderlies.

Cases of Malarial Relapse.—The greater part of our time was spent in treating cases which relapsed time after time whilst actually under treatment with quinine, either in hospital or after discharge from hospital. Relapse on or about the tenth day after the onset of a malarial attack was very common, even among patients confined to bed and taking 30 gr. of quinine in solution daily. All except the slight cases were if possible kept in hospital for a period of ten days, and an intramuscular injection of quinine was given on the ninth or tenth day. Men who suffered from numerous relapses were sooner or later sent out of the infected area; but even when this was done, relapses continued in spite of treatment; this I found by observation when I was afterwards on the staff of a hospital in Nairobi.

The patients who had been treated originally by intramuscular injections of quinine suffered from relapses quite as frequently as those treated by quinine orally administered. In a proportion of cases, in short, the results of treatment were most disappointing whatever method we used. We were not supplied with any derivatives of cinchona other than the quinine salts mentioned, nor was there available a sufficient quantity of any of the organic arsenical preparations to enable us to use them in the routine treatment of malaria.

In this connexion an interesting case under the care of Major R. S. Herrick, I.M.S., is worthy of record. A British patient, while undergoing a regular course of treatment for syphilis, had an attack of malaria with severe cerebral symptoms. Subtertian parasites were found in the peripheral blood. This man at the time had already received several injections of neosalvarsan.

Cerebral Malaria.—As regards the treatment of this serious condition, we made what we thought was a valuable discovery. In the coastal area at one time cases of cerebro-spinal fever were occurring among Africans, and, therefore, lumbar puncture for diagnostic purposes was carried out in all doubtful cases of illness in which cerebral symptoms were present. The cases of meningitis were benefited or relieved to some extent by this procedure, but it was noticed that some which proved to be cases of cerebral malaria, were benefited to a much greater extent, and in a few cases the effect was most gratifying. The following is an instance of such a case:—

An Indian patient, admitted under the care of Captain Rao, I.M.S., was found to be deeply comatose and to have general rigidity. Diagnostic lumbar puncture was carried out by me at once, and at the same time a specimen of blood was taken for the examination for malarial

parasites. The cerebro-spinal fluid proved to be clear, but the blood contained subtertian parasites. An intramuscular injection of quinine was given immediately. Two hours later when I again saw the man, he was sitting up in bed and enjoying his food. He made a rapid recovery.

After this success, I deliberately tried the effect of lumbar puncture in cases of cerebral malaria that came under my care, withdrawing about 15 c.c. of fluid. In some cases the procedure was followed by improvement, and in no case was any deleterious effect noticed. I afterwards found that lumbar puncture had been recommended by other medical officers, but at the time I was not aware of this.

It has been suggested that any benefit which accrues to the patient is due to a simple lowering of the intracranial pressure. I make the further suggestion that the mechanical lowering of intracranial pressure allows a rush of quinine-laden blood into the clogged brain capillaries. It is, of course, understood that in cases with cerebral symptoms, in addition to the carrying out of lumbar puncture, quinine should be administered vigorously by any method selected. Administration by the rectum was recommended by some medical officers because, by that route, large doses (30 or 40 gr.) may be given. I found this method most unsuitable. The patients were often so rigid and irritable that an anæsthetic was required to enable a nurse to give the injection. The intravenous injection of quinine was sometimes difficult to carry out for the same reason; indeed, some of the patients were so noisy and violent that it required several men to restrain them. To one such maniacal patient I administered 16 gr. quinine bihydrochloride intramuscularly into each buttock, and in addition gave a dose of hyoscine hypodermically with a most satisfactory result.

Disposal of Malaria Patients.

Malaria is a disease in which there is a tendency to relapse. In civil life it is usually only the patient's interests that have to be considered. On field service, however, once a man has contracted malaria, it may be difficult to reconcile his interests with the interests of the force in which he is serving. In this campaign a man relapsing time after time, became little more than a passenger between his unit and a hospital or sanatorium. He thus occupied some of the already limited space in sick convoys, hospital ships and returning transports, and his presence increased the overcrowding on each of these. If, on the other hand, every man who had contracted malaria had been invalided out of the country, the force would soon have ceased to exist; each case had to be considered by itself. The disposal of malaria patients was thus a difficult administrative problem.

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Discussion.—Major J. D. KIDD, R.A.M.C., said that with regard to intramuscular injection of quinine, it was found on the arrival in East Africa of the Indian Expeditionary Force, that the officers of the East African Medical Service were in the habit of treating practically all cases of malaria by the intramuscular method. They preferred this to the oral, and were convinced that they obtained better results. Some of them even went so far as to give prophylactic injections every month or two to those living in specially malarious parts of the country.

The intramuscular method was largely adopted by the medical officers of the Expeditionary Force, with good results; so far as I remember, there were only two cases of tetanus.

Colonel J. C. KENNEDY said that, with regard to quinine-resisting cases, quinine prophylaxis and treatment, recent investigations connected with the malarial treatment of general paralysis of the insane, and in particular the evidence of individual susceptibility or resistance to infection, had helped to consolidate our ideas on these questions.

Discussing the intravenous and intramuscular methods of quinine administration, whereas the success of the intravenous method was closely related to the regulation of the dose, the occasional serious results of the intramuscular method would have to be explained by biochemical research and could only be avoided by the substitution of a preparation of quinine which would be more readily assimilable by tissue fluids than the salts in present use.

He was unable to subscribe to the suggestion that a reticular appearance of the red cells was to be regarded *per se* as evidence of latent malaria. This appearance might be found in anæmias other than that due to malaria.

Wing-Commander H. E. WHITTINGHAM said that he did not agree entirely that field service conditions were incompatible with the taking of adequate precautions against malaria. During a great war, owing to the employment of temporary troops, anti-malaria measures were likely to break down, but with regular troops this should not occur, since plain practical instruction of all personnel in anti-malaria work in peace time should so fit them for field service that malaria would not be such a scourge as it was in the East African campaign. Even during the war more could have been done to prevent malaria if daily routine lectures had been given to troops on board transports en route for the more eastern theatres of war.

With regard to the method of staining employed for the detection of malaria parasites, he (the speaker) was never an advocate of the thick drop method, which required a long time for fixing and staining, and was therefore apt to be left unfinished or forgotten during a strenuous day's work. Further, much valuable information was lost because a differential count could not be performed on thick drops. He used, and recommended, a simplified method of staining with Leishman's stain for routine work. This consisted of 0·5 per cent. solution of Leishman stain in methyl-alcohol. The stain was kept in a stoppered cylindrical staining jar of sufficient size to receive at least four slides. Ordinary blood smears were left in this stain for five minutes, rapidly washed in distilled water and allowed to dry in the air; they were then ready for examination. This method of staining saved much time, reduced labour to the minimum, and considerably lessened the occurrence of artefacts.

He thought the quinine was absorbed by the rectum and was of some value for malaria patients suffering from coma. There were instances of quininism following on the rectal administration of quinine in persons with an idiosyncrasy to that drug. Furthermore, in Mesopotamia in 1916 and 1917 quinine enemata (20 gr. to one pint of cold water) were given in heat-stroke cases—in 75 per cent. of which the patients were found to be suffering from malaria—with beneficial results.

Many of the large number of cases of quinine resistance recorded were not true instances of resistance. Some authorities doubted whether such resistance occurred. In the case of a nursing sister, to whom he had personally administered quinine by mouth and by vein, the malaria parasites had disappeared from the peripheral blood, but crescents could always be found in peripheral blood films the day after an intravenous injection of N.A.B. despite the continuance of quinine. In this instance the N.A.B. appeared to have driven out lurking parasites from the spleen and other internal organs. He mentioned this case, which was not an isolated one, because it pointed the way to a rational treatment of malaria.

The routine quinine treatment left much to be desired since the so-called cure at times did as much harm as the disease. The treatment could not be forced. In connexion with more rational treatment certain essential facts had to be borne in mind: (*a*) The malaria parasite was most vulnerable when circulating in the peripheral blood, as was the case in recently acquired infection. (*b*) In chronic or established malaria many of the parasites lurked in backwaters of internal organs, especially in the spleen. (*c*) The spleen could be made to contract by such substances as adrenalin or N.A.B., and thus certain of these lurking parasites could be forced out into the free circulation, where proper medication would aid in their destruction. (*d*) Quinine was not directly lethal to the plasmodium of malaria, but acted in some indirect manner, perhaps by damaging its vitality in sufficient degree to allow malaria antibodies to complete the destruction. (*e*) Quinine tended to damage not only the protozoa of malaria, but the cells of the human body. The problem was to strike

the happy medium. In recently acquired malaria it was feasible, and often advisable, to give large doses of quinine combined with alkalies, to eradicate the infection completely and quickly before a foothold was obtained in the backwaters of the blood stream.

In chronic malaria however it was useless to flood the system with quinine. It was preferable first to estimate the expected occurrence of relapses by means of a careful history or blood examination. Quinine should be administered just before and during these relapse periods. In addition, weekly intravenous injections of N.A.B. should be given to act as a tonic and to drive lurking parasites into the free circulation; quinine being administered orally during the following twenty-four hours to aid in the destruction of the parasites. The minimum amount of drugs necessary to attain this end should be given, and for this purpose blood films and careful clinical examinations should be carried out weekly before each injection. Experience over hundreds of cases of malaria had proved the beneficial effect of combining N.A.B. and quinine treatment in much the same way as N.A.B. was used in conjunction with mercury in the treatment of syphilis. N.A.B. was an excellent tonic, and a most necessary adjuvant in the treatment of chronic malaria.

Surgeon Commander S. F. DUDLEY said that Major Keyworth's paper was one more demonstration of the absolute hopelessness of all attempts to prevent malaria among troops operating in a country where the disease had a high endemicity. During the Great War of course every ton of shipping was required elsewhere, but he would like to know whether for peace-time military operations where the base had to be on the coast of a malarious country the military authorities had seriously considered the possibility of a floating base as a preventive measure against malaria. It was true that with the wind off-shore a stray mosquito or two might be blown a mile out to sea, but mosquitoes rarely appear in dangerous numbers in a ship moored a few hundred yards from the beach. At all events, the Royal Navy, working in rivers and off malarious coasts, had never been troubled with malaria to the extent which Major Keyworth had described, provided the men were sleeping in their ships. Except the expense he (the speaker) could see no administrative difficulty of any kind to prevent the whole base, under such conditions of warfare, being kept in ships. The supply services, the staff and reserves, and at least the Hospital, could be kept afloat. Even in regard to expense, it was problematical, if such an arrangement caused a drop in malaria incidence of from two to one invaliding per man per year, whether the fact of only having to replace all one's effectives once a year instead of twice, would not make a floating base a sound financial proposition.

Section of Tropical Diseases and Parasitology.

President—Dr. J. B. CHRISTOPHERSON, C.B.E.

[*November* 1, 1928.]

A National Outlook on Tropical Medicine.

PRESIDENT'S ADDRESS.

By J. B. CHRISTOPHERSON, C.B.E., M.D.

AFTER his introductory remarks as President, Dr. Christopherson went on to make an appeal for women doctors for the colonies, especially for the additional and important social and educational work amongst women and children.

This home country of ours has reached a stage where it finds a difficulty in absorbing all its well-qualified medical women. I can assure them that they would find that Africa is a fascinating field where there are many medical problems still awaiting solution.

There are a number of women already engaged in medical work in Africa doing amongst the native population not only routine work—giving stock mixtures and dressing wounds—but to the best of their ability undertaking any medical duty which presents itself, and doing it wonderfully well; they have to treat dysentery, malaria, yaws, bilharzia, hookworm, above all they have fearlessly adopted leprosy as their particular charge. I speak of the well-trained hospital nurses working with the missionary societies, who, in remote districts, supplement the work of the Government doctor in areas where, at present, the latter seldom reaches. I ask my hearers, if they have the opportunity, to give them every help in their work.

This Section is a section not only of tropical diseases, but also of parasitology, a subject of importance. The majority of tropical diseases are caused by the action of parasites, and we recognize that tropical medicine has been so successful in recent years because investigators have realized that it is of little use attacking the disease in man unless they are in full possession of the life history of the causal parasite, both inside and outside the human body. I should like to congratulate my predecessor in this chair in having raised his own branch of parasitology (helminthology) to a position of such importance, both in medicine and veterinary practice, that the British Government have shown their recognition of this fact by establishing an experimental farm under his direction at St. Albans.

It is, of course, important to deal with the cause of a disease at all points, but the primary attack should be on the parasite in the human being, for the following reasons: Our therapeutic weapons are so effective that we can well hope to kill the parasite in a direct attack, and not have to rely to the extent we do on the indirect principles of health. In the campaign against endemic disease in the tropics, it is no less important to attend to the infected individuals and thoroughly cure them than to pursue the parasite in its career outside the human body. We should, I suggest, aim at adequate disinfection of the patient in order to prevent the *intermediate host* from becoming infected. This is more important and more feasible than attempting to destroy the intermediate host wherever we find it, a task which is almost impossible. I fully recognize the importance of hygienic measures for the prevention of parasitic diseases in the tropics, but the direct attack on the disease in man by therapeutic methods should have the first place in the treatment of endemic tropical disease.

"The old aim of breaking the chain somewhere is replaced by the new one of killing the parasites themselves." This applies to other widespread diseases besides *malaria*. It applies to ankylostomiasis, bilharzia, and to leprosy. In fact, it applies to parasitic disease generally in the tropics. In making a general statement such as this one must carefully distinguish between diseases which are endemic and those

which are epidemic; prophylaxis naturally takes the first place in the attack on epidemic—potentially pan-endemic—diseases such as cholera, plague, small-pox and typhus, and it must be the chief aim intelligently to foresee that epidemic diseases have little opportunity to enter or re-enter a country.

With regard to intelligent anticipation of epidemic disease, I may cite Sir Leonard Rogers' important work on forecasting epidemic disease, small-pox, plague, cholera in India, by studying climatic indications, especially humidity and rainfall. This is prophylaxis of the best kind, with a wide implication, for it postulates that hygiene is not essentially medical, but a science of equal interest to all people and all professions. It is quite obvious that prophylaxis takes an important place in all medical undertakings.

But the indirect principles of medicine are not easily applied in tropical countries without interfering with long-established and jealously-guarded prejudices, customs and practices. In Mohammedan, Hindu, and other countries, in Africa and India, for example, firmly-held religious opinions are apt to impede the sanitarian, and the native religious question has to be sympathetically considered. On the whole, "natives" inhabiting tropical countries take medicines very well, but they take hygiene very badly.

I would like just to refer to the problem of the elimination of bilharzia from Egypt by the direct method, that is, by intravenous antimony injections. Antimony tartrate is a cheap and easily administered remedy, and there is no danger of shortage. It cures 100 per cent. of those who take the full course. This is the written opinion of Drs. Lasbrey and Coleman, who treat at the C.M.S. hospital, Cairo, every year over a thousand cases of bilharzia by intravenous injections.

Professor Khalil, in a recent report, states that "the number of bilharzia patients who attended the special Egyptian Government clinics in 1927 was 284,934. This number does not include those treated in other Government or private hospitals, nor the large number of cases treated privately in clinics." He continues: "It is not exaggeration to estimate the total number of cases presenting themselves for treatment against bilharzia in Egypt at half a million a year." There is evidently no prejudice amongst native Egyptians against intravenous injections of antimony. If the remedy were taken to the villages by means of mobile hospitals many more would be able to avail themselves of the treatment, and fewer would break off treatment before the full course was completed, for they would not have to consider the time and expense of a journey to the towns where the special hospitals are now located. The effectual treatment of 1,000,000 cases, only twice the number of cases now treated, would be a substantial step towards eliminating bilharzia from Egypt. It is a more practicable problem to control bilharzia disease in man than to control the disease in the snail.

I agree that it is just as indispensable to deal with *infected* snails as it is to deal with infected mosquitoes in malaria. But my own opinion is that to attempt to "break the chain" by grappling with the snail carrier is too difficult a problem in Egypt.

Great progress has been made in the treatment of leprosy, especially of early cases during the last decade, owing to the inexhaustible energy of the first President of this Section.

In Norway compulsory isolation reduced leprosy to a rarity. But segregation of only such cases as the authorities could collect has not reduced the incidence of the disease in the colonies and dependencies; it has, on the contrary, prevented cases being brought to notice before their condition was hopeless. Rational treatment is now replacing segregation.

The British Empire Leprosy Relief Association, which has Sir Leonard Rogers as its principal adviser, is to be congratulated on its policy, which is securing the

confidence of the lepers and bringing the improved treatment within their reach by means of out-patient dispensaries and by careful discrimination between infectious cases and those which are practically non-infectious. It is to be hoped that the stigma still attached to the disease in England will soon be effaced, so that practitioners may be better able to deal with the cases which they occasionally encounter in this country.

Tropical Medicine from a Political Standpoint.

There is an aspect of tropical medicine which appears seldom to occur to politicians, namely, the effect of widespread endemic infection on the moral and material condition of a community or nation as a whole. This is notably the case in Egypt, a country in which I have spent many interesting years. I venture to put before you to-night one of its particular problems. Countries, regions, areas, vary much in their conditions, but the principles which I support will, I think, hold good in all. They can be maintained, but modified and adjusted to the needs of each.

Egypt occupies the north-east corner of Africa as far south as latitude 24; its southern boundary is in the tropics. It is approximately 1,000 miles long and 200 miles in width. The Nile is the longest of all rivers, excepting the Amazon. One-fourth of the Nile flows through Egypt, and the prospects of the Egyptians depend entirely on the agricultural population who dwell on its banks.

Egypt has been cited as one of the most striking contrasts in the world between past grandeur and modern decadence.

For the purpose of my thesis the Egyptian nation may be divided into three classes: (1) The Turco-Egyptians, who were the ruling class till 1879, when the Khedive, Ismail Pasha, was deposed. (2) The great and increasing professional class, educated at the Government schools in the large towns and at post-graduate classes in the European capitals.

We are all well acquainted in London with the Egyptian medical graduate; we admire his great capabilities and do all in our power at the medical schools to equip him for the many difficult medical problems which confront him when he returns home; it is not, however, the intelligentsia of Egypt about whom I wish to speak this evening.

(3) The numerically great agricultural class which makes up the immense majority of the Egyptian nation, the fellaheen,[1] nearly 14,000,000 in number, who cultivate the soil, on whose work and on whose health the welfare of the country depends; for the educated classes and the foreigners together do not number 500,000. The fellaheen are the vitalizing element of the nation.

Since 1879, under British guidance, Egypt has acquired a thoroughly satisfactory irrigation scheme, imposing engineering works (the Assuan Dam), railways of which she may be proud, Government schools, a code of civil law based on sound principles of justice, hospitals, a sanitary department, asylums, quarantine, well organized police and army, and the framework of a constitutional government.

Everything is in her favour, but this: the majority of Egyptians are quite illiterate and are embarrassed by disease. The health problem in the past has been a difficult one to contend with, and to-day it is, I consider, the most urgent.

It is well known that the people of Egypt have to contend with four serious diseases, bilharzia, ankylostomiasis, pellagra, and ophthalmia, which have been endemic from ancient times.

Ophthalmia is no newcomer; the earliest known oculist, appropriately named *Iri*, was an Egyptian living in the sixth dynasty, about 2500 B.C.

Ankylostomiasis is probabably referred to in the Ebers Papyrus, written 1550 B.C.

1 The immense majority of the population of Egypt are fellaheen (agriculturists)—Lord Cromer, "Modern Egypt," ii, 193. There can be no doubt that it was the hand of England which first raised him from the abject moral and material condition in which for centuries he wallowed.

We know certainly that bilharzia was present in the country between 1250 B.C. and 1000 B.C. It was found by Armand Ruffer in mummies of the twentieth dynasty.

I do not imagine that pellagra has only recently been introduced, so that we may assume that Egyptians were contending with these diseases before 1000 B.C., which is about the time when Greece became a nation.

MacCallan states that granular lids affect 75 per cent. of the population. There were in 1907 as many as 148,280 totally-blind Egyptians and 363,702 blind in one eye. Few will deny that granular eyelids and loss of vision are serious obstacles to education and economic efficiency.

Pellagra—a chronic and disabling disease—very prevalent amongst the agrarian population, fills the two asylums of Egypt, and it is only the later stages of the condition which reach the asylum. The treatment at present is unsatisfactory and the ætiology a matter of dispute, further research is urgently called for. The two most formidable diseases of Egypt are bilharzia and ankylostomiasis.

It is no exaggeration to say that 75 per cent of the Egyptian nation suffer from bilharzia. (*Ankylostomiasis and Bilharzia Report*, Egypt, 1924, p. 111.)

According to the statistics of Kasr el Aini Hospital, 8 per cent. of the total deaths in hospital are directly due to bilharzia, and in some agricultural districts 94 per cent. of the inhabitants are infected with bilharzia.

Not only does bilharzia in Egypt run a chronic progressively disabling course, owing to constant re-infection, but as Khalil has pointed out, serious epidemics of acute bilharzia occur in the villages which carry off in a few weeks as many as 100 from, say, a population of 5,000.

Griesinger and Bilharz regarded ankylostomiasis to be the cause of one fourth of the total deaths in Egypt. There is no reason to doubt these figures. Like bilharzia, constant re-infection, to which Egyptians who live in endemic districts are exposed, renders ankylostomiasis especially deadly.

In England we see bilharzia and ankylostomiasis only in their mildest forms—in Egypt both diseases are aggravated, progressive, and often fatal. The two diseases possibly account directly for one-third of the deaths in the whole country. Both are caused by parasites which derive their sustenance from the blood and contaminate the circulation with toxic products.

Ankylostomiasis produces advancing anæmia with its attendant symptoms, physical lassitude and mental apathy, whilst the stoutness of the hookworm patient is a pathologic condition which misleads by giving the appearance of muscular strength.

The early stages of bilharzia, deterioration of the blood and toxæmia, are followed by cystitis, perineal fistulæ, stricture, stone, pyelitis, hepatic cirrhosis and cancer. Egyptian hospitals contain many aggravated and distressing surgical bilharzia cases.

It is a fair statement to make that these diseases which affect so large a proportion of the 14,000,000 inhabitants of Egypt affect the intellectual capacity of the nation as they undoubtedly affect its physical competency.

We may justly describe the Egyptian fellah as a man of good physique with a somewhat hang-dog expression of countenance, a pale, sallow, muddy complexion, not over-clean in his habits and not too tidy in his dress, his feet naked, his wardrobe limited to three cotton garments, soiled and humble in the extreme. He has as little thought for others as he has for himself; he is said to be of a cruel disposition, unreliable, impulsive, emotional, fatalistic, fanatical, easily influenced to criminal acts. He will submit to much ill-treatment and bullying from those in a position to tyrannize over him. He works hard for the short seasons of Egyptian cultivation, but is unable to sustain a prolonged effort. He appears to have no ambition to improve his position in society. His outlook is entirely limited to satisfying his daily needs, and does not extend beyond his home and village. His manners, habits and

resources are those of 2,000 years ago. He is therefore very attractive to the numerous tourists who are interested to-day to see the very types which the Bible has made so familiar.

Now the politician would, without much ado, find a cause for the unfortunate infirmities characteristic of the fellaheen in *national decadence*, and would seek a remedy from the lawyer and the educationist, who would prescribe legal codes and educational schedules, and before Griesinger discovered the cause of Egyptian anæmia and Bilharz the cause of endemic hæmaturia, they could have gone to no better counsellors for advice.

But the law is no remedy for, and education is lost on, a population suffering from ophthalmia, pellagra, bilharzia, and ankylostomiasis.

The medical view of this suspended national evolution must be from another angle than that of the politician.

It is a striking fact that a community of 14,000,000 living in close proximity with Western civilization should be 2,000 years behind the times, as the fellaheen of Egypt are.

Can we maintain that the tyranny and oppression of Egyptian task-masters alone is a sufficient reason? Has not wholesale impairment of national health and the merciless burden of sickness played a not insignificant rôle in bringing about this arrest of intellectual evolution?

The physician finds that an individual infested with the parasites of bilharzia and ankylostoma has much the same deterioration of physical and mental qualities as those I have described in the Egyptian fellah, but that the patient with these infections is a different individual after he is rid of his parasites. He becomes an ordinary healthy individual, able to do a good day's work, and endowed with plenty of ability to learn and assimilate what western civilization has to teach him. A nation of such emancipated fellaheen should be quite able to maintain an honourable place amongst the European nations.

Can we conclude that the character, outlook, the mental and physical attributes of the agrarian population of Egypt stand in some relation to the parasites they harbour and have harboured since the days of the Pharaohs?

We are supported in such an opinion when we learn that the Egyptian fellaheen army which until 1923 rejected recruits suffering from ankylostomiasis and bilharzia, no longer does so. Now all newly-enlisted soldiers are examined especially for these diseases, and those infected are treated in special hospitals, so that each soldier now commences his military career free from disease. The efficiency of the army has thereby been very effectively increased, and only one-third the number report sick that formerly did.

Systematic treatment of helminthic diseases was adopted on the suggestion of Major Neven Spence, R.A.M.C., and the eradication of these formidable diseases from the Egyptian army is largely due to him.

Medical treatment which is successfully applied to individual cases, and which has been with equal success applied to an army of 18,000 men, can be applied with the same measure of success to the Egyptian nation.

The land of Egypt has been the house of bondage for the agricultural classes since before the Exodus.

The tyranny and oppression of pashas in the past would not appear in itself to be sufficient to account for what Lord Cromer wrote of as the "abject moral and material condition in which for centuries the fellaheen wallowed." For had the numerically great agricultural class in Egypt been physically fit to do so, it would have revolted many times in the past and thrown off the yoke.

They have not been without leaders—in our own time patriots of outstanding ability have sprung from the ranks to champion the national cause—Ahmed Arabi Pasha, in 1881, rose from the fellaheen, and although it is true that Arabi did not

intend to set up a constitutional government, his rebellion failed miserably because he relied on military qualities which the fellaheen did not possess.

Said Pasha Zagloul, the greatest leader of all Egyptian nationalist leaders, was of fellaheen stock. His policy was Egypt governed by Egyptians under the constitution, but constitutional government is not possible whilst the majority of voters, the fellaheen, are inarticulate in the parliamentary sense by reason of their complete illiteracy.

My experience leads me to the conviction that the fellaheen cannot be educated until the diseases which so much oppress them (bilharzia, ankylostoma, pellagra, ophthalmia) are banished, so that a healthier body and a complementary more receptive mind are prepared for the fundamentals of progressive development.

The somewhat striking activities of a great missionary organization are worthy of attention. The Church Missionary Society in old Cairo has wisely realized this point of view, and combine medicines with education in their fine hospital, and during the years 1926 and 1927 no fewer than 13,167 persons were treated there for bilharzia and ankylostomiasis.

Under the circumstances the physician is the adviser of outstanding importance for Egypt at the present time. Let Egypt be freed from her endemic diseases so that the agricultural classes can be educated and make use of the vote, then the government will be essentially national and constitutional, and a new era of prosperity, if not a return of her ancient grandeur, is in store for her.

This, I submit, is an example of applied tropical medicine in the case of a nation in which we all take a sympathetic interest.

Some Practical Considerations in the Diagnosis and Treatment of Chronic Dysentery.

By J. Graham Willmore, M.D.

The treatment of patients suffering from chronic dysentery still leaves much to be desired; this problem cannot be regarded as finally solved until it is possible to give a reasonable guarantee that the disease can be eradicated in any given case, so that it will never recur, apart from re-infection.

The object of this paper is to expose my own experience—of 570 admissions to a Ministry of Pensions hospital during the past four years—and to solicit an exchange of views from other clinicians, in the hope that thereby something of benefit to cases of chronic dysentery may emerge.

In extenuation I may remark that, to the best of my knowledge and belief, there has only been one death directly due to dysentery (bacillary) in my series.

Diagnosis.—The *sine qua non* of the treatment of dysentery is, of course, diagnosis. For this I depend, as a routine, on three methods:—

(1) Careful clinical examination of every organ and system in the body. In this connexion I have only time to mention one or two points; firstly, a tender, hard, thickened, descending colon always means bacillary dysentery—past or present—though it does not exclude amœbic dysentery as well. Secondly, tenderness and thickening over the cæcum and appendix usually indicate amœbiasis, especially in connexion with an enlarged liver. Thirdly, any obstruction is practically invariably due to some cause other than dysentery; it may be malignant, tuberculous or syphilitic. Fourthly, dysentery rarely causes profound anæmia; if this be present, again look for some other cause, such as malignant disease, sprue, apical dental sepsis, the blood diseases or syphilis. Two of the most profoundly anæmic cases that I have recently seen were due to syphilitic colitis supervening on old amœbic dysentery. (Note that the Wassermann reaction is sometimes negative in these cases.) The same is true of jaundice; in my experience deep jaundice in a case of liver abscess always means that bacterial infection—primary or secondary—is present.

(2) Microscopic examination of the stools. This cannot be done too often; not only may repeated examination be necessary to detect protozoa, but in mixed cases it is remarkable how one specimen may consist of bacillary exudate and another, from the same patient, of amœbic. Ten years of constant work since Shearman and I first published our paper on the cytological diagnosis of the dysenteries have only confirmed my reliance on this method; in my experience, it is as accurate in chronic as in recent cases, though more attention should be paid here to the changes occurring *in* the cells (cytokaryolytic in bacillary dysentery, cytoplasmolytic in amœbic) than to the number and nature of the cells composing the exudate. Charcot-Leyden crystals, commonest by far in amœbic dysentery, are due to the breaking down of eosinophil cells and may consequently be present in bacillary and other non-amœbic stools, particularly in "intestinal allergy."

(3) Sigmoidoscopy. It would be as logical to attempt to treat a case of sore throat without looking at it, relying merely on examination of the sputum, as it is to dispense with the sigmoidoscope in dysentery. I have not time here to describe all that one may see with this instrument; but a good general guide is to remember the essential difference in the pathology of bacillary and amœbic infections.

In bacillary dysentery one is dealing with a diffuse, widespread, comparatively superficial, primary inflammation; so that, depending on the phase, one may see a general swelling, hyperæmia, œdema, hæmorrhagic infiltration, coagulation necrosis and exfoliation of the necrotic mucosa, leaving an extensive raw granular surface beneath, or scattered coralline ulceration; or, in quiescent cases, the mucosa may be almost entirely replaced by a generalized fibrosis, causing the gut walls to become thickened and inelastic, the lumen to be narrowed and the surface to be pale, smooth and glistening, with a curious geographical appearance—rather like crocodile skin—due to dilated veins. In some cases the hæmorrhoidal veins, especially below and on the valves of Houston, may dilate to the size of one's finger, burst and give rise to serious hæmorrhage. Another not uncommon sequela of bacillary infection is the development of papillomatosis, due, I imagine, to buds of mucosa becoming caught up by irregularly contracting bands of fibrous tissue and undergoing adenomatous proliferation subsequently; sometimes they become carcinomatous.

In amœbiasis, on the other hand, one is dealing with discrete scattered foci of primary degeneration in the submucosa, appearing first like yellow pimples with a narrow red ring around them in the pink mucosa; by cutting out one of these "pimples" it is often possible to demonstrate teeming amœbæ, even when repeated examination of the stools has been negative. As they enlarge, coalesce and break down they form deep ragged ulcers, with undermined edges and "wash-leather" sloughing bases; as they heal they leave scars to match. They are often pin-point, if the original ulcer has been small. Even in the most extensive and chronic cases, with secondary infection, it is wonderful how true to type the picture remains.

Tuberculous enteritis, too, gives rise to characteristic changes, which are often first apparent through the sigmoidoscope and may be confirmed by finding tubercle bacilli in the scrapings. Finally, early carcinomatous growths may be submitted to biopsy by removing a small piece with crocodile forceps. Of other methods of diagnosis by stool culture, agglutination and X-rays (meals and enemata), each may be useful in individual cases; but I have found those detailed above most useful as routine measures. As a general rule, one cannot hope to isolate dysentery bacilli in cases of ten or more years' standing; as to agglutination, its value may be gauged by the following instance. An ex-soldier contracted Shiga dysentery in 1916; from 1920 to 1926 he was in more or less good health, when he had a severe recurrence, which was diagnosed as bacillary by sigmoidoscopy and the cellular exudate. The bowel cleared, under treatment, all except a patch of rather deep ulceration in the rectum; scrapings from this area, taken by a sharp spoon through the sigmoidoscope, gave a pure culture of *B. shiga*, verified by all the usual biochemical and

serological tests. His blood, however, remained completely devoid of agglutinins for this and all other strains of *B. shiga* for as long as he was under observation. The ulcer cleared up under direct local actinotherapy, applied, through the sigmoidoscope and a quartz applicator from a Kromayer lamp, by my colleague, Dr. Weinbren. The patient has remained in good health, to the best of my knowledge; but, since he had previously had six years' freedom from dysentery, how can one say, even now, that he is *cured ?*

Treatment.—(1) Amœbic. I have already published (*Brit. Med. Journ.*, 1926), in collaboration with W. H. Martindale, a detailed account of the "combined" treatment of chronic amœbiasis by means of auremetine, stovarsol and bismuth.

The method followed since the summer of 1924 is roughly as follows, though, of course, it is modified to suit individual cases :—

Acutely relapsing cases—that is, those with blood, mucus and *Entamœba histolytica* in the stools, or found in scrapings at sigmoidoscopy—are given :—

(*a*) Auremetine, 1 gr. in soft gelatin capsule four times daily after food, on alternate days for seven days, and then daily to a total of 40 or 60 gr. ingested.

(*b*) Stovarsol, 4 gr. three times daily for seven days, on alternate days with the auremetine. Previously we gave 4 gr. twice daily for ten or even fifteen days, alternating with the auremetine days.

(*c*) On stovarsol days a rectal injection of emetol, 2 drm. (1 gr. emetine base), in ether, 6 drm., and olive oil, 12 oz. (Time retained to be charted.)

(*d*) "Panama bismuth," three-hourly for twenty days, and then three times daily.

(2) "Chronic" or cyst-carrying cases are given :—

(*a*) Auremetine, and (*b*) stovarsol, on alternate days, as described above.

(*c*) "Panama bismuth," three times daily before food.

(3) When hepatic involvement is present, 1 gr. of emetine hydrochloride by intramuscular injection, on alternate days for six days (that is, 6 gr. in all), may supplement or replace the emetol.

Since then I have given yatren a further extensive trial; administered *per os*, in doses up to 1 grm t.d.s. for twenty days, it seems to have little to recommend it. (Its cost has even less to recommend it.) In no case in which other methods had failed did it succeed, even as an efficient palliative. Further, in those dangerous cases of mixed amœbic and bacillary infection, I think it is definitely contra-indicated. I was recently called to such a case in Switzerland, in which the patient all but died of hæmorrhage and toxæmia; we could only attribute his condition to the use of yatren, in that the disease became definitely worse during its administration and improved when the "combined" treatment, together with sulphates and careful lavage, was substituted for it; and that similar, though less severe, symptoms had immediately followed its administration during an attack some nine months previously. The patient eventually recovered, up to a certain point. Yatren seems to be too irritant for use in bacillary cases.

Administered *per rectum*, however—usually a litre—in 2 to 4 per cent. solution, it seems much more efficacious, and—though it has failed in some cases in which, it must be admitted, all other treatment had equally failed—in at least three cases its use was followed by apparent cure after other measures had failed. The cases are too recent for me even to speculate how long this happy state of affairs may last. As a routine treatment, therefore, I still use the method already described, as so far I have failed to find a better. This does not mean that I am satisfied with it, and I supplement it—in all cases where vegetative amœbæ are found, or where sigmoidoscopy shows active amœbic infiltration and ulceration—by a course of twelve to twenty yatren irrigations, the first two days at 2 per cent., the others at 4 per cent. strength, daily or every other day. Yatren, therefore, like other drugs has its uses, but one must, with Otto Willner, in China, deprecate

the extravagant claims made for it in the German press—claims possibly not unconnected with commercial considerations. It is to be noted that Willner, with his immense experience, also considers the "combined" treatment, supplemented by yatren, to be the most effective; but there is still room for much improvement.

(2) The treatment of chronic bacillary dysentery leaves even more to be desired; this is only to be expected when one considers the extent of the damage that has taken place. I do not know to what extent the mucosa can regenerate, but I imagine its powers are very limited. Consequently when one sees—as I have often seen—from ten to twenty bed-pans a day, for several days, nearly filled with blood, muco-pus and sphacelated mucosa; when the bowel, as far as one can see, resembles a large raw wound covered with exuberant granulation tissue, bleeding at a touch; or buds of this protruding through the bile-stained, greenish-black, necrotic mucosa; when the most that one can hope for is the restitution of a functionless, fibrosed, narrow, inelastic tube; the question then is more how immediately to save life than to attain complete cure. To my mind, the answer is by elimination and neutralization of toxins. The neutralization can only be effected by massive repeated doses of anti-dysentery serum (multivalent), 80 to 120 c.c. daily for three to five days. In ex-Service men—so many of whom have had serum previously, for wounds, diphtheria or dysentery—it is wise always either to test the skin reaction or to desensitize; for this reason, unless the case is desperate, intramuscular injection is much safer than intravenous injection. In all ex-Service men, at least, death from true anaphylactic shock—symptoms of which manifest themselves in a few minutes from the commencement of the injection as precordial pain, air-hunger and unconsciousness, with or without convulsions—is not the chimæra that some would have us believe. Whether the serum is truly antidotal, or whether it acts by protein shock effect, I do not know; at all events, anti-dysentery serum is more effective than any other serum, and I am convinced that in many of my cases it has made just that small difference which has meant life or death. In chronic bacillary dysentery, however, and in the so-called ulcerative colitis—which for reasons that I have not time to give now, I am convinced is the same disease—the effect is not *curative*, as it is in acute, recent cases, and it gives no more guarantee against relapse than does any other treatment.

The elimination of toxins may be carried out in three ways:—

(1) By cæcostomy, appendico-cæcostomy, or appendicostomy, depending upon the urgency of the case. In cæcostomy the cæcum is opened and drained in one stage, and irrigation is commenced forthwith; both drainage and irrigation are more effective with this operation, which is reserved for desperate cases, but it is impossible to prevent the wound from becoming infected, with consequent protrusion of mucosa and hernia. In the other operations—many of which were performed for me by my surgical colleague, Mr. MacNeill—the appendix and a "spur" or small pouch of cæcum are left protruding from the wound, which is firmly sutured round it and to the bowel. The wound is healed by the time the appendix sloughs, and is then dilated to take a large De Pezza's catheter.

(2) By copious lavage per rectum.

(3) By giving the patient as much bland fluid as he can be induced to swallow, mainly in the form of glucose-lemon-orangeade, neutralized to litmus by sodium bicarbonate and containing 1 lb. of glucose to the pint. This forms the exclusive dietary at first. This washing-out from above is aided by sulphate of soda 1 drachm with syrup of orange and orange-flower water, at first every two or three hours, later three times daily and finally once only, the first thing in the morning. In the absence of gastric or duodenal symptoms, this is the only medicine that I now give by the mouth; its value simply cannot be over-stated, and is, I think, partly due to its combining power with some of the poisonous amino-compounds in the intestine, to form non-poisonous ethereal sulphates: cf. its antidotal effect on the phenols. The various preparations of kaolin, which is supposed to adsorb the

toxins, the much vaunted prescription of iron and mercury perchlorides, rhubarb, the so-called intestinal antiseptics one and all, I consider to be quite useless.

Above all, opiates are to be avoided; paradoxical as it may sound, a patient wakes up more exhausted from a "doped" sleep—even of some hours' duration—than he is by constant appeals to the bedpan during a similar period.

I do not think it matters much what irrigating fluids one uses, provided they are non-irritating in the "acute" stages, as it is the mechanical cleansing which is important. In my practice I employ the following:—

(1) Cooking or sea salt 1 per cent. with or without sodium bicarbonate, 1 drachm to 1 pint.

(2) The above with eusol, iv oz. to 1 pint, or Milton, a teaspoonful to the litre, which I prefer.

(3) Salt, 1 per cent., with trichlorphenyl-methyliodosalicylic acid (T.C.P.) 1 : 1,000 (1 : 100 of the stock solution).

(4) Ether, vi drachms to olive oil, xii oz. This should not be used in very acute cases, but is often remarkably efficient later and is useful in mixed cases, when emetol may be combined with it.

(5) Finally, when the case "hangs fire" near recovery—but never in the acute stages—silver nitrate 1 : 2,000 to 1 : 1,000 in distilled water. To my mind, the effect of this is to provoke a reaction in "lazy" tissues and any benefit depends entirely on this.

In chronic dysentery all that one can achieve by irrigation is to minimize septic absorption, just as one does in the surgical treatment of infected wounds elsewhere in the body. The germs responsible for the disease are safely tucked away in the submucosa and completely protected from the action of any antiseptic which does not penetrate the bowel wall. Since ether appears in the breath, and T.C.P. in the urine, it was hoped that these substances might exert a germicidal effect *en passant*. Consequently, I think the rationale of using expensive non-coagulant organic silver preparations is based on a misconception of the pathology of the disease, and in practice I have not found them to justify their cost.

From a consideration of the above, it will be realized that, as the essential germs are inaccessible to local or oral medication, one can only hope for cure by so developing the patient's own resisting power, his immunity, his *vis medicatrix naturæ*, that he will eventually cure himself. No one else can, as yet, do that for him; we can only help him by eliminating toxæmia and by using every means in our power to aid him to build up his strength. It follows from this that the diet is all-important: the initial glucose-orangeade must be increased as rapidly as possible. So many physicians, it seems to me, treat the bowel at the expense of the patient; it is my practice—once the "danger" is past—to encourage patients to eat all they can, and above all to consult their likes and dislikes. Fats, as a rule, are badly borne (the total fat content in some cases of dysentery may be 40 per cent. or over, quite apart from sprue), but almost every other article of diet may be given, beginning with potato or lentil purées, soufflées, custards, farinaceous puddings, purées of fish or chicken, and so on to chicken, chops and underdone steaks. While there is no more certain method of inducing a relapse of dysentery than the taking of alcohol in excess, in these debilitated patients a small dose of alcohol with their meals, preferably a whisky and soda or a light wine—helps, I am sure, that "good digestion" which "waits on appetite." Yaghûrt, with sugar and chocolate, is given to the extent of at least a pint a day. Flatulence is often troublesome and may be relieved by cachets t.d.s., p.c., of taka-diastase, pancreatin and salol (Castellani).

℞ Taka diastase ... ... gr. v
Pancreatin ... ... gr. v
Salol ... ... gr. x
Soda bicarb. ... ... gr. x
Pulv. cinnamomi ... gr. ii
(For two cachets.)

Similarly with regard to rest; though this is essential in the early stages, a patient should not be bedridden a day longer when he is able to get up. Over-fatigue, of course, is bad, but I am not sure that too much bed is not worse.

Of general tonics, no drug is of the slightest use: change of air and scenery—and escape from hospital—are always indicated provided that the patient likes his surroundings. No good is ever obtained from what may theoretically be the most ideal scenery and climate if the patient happens to take a dislike to the place.

Of methods aiming at a direct raising of the patient's "immunity," actino-therapy—either heliotherapy, or general baths from carbon-arc lamps—easily hold first place, the most obvious improvement being the gain in weight; but here again one has to treat the patient rather than the disease, and I have seen more than one instance when exposure to light in any form had a definitely bad effect. It was almost as if these patients were photo-sensitive and suffered from light allergy. Theoretically one should, of course, be able to raise the immunity by inoculation; in practice, whether one uses ordinary, "detoxicated," "immunogenic" or "local immunity" vaccines, the immunity is not increased. Perhaps, in the future, some development of the bacteriophage, or of those mysterious substances which, in certain media, the intestinal bacteria elaborate to their own undoing, may afford that "final solution" which, to most of us at least, seems still so far off.

Discussion.—Dr. G. CARMICHAEL LOW said he had listened to Dr. Willmore's paper with special interest as he himself had now been treating dysentery for over twenty-eight years and he agreed with practically everything that Dr. Willmore had said. Yatren, in his hands also, had not given the great results claimed for it in Germany. The sigmoidoscope was a most valuable instrument in diagnosing dysentery and he (Dr. Low) used it in every case. It was also valuable in detecting carcinoma of the rectum and such an examination should never be omitted. Repeated stool examinations were required for the diagnosis of amœbic cases, a few negative findings being of little value.

Dr. BROUGHTON ALCOCK said his own experiences of the cytological diagnosis in chronic as distinct from acute dysentery did not conform to those of Dr. Willmore. In the diagnosis of chronic dysentery or recurring dysenteric symptoms in old cases of bacillary infection, from the cytological picture seen on microscopic examination of the fæces, he had found that other acute bowel infections such as he had found due to *B. morgan* No. 1 or certain strains of *B. paracoli* might have cytological characters indistinguishable from those seen in chronic bacillary dysentery. Such non-dysenteric infections might arise on a damaged bowel wall in which the *B. dysenteriæ* organism might or might not be embedded, but he could not agree that the dysenteric organism had always lighted up and started the infection. Finding in association with active bowel symptoms numerous colonies of certain organisms, other than a *B. dysenteriæ*, not found or only in a very rare colony in fæces from non-inflamed bowel walls, he admitted the probability of another infective organism producing symptoms in old bacillary dysentery cases without the presence of *B. dysenteriæ*. Moreover, admitting the possibility of a *B. dysenteriæ* being caught up in the fibrosis of the colon after bacillary dysentery, his experience of bacillary dysentery cases during and since the war led him to believe that in temperate zones chronicity, recrudescence or carrier state was extremely rare and less so than amœbic dysentery. An important adjunct to the cytological picture of a dysenteric infection was the presence of red blood-cells with the leucocytes and tissue cells.

The fact that the blood-serum did not agglutinate these other organisms did not exclude the possibility of bowel infection, as they did not appear to pass through the bowel wall.

Moreover, the blood-serum did not agglutinate a *B. dysenteriæ* in the cases to which he had referred.

Section for the Study of Disease in Children.

President—Dr. F. J. POYNTON.

[*October* 26, 1928.]

Dyspepsia in Childhood.

By JULES TAILLENS, M.D.

(*Professor of Children's Diseases at the University of Lausanne.*)

ABSTRACT.—(1) A dyspeptic child rarely, and a baby never, complains of indigestion. It is only from the age of ten years that subjective symptoms sometimes exist. It follows, therefore, that dyspepsia in childhood is often hidden, and has to be searched for in order to be diagnosed.—(2) Dyspepsia in children often takes a misleading form, as it produces a general weakness and wasting, often slight fever, and even coughing. It is therefore frequently confused with tuberculosis.—(3) Dyspepsia in children is a functional complaint, and its cure is therefore almost invariably easy and often rapid.—(4) The different digestive actions depending upon one another, it is usually the first which causes dyspepsia in children. In other words, such dyspepsia originates in the stomach.—(5) In more than three-fourths of the cases this gastric trouble consists of a hyperchloracidity. (6) Treatment is as follows: An approved diet, chiefly consisting of milk and farinaceous foods, a two-hours' rest after meals, and the administration of alkalies. A quick progress towards a complete cure is observed, and, in particular, an increase in weight is very rapid. Statistics drawn up from 200 cases show this, and also that such hyperchloracidity can reach high figures even in very young children.

ONE of the branches of pædiatrics which has most closely occupied my attention during the past ten years is that of dyspepsia among children of from 1 to 14 years of age. I leave on one side the question of infant digestion, which is so special.

Time prevents my dealing with my subject fully. I cannot, for example, go into the ætiology of these frequent troubles, or into my methods of examining them. I wish only to bring to your notice certain points which seem to me of more general interest.

(1) It must be noted that a child rarely complains of the digestive troubles from which he is suffering. Until the ages of from 8 to 10 years, subjective symptoms are completely lacking. From the age of 10 years a child, in rare cases, may complain of pains in his abdomen, and the older he grows, the more often he speaks of these pains. One knows, in the adult, how constant are these symptoms.

All this shows that dyspepsia in children may pass almost entirely unnoticed, unless one is on the look-out for it, and all the more easily because its symptoms are often so entirely misleading. As aggravated cases frequently show such symptoms as emaciation, general bad condition, a slight feverishness and coughing, it can be understood that tuberculosis is often suspected. And, in point of fact, a number of children are frequently sent to the University Children's Clinic at Lausanne with a diagnosis of latent tuberculosis, of tuberculosis of the glands, and even of pulmonary tuberculosis, who are in reality suffering from dyspepsia and are generally hyperacid dyspeptics.

Therefore, as a first point, we come to this conclusion: *That dyspepsia among children*, or, in other words, *digestive and especially stomachic troubles among children, are at this age so frequently hidden that to be found, they must be thought of and searched for.*

(2) Child dyspepsia takes many forms, but declares itself almost invariably by a general debility; in slight cases the only symptom is a backwardness in putting on weight. Nearly always the growth in height continues normally. General weakness increases in proportion to the seriousness of the dyspepsia, and in the more

aggravated cases one finds, as I have stated before, a collection of symptoms entirely resembling those of tuberculosis, namely, wasting, bad colour, irregular temperature, with a tendency to feverishness, loss of appetite, nervousness and a tendency to frequent attacks of pharyngitis and even bronchitis. Only a complete investigation enables us to establish a diagnosis, confirmed later by therapeutics.

So we come to our second point:—

Dyspepsia in children upsets the general state of health, and, especially in accentuated cases, readily takes on the appearance of tuberculosis.

I lay stress on this, having seen children treated as tuberculous and sent as such to a sanitorium, where they stayed without deriving any benefit. Once their condition was clearly understood and diagnosed as dyspepsia, and an appropriate treatment given, a complete cure speedily followed.

(3) Dyspepsia in childhood is, with few exceptions, purely functional (though we know that with advancing years the contrary is the case), and in examining children this point must be remembered,

It follows that a complete cure of dyspepsia is infinitely easier to obtain in childhood than in adult life. Taking for my authority the several hundred cases I have personally closely followed, I would even go so far as to state that the cure of such dyspepsia is almost invariably certain if an exact diagnosis is made and judicious treatment applied.

My third point, then, is that *dyspepsia in children is purely functional, and, as such, is easy to cure.*

(4) Physiology teaches us that digestion is a grouping of several actions, each one of which, like an act of a play, is at once separate from yet dependent on its predecessor. Thus, digestive troubles originate invariably in the stomach, and analysis of food in its different stages of digestion has proved that any disorder in the intestines, liver or pancreas, is secondary to an original gastric trouble. Gastric therapeutics again prove this.

So we come to our fourth point, namely, that *dyspepsia in children originates habitually in the stomach.*

(5) If we investigate further, we find that three-quarters at least of these cases are caused by a hyperchloracidity.

Desirous of creating a practical method easily applied in hospital, clinic or private house, we have put aside all complicated means of research, and have kept to that supplied by simple gastric chemistry.

The child eats a "test-meal," the ingredients varying with his age, but approaching as nearly as possible those of a normal meal.

(It has always appeared to me utterly illogical to give as a "test-meal" a cup of tea and a roll. Such a meal is in no way normal, and the stomach is, consequently, examined under abnormal conditions, making exact calculations impossible.)

This "test meal" is withdrawn after from an hour and a quarter to an hour and a half, and is tested for free hydrochloric acid and for hydrochloric acid in combination, and, by the addition of the two figures, for the total hydrochloric acid.

Long experience permits me to say that, as a result of such an examination, the gastric juices of children should not yield more than 0·50 per cent. of free hydrochloric acid, or more than 2 per cent. of total hydrochloric acid. All higher figures may be considered as abnormal.

As a fifth point, therefore, we can state that *gastric hyperchloracidity is frequent, and, in the majority of cases, is the basis of dyspepsia in children.*

(6) Finally, after a short survey of this subject, we come to the treatment employed.

First of all a diet must be strictly observed, and we follow that which we call a "white diet," composed of milk and farinaceous foods, with the addition of eggs in small quantities, white meat in very small quantities (never before five years of age), and stewed fruit. Bread is replaced by toast and biscuits.

Secondly, the child is made to rest, as we have noticed how often the nervous system of children is affected, and resting in bed is the best sedative. Rest, even with older children, is essential for two hours after every meal, and during that time a hot bottle, linseed poultice or an electric cushion is laid on the abdomen.

Thirdly, we administer an alkaline remedy, and here again experience has shown us that sodium bicarbonate is the best, and we give doses of from quarter to half a teaspoonful in half a glass of hot water, three-quarters to one hour after meals. In exaggerated cases of hyperacidity, we add "alucol," a Swiss preparation of aluminium hydrate, which has produced us the best results. This is given from half to three-quarters of an hour before each meal in a little hot drink. It acts as a sedative on the gastric membrane and modifies the acidity.

As a result of this treatment, the digestive functions begin to reassert themselves, the general health improves and the child puts on weight—in a word, the cure takes place. At the hospital where the treatment is combined with discipline, a complete cure is only a matter of a few weeks. Regular examinations repeated at varied intervals demonstrate this. I have witnessed, for example, in apparently serious cases, a surprising increase in weight.

As all these statements are based on numerous cases under observation, and in order to bring further weight to bear on my subject, my assistants have drawn up for me statistics on the 200 most recent cases of dyspeptic children under my care. From these statistics I am able to draw certain deductions, the more interesting of which I now place before you:—

(1) Hyperacidity, of which I have spoken to-day, is found in children of any age, even in an infant of fifteen months.

(2) This acidity varies from the slightly-marked to the very accentuated cases, this latter occurring frequently with children. The figure 2 per cent. being that of total normal acidity, I find figures greater than 5 per cent. and even 6 per cent. For example, 6·4 per cent. in the case of a boy aged 7, and 6·3 per cent. in that of a boy aged 9. None of these cases had originally been diagnosed as dyspeptic, but almost invariably as tuberculous.

(3) All these cases, even the most accentuated, respond rapidly to treatment.

I quote the following examples from my statistics:—

A	girl	aged	14	years	put	on	8	lb.	9	oz.	in	2	months
,,	boy	,,	9	,,	,,	,,	9	,,	10	,,	,,	2½	,,
,,	girl	,,	9	,,	,,	,,	4	,,	4½	,,	,,	1	,,
,,	,,	,,	14	,,	,,	,,	14	,,	14½	,,	,,	5	,,
,,	,,	,,	13	,,	,,	,,	10	,,	12	,,	,,	1	,,
,,	,,	,,	14	,,	,,	,,	7	,,	8	,,	,,	2	,,
,,	boy	,,	13	,,	,,	,,	8	,,	9	,,	,,	2	,,
,,	girl	,,	12	,,	,,	,,	12	,,	13½	,,	,,	5	,,

This, surely, is the best proof that cases so frequently sent to us for treatment as tuberculous are really dyspeptic.

Dr. ROBERT HUTCHISON said that those who had given special attention to the subject of dyspepsia in children were in agreement. Dr. Taillens had done well to emphasize what was so striking about dyspepsia in children, namely, that it often simulated something else. It might be said that in the adult, other things simulated dyspepsia, but in the child dyspepsia simulated other conditions. In the adult, gall-stones, for example, simulated dyspepsia, while in the child dyspepsia did not come as an obvious visible dyspepsia, but masqueraded as something else, particularly as tuberculosis, with wasting, cough, and other symptoms. Over and over again he had seen, as Dr. Taillens had seen, cases sent to hospital under the belief that they were cases of tuberculosis, whereas the trouble was dyspepsia.

With regard, however, to the statement that dyspepsia in children was nearly always a functional complaint, there was a difference of opinion. In a fair number of instances in this country he (the speaker) thought there was a catarrhal element, a gastritis, possibly acid, and if the mucous membrane of the stomach were examined, one probably would find catarrhal changes, because many originated from an acute attack of gastritis which later became more or less chronic.

Dr. Taillens had said that the stomach was the primary organ at fault in practically all the cases, and here again, he (Dr. Hutchison) did not know that the experience in this country would support that; possibly the case was different in Switzerland. He classed dyspepsia in children into three groups: (1) primarily gastric; (2) primarily hepatic; (3) primarily intestinal. He was struck in practice—perhaps especially in private practice—with the number of cases in the hepatic group, those who were being overfed with "liver foods"—rich milk and cream,—who became irritable, lost appetite, wasted and had offensive breath, and who immediately improved and put on weight when the "liver foods" were cut down. These children also had acidosis and attacks of cyclic vomiting. These he had always considered as primarily hepatic cases, perhaps of "hepatic inadequacy," of children born with a liver which was not functioning very efficiently. And one could not help recognizing the primarily intestinal group. These correspond closely with Professor Taillens' description of children whose main complaint was constipation and the passage of mucus, with colicky pain.

Dr. Taillens had made the interesting statement—evidently after a good deal of investigation on the point—that hyperchlorhydria was of frequent occurrence; he spoke of an acidity of 6 per cent., which was a very high figure for anybody. But even if that high acidity was found, it did not necessarily follow that this was the cause of the symptoms. He would have thought that one of the most striking conclusions of modern gastrology was that these variations of acidity in the stomach, either upwards or downwards, i.e., hyperchlorhydria or achylia, were of comparatively little importance, so long as the stomach was performing its motor functions properly. He (Dr. Hutchison) would have thought that defective motility was responsible for a certain number of cases of primary gastric trouble in children.

There must be agreement with Dr. Taillens' treatment, because it had been proved by his success. It was only fair to point out, however, that with a different diet, but adopting the same hygienic measures, physicians in this country achieved just as great a measure of success. If one took these dyspeptic children from home and sent them to the seaside for two or three weeks, they at once began to eat better and to put on weight.

With regard to diet, in this country there was general agreement that a large number of the cases of dyspepsia in children were due to excessive consumption of carbohydrates, and success was achieved chiefly by cutting down starchy and sugary foods. That, of course, might be a question of national habit. Few observers at any rate seemed to have made so many accurate estimations of the gastric secretory function as Professor Taillens had done.

CASES AND SPECIMENS.

Congenital Short-neck.—ERIC PRITCHARD, M.D.—F. F., female, aged 1 year and 8 months.

Although there is no definite webbing between the mastoid process and the acromion process, the case appears to be an atypical example of Klippel Feil's disease, or web-neck (fig. 1).

A skiagram shows definite congenital deformity of the lower cervical and upper dorsal vertebræ, which appear to be bifid, and there are either small cervical ribs, or enlarged transverse processes (fig. 2).

The child has had right facial paralysis since birth, and there is a congenital nodule on the right cheek. There is also a right corneal ulcer, now improving under treatment.

Tumour of right side of neck (?) Teratoma.—ERIC PRITCHARD, M.D.—B. P., a girl, aged 4 years and 2 months, came to Infants' Hospital, October 11, 1927, with swelling on right side of neck, of four months' duration. Provisional diagnosis was cystic enlargement of thyroid or thymus, and the mother was instructed to bring the child six months later for further examination.

On May 3, 1928, patient was again brought to the hospital, having gained half a pound in weight (35 lb. 6 oz.). Swelling had completely disappeared from original situation. Patient was in perfect health, except for a slight cough and a small quantity of sputum. A fullness was now found about the right clavicle, which bulged slightly,

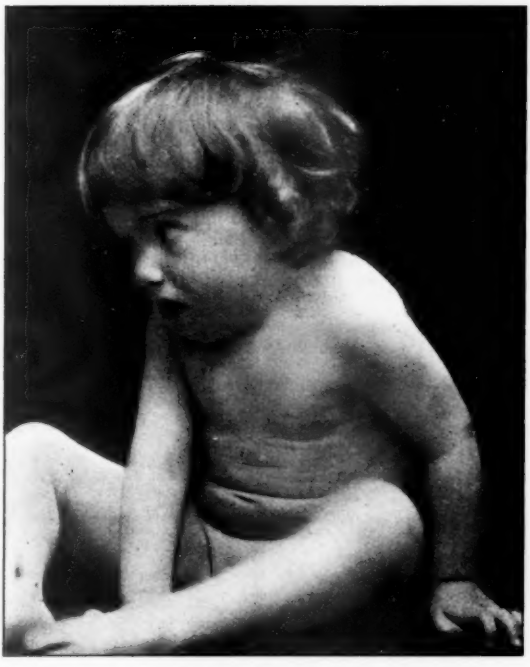

FIG. 1.—Congenital short neck. Klippel Feil's disease.

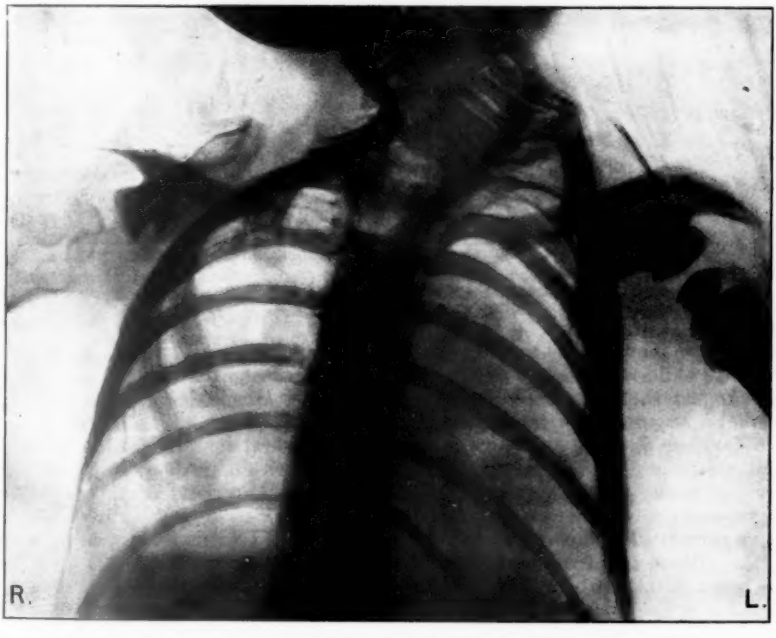

FIG. 2.—Skiagram showing malformation of cervical and upper dorsal vertebræ.

as did the upper part of the right side of the thorax. There was also slight distension of the superficial vein on the front of the upper part of the right side of the chest. There was extreme dullness to percussion over the upper two-thirds of the front of the chest, with slight inspiratory stridor. Posteriorly, there was almost complete dullness to the level of the sixth dorsal spine, with extreme muffling of breath sounds. Skiagram showed dense circumscribed opacity over the area of dullness, giving an appearance of a cyst or empyema which did not conform completely with the position of the upper lobe. The lower external boundary was particularly sharply defined, suggesting the limits of a capsule.

On March 26, 1928, child admitted as in-patient, with provisional diagnosis of sarcoma of lung; general condition appeared unchanged since May 3. While

Radiogram showing tumour on right side of thorax. Arrows from above downwards, in order, indicate: (1) Dislocation of œsophagus to the right; (2 and 3) pervious bronchi and bronchioles mapped out by injection with lipiodol.

in hospital the temperature varied from 97° to 100° F., and there was a slight cough. On June 12 child referred to Radium Institute for treatment.

On August 6, 1928, re-admitted to the Infants' Hospital and sent to Radium Institute on August 16 for further examination.

On October 6, again admitted to hospital. General condition still satisfactory. Weight stationary, and there was a slight cough, with a small quantity of blood-stained sputum. Right arm was a little swollen, but not œdematous. X-ray examinations showed no change; von Pirquet's test was negative; tuberculin test negative; hydatid complement fixation negative; Wassermann reaction, weakly positive. Blood-count showed: hæmoglobin, 80 per cent.; red cells, 4,680,000; colour index, 0·095; white cells, 7,300.

[*Differential Count.*—Polymorphonuclears, 69·5 per cent.; small lymphocytes, 20 per cent.; large lymphocytes, 4·7 per cent.: eosinophils, 4·3 per cent.; hyalines, 1 per cent.; basophils, 0·5 per cent.]

Examination under lipiodol and X-rays showed open bronchus to lower lobe; no indication of bronchi in tumour area. Trachea displaced slightly to left.

Needling of second right interspace, in nipple line to depth of one and a half to two inches, revealed some resistance on passing through pleura or capsule. Deeper penetration was apparently into soft tissue. Blood-stained fluid contained blood corpuscles only.

The maintenance of good health during the last twelve months and the apparent stationary character of the swelling for the last six months, combined with the maintenance of weight, militate against a diagnosis of sarcoma.

Discussion.—Dr. BERNARD SCHLESINGER said that early in the year he had shown a case of which Dr. Pritchard's case reminded him strikingly. The child was of about the same age and had similar signs in the chest, although there had never been anything abnormal visible in the neck. The X-ray appearances of the lungs were almost identical with those seen in Dr. Pritchard's skiagram. A diagnosis of mediastinal dermoid had been made, and needling of the tumour had only produced blood, as in this case. In most instances the dermoid arose from the mediastinum and not in the lung itself, and this might account for the lymphatic and venous obstruction of the arm and neck, and might easily have caused stridor by pressure on the trachea. The signs in the lung in this case—a resonant percussion note and good air entry at the base, with distant tubular breath-sounds and dullness over the upper part of the right side of the chest—were more compatible with collapse of part of the lung from pressure, than with disease of the lung substance. A lung lesion of such long duration would almost certainly have resulted in more distinctive symptoms than this patient complained of. Confirmatory evidence of the actual site of the tumour might possibly be obtained by X-ray examination after the establishment of a partial pneumothorax on the affected side. If the tumour were in the lung it would tend to collapse with the lung, or prevent the lung from collapsing. If on the other hand it were outside the lung, the tumour would stand out in greater contrast after complete collapse of the lung by pneumothorax. This method had been attempted in his case, but, owing to pleural adhesions, a successful pneumothorax had not been obtained.

Nevertheless it might be worth trying in this case in order to establish the diagnosis definitely. He did not advise operative treatment here because of the fatalities that had almost invariably attended surgical interference in the past.

Professor F. LANGMEAD suggested that it would be well to explore this child's chest by an open operation. He supposed that by the term "dermoid" a teratoma was meant. He agreed that this was the probable diagnosis and that so well-defined and dense an outline in the radiogram made sarcoma most unlikely. He saw no means other than by open exploration of establishing the diagnosis, and assessing the prospects of treatment by removal or otherwise.

Dr. F. J. POYNTON (President) referred to the case of an infant aged four months, whom Dr. Eric Lloyd had asked him to see. The child had dyspnœa, and there was dullness across the front of the chest, and the appearance in a skiagram was rather like that of a pericarditis the wrong way up. He had found a fluid thrill going from one side to the other, and had advised chest aspiration. A clear fluid had come at first and had continued to do so for some time; at last a milky fluid had been obtained. A sure diagnosis could not be made, and several times operation was discussed. Eventually the child died, and a specimen from the case was now in the Royal College of Surgeons' Museum. It showed a teratoma in relation to the mediastinum. In that instance an operation would have proved fatal. He (Dr. Poynton) was not, however, applying the lesson in the present instance, but referred to the case in connexion with the suggestion of teratoma.

Muscular Hypotonia with Hypoglycæmia.—C. WORSTER-DROUGHT, M.D.—J. P., male, aged 8 years, brought to hospital because he was unable to balance himself properly, and was backward at school.

History.—The patient was an eight months' child, labour having been induced because of a contracted pelvis. He weighed 7 lb., but was very fragile at birth; did

not sit up until twelve months old; walked at three years, and first talked at that age. Had control of the bladder at usual time. He made slow progress at walking and talking between ages of 3 and 4, then progressed more quickly. When aged 7 went to school, but made slow progress. When writing, he can form letters and figures, "but does not seem to have sufficient control of his hand to keep them even." He tries to play games, but cannot catch a ball. His walking has never been perfect; he walks slowly and loses balance easily, and has had several bad falls. He can run, but has a peculiar gait when he does so.

Previous Health.—He had whooping-cough at 2 years; one "fit" at 3 years at beginning of measles; fracture of right leg due to losing balance at age of 7 years.

Family History.—No history of similar condition in family. Sister aged 13 years whose left upper eyelid droops.

Condition on Examination.—He is somewhat dull mentally and fails to obey commands for more than a few seconds. Pupils equal and react normally. Speech indistinct, with slight explosive quality and some slurring of words. Muscles of face are hypotonic. Other cranial nerves normal.

Upper limbs: Both arms very thin; motor power poor, but greater on right side than left; muscle tone much decreased; tendon reflexes present and equal. Lower limbs: Motor power poor, but greater on right side than left; muscle tone much decreased: knee and ankle jerks brisk and equal; no ankle clonus; plantar reflexes flexor. Abdominal reflexes present and equal. Gait: Walks slowly and carefully with knees slightly bent, on a broad base and with movements imperfectly controlled; movements of left leg are guarded, left arm held close to the side and the right swings a little. Co-ordination: The finger-nose test is performed fairly well, somewhat better on the right side than on the left. Sensation normal.

Pulse, 96; blood-pressure, 80 systolic, 55 diastolic. General pallor of whole body. All the other systems are normal.

Skiagram of skull reveals nothing abnormal. Cerebro-spinal fluid normal. Wassermann reaction in both cerebro-spinal fluid and blood is negative. Sugar tolerance test: Following the administration of 25 grm. of glucose, the blood-sugar rose from 0·078 per cent. (resting) to 0·180 per cent. in half an hour, fell to 0·100 per cent. in one hour, to 0·060 per cent. in one and a half hours, and to 0·062 per cent. in two hours. At the end of two hours there was a trace of sugar in the urine.

The case is an example of pronounced muscular hypotonia of congenital origin in association with a low "leak point" for sugar. Clinically, the only distinguishing feature between such a degree of hypotonia and amyotonia congenita (Oppenheim's disease) appears to be that in the latter condition the deep reflexes are absent. On the other hand, I have recently seen two cases precisely similar to the child shown: in one of these the deep reflexes were actually absent and she showed the same degree of hypoglycæmia, with low "leak point." It would be interesting to know: (1) if such muscular hypotonia is constantly associated with a low "leak point" for sugar, and (2) if cases of true amyotonia congenita show a similar blood-sugar curve.

It might be suggested that the case shown is one of cerebral diplegia of a static-hypotonic type. Against this is the fact that muscular hypotonia is the main feature, while the degree of incoördination is small and quite sufficiently accounted for by the hypotonia.

Dr. ERIC PRITCHARD referred to two recent cases of marked hypotonia, in infants, in which the sugar curve had been taken, and in both it had corresponded more or less with that in this case; rather a rapid rise after the administration of sugar, and a fairly rapid fall to below the normal line, which persisted during period of observation. In the second case it was not recognized that it might be one of amyotonia until this curve was obtained.

Unilateral Lesion of Cerebellum (? Tuberculoma): Recovery.—I. M. ALLEN, M.D. (introduced by Dr. C. WORSTER-DROUGHT).—L. C., male, aged 6 years, admitted to hospital July 21, 1928.

History of Present Illness.—Three weeks before admission, he and another child bumped their heads together in the playground. Patient was hit on right side of head near eye. He was able to return home by himself but was not well afterwards. His appetite lessened and he lost weight. For one week complained of increasing frontal headache and earache but no aural discharge. For four days vomited suddenly without nausea.

Previous Health and Family History.—Nothing of importance.

Condition on Examination.—July 23, 1928. Patient very drowsy and lies in bed on left side; vomits at times quite suddenly without nausea. He cannot stand up properly and easily overbalances, most often towards the right side. Pupils equal, sometimes react briskly to light and sometimes sluggishly.

Optic Discs.—Some congenital fibrous tissue on discs which makes them appear rather blurred, but no definite papillœdema. Nystagmus on looking to left. Muscle power good. Tendon reflexes present, equal and normal. Abdominal reflexes brisk and equal. Plantar reflexes vary, sometimes flexor, sometimes extensor. Coördinated movements of arms slightly worse on left side; inability to perform repeated fine movements with left hand and arm. No intention tremor.

Circulatory, respiratory and alimentary systems normal.

Subsequent Progress.—For the first week he was slightly drowsy and complained of intense headache, occasionally shouted with pain, and vomited daily. At different times during the succeeding three weeks the following signs were observed: Pupils small but left larger than right; react normally. Optic discs: outline indefinite; veins full, tense and blue; physiological cups cannot be focused. Nystagmus in both directions. Speech: spacing of syllables and slurring of words. Sluggish corneal reflexes. Left facial weakness. Motor weakness of arms with diminished tone, left more than right. Decreased reflexes in right arm, absent in left. Diminished tone in left leg, with absent knee- and ankle-jerks. Right knee- and ankle-jerks sluggish. Plantar reflexes flexor throughout and abdominal reflexes present. Patient can stand with assistance but falls to the right. No spontaneous movements. Coördination: with right hand finger-nose test performed well with eyes both open and closed; with left hand there is exaggerated intention tremor which prevents finger reaching the object. In left arm decomposition of movements, dysmetria, past-pointing and great difficulty in carrying out repeated alternating movements. Spontaneous deviation of the left arm to the left. Sphincters normal. X-ray examination of skull negative.

After the fourth week in hospital the patient began to improve, and by the end of the fifth week there was considerable improvement in the nystagmus and the muscle tone and incoördination of the left side. For some days he continued to walk on a broad base and to deviate to right, but by September 10 this latter symptom had disappeared.

October 9, 1928.—During convalescence the weight has increased by six pounds. There is very slight nystagmus on looking to the left. Optic discs unchanged. Very slight tremor of left hand. Co-ordinated movements are performed with ease. Muscle tone in the left arm still less than in right. Gait: Walks quite freely, but still on a broad base. General health much improved.

Specimen: Erosion of Internal Jugular Vein in Scarlet Fever.—J. D. ROLLESTON, M.D.—The specimen shows an erosion of the left jugular vein, lying in a mass of inflamed cervical glands.

The patient, a female infant, aged 2 years and 4 months, had a severe attack of septic scarlet fever, characterized by ulceration of the tonsils, palate and tongue, profuse nasal discharge, suppurative cervical adenitis, broncho-pneumonia, and continued high fever.

The glands at the left angle of the jaw were opened on the twelfth day, and those on the right side ten days later.

Some oozing of blood from the left wound took place on the twenty-fourth day of disease, and in the early morning of the twenty-sixth day there was a profuse hæmorrhage. This was stopped by plugging, but death took place about an hour and a half later.

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Discussion.—Dr. ROLLESTON said that erosion of the neck vessels giving rise to fatal hæmorrhage was a rare event and very seldom occurred except in connexion with scarlet fever and gangrenous angina. Sixteen years ago he had shown at a meeting of the Section a specimen from a case of fatal hæmorrhage in ulcerative sore throat, in which the bleeding vessel had not been identified but was probably in the laryngeal portion of the pharynx. In scarlet fever the vessels liable to be involved were the external carotid, internal carotid, superior thyroid artery, lingual artery, external jugular and, as in the present case, internal jugular vein. The hæmorrhages might take place from the mouth as in his (Dr. Rolleston's) previous case, the ear, as in another case he had recently seen, or from a wound in the neck, as in the present case. The complication was usually fatal, but Pepper, May, and Rivers had recorded cases in which recovery had taken place after ligature of the common carotid, and cases in which the hæmorrhage had been controlled merely by compresses and the patients had recovered had been reported by Snow and Hosmer. Early suppuration of the cervical glands to which the erosion of the vessel was due was a well recognized but uncommon complication of scarlet fever, having been noted in only 0·68 per cent. of 21,000 cases of scarlet fever admitted to the Metropolitan Asylums Board Hospitals. By a curious coincidence he (Dr. Rolleston) had seen only six months previously a fatal case of hæmorrhage from the ear of an infant, aged 11 months, secondary to a cervical abscess, but the source of the hæmorrhage was not identified post mortem. These were the only examples of fatal hæmorrhages from erosion of neck vessels in scarlet fever which he had seen in over twenty-seven years' experience of fever hospital work.

This case was further of interest in that septic attacks of scarlet fever, especially those which derived no benefit from scarlet fever antitoxin, of which two large doses had been given, were very uncommon at the present time.

Dr. F. J. POYNTON (President) said he had seen two examples of these hæmorrhages; one in a case of ulcerative throat. In the other case the boy had fallen on his head and damaged the carotid artery where it entered the base of the skull. He had recurrent hæmorrhage in the throat, the cause of which had not been diagnosed, and which had proved fatal.

Dr. J. D. ROLLESTON said that similar cases of fatal hæmorrhage in scarlet fever due to opening a hæmatoma under the mistaken belief that it was a tonsillar or cervical abscess had been recorded by Huber[1] under the name of "hæmatoma scarlatinosum" and more recently by Griffiths and Riddell.[2]

Anæmia and Jaundice in a Newborn Child.—HAZEL H. CHODAK GREGORY, M.D.—Male child, aged 6 months. Shown at meeting of Section, 1928, when aged 3 weeks, suffering from extreme anæmia, with jaundice and an enlarged spleen.

Subsequently had large hæmorrhage into scrotum and smaller hæmorrhages elsewhere, and passed blood in fæces and urine. On two occasions a streptococcus was found in the urine. The mother was then discovered to be suffering from a severe streptococcal pyorrhœa, but during the process of cultivating these organisms for the purpose of classification the growth died, so that it was not established whether the two infections were identical.

A polyvalent antistreptococcal serum was given to the child under the skin. The dose was not repeated as the administration had resulted in an enormous hæmatoma. From that time recovery began to take place, the spleen steadily diminished in size, and the blood-count became normal. The patient is now a normal healthy child.

1 *Deut. Arch. f. klin. Med.*, 1871, viii, 422.
2 *Glasgow Med. Journ.*, 1910, lxxiii, 23.

I still do not feel certain about the diagnosis, but it is tempting to think there was a streptococcal septicæmia, though the temperature was not raised. If that was the case, one wonders why more babies do not have streptococcal septicæmia, because it must be common for pregnant women to have septic pyorrhœa. I have not thought of another diagnosis for the case.

Dr. BERNARD SCHLESINGER said there might be a combination of two conditions. The cure of hæmorrhagic disease of the newborn, by a single dose of human or blood-serum, was now universally known. Recently in the *Guy's Hospital Reports* (1928, 78, 199) Hampson had reported a series of cases of congenital jaundice. The cases were mostly familial and often fatal, but curable if serum were given. He wondered whether in Dr. Gregory's case these two pathological conditions were both present, and though it was tempting to attribute the success of the treatment to the anti-streptococcal serum administered, possibly ordinary horse serum would have had the same effect. Most streptococcal septicæmia that he had seen in infants had proved fatal in spite of all forms of treatment.

Pseudo-hermaphroditism.—T. VINCENT DICKINSON, M.D.—Patient, child of Italian parents, born at the Italian Hospital, London, February 19, 1928. The birth was premature, a few days before the mother's death from acute double lobar pneumonia. The infant weighed 3 lb. 2 oz. at birth, and showed an abnormality of the external genitalia, which became more evident as it grew older. Now it weighs 10¼ lb. and is fairly well developed. The genital organs look, at first sight, like those of a female, but on closer examination it will be noted that there is a complete absence of

Pseudo-hermaphroditism.

an introitus vaginæ, and that the clitoris is replaced by a hypospadiac penis, in which the glans and prepuce may be distinguished. From time to time a pea-like body can be felt in the right inguinal canal, which probably represents an undescended testicle. At other times this body cannot be felt, having slipped back into the abdominal cavity. No such body has ever been felt on the left side. The child was baptized as a girl, but it seems that we have to do with a male, showing a cleft scrotum, hypospadias and undescended testes.

The PRESIDENT said that at the last International Congress of Medicine he had shown a patient who had been brought up as a girl but had proved to be a boy.

Nævus and Left-sided Hemiplegia.—T. BRUSHFIELD, M.D.—B. G., a girl, aged 12. Admitted to Tooting Bec Hospital, October 12, 1920 (aged 4 years and 4 months).

Family History.—Father died at age of 36; mother at age of 32, at birth of patient, who is second child. Elder child (male), normal. Father's mother and sister died insane. Mother's parents French.

Patient's History.—Labour at birth was prolonged and difficult; patient was asphyxiated by cord. Arm paralysis noticed at birth; mental defect noticed at age of 3 years. In early childhood had radium treatment of nævus. Wassermann reaction, positive. Patient has had scarlatina while in hospital; her mentality has somewhat improved, but she is still confined to a chair all day, is very babyish and not clean.

Nævus and Hemiplegia. Shown for Comparison with previous Case.—T. BRUSHFIELD, M.D.—H. P., male, aged 67.

General History.—Family history unobtainable. Patient was admitted to Leavesden Mental Hospital on October 27, 1910; a report from that institution discovers him as an imbecile of low grade, a cripple with a right hemiplegia, and exhibiting a large "port wine stain" which covers his left eye, nose and forehead.

Transferred to Tooting Bec Hospital, December 14, 1926 (Dr. Brown). Mental state was that of a quarrelsome inconsequent imbecile, with very low intelligence, suspicious, deluded and irrational in manner; he could feed himself and get about in a wheeled chair.

His hemiplegia is exemplified by contractures of the right arm, hand and leg; the hæmangioma involves the left sclerotic coat. There is arterio-sclerosis and a high tension pulse.

Radiographer's Report.—At Dr. Brown's instigation, a film was exposed to a lateral view, L.—R., of the patient's head.

The film shows a worm-like convoluted figure, extending like an inch-broad ribbon from the frontal pole to within an inch of the tentorium—following the convexity of the cerebral hemisphere at about an inch below the level of the superior longitudinal fissure. The posterior extremity of this band widens considerably and there appears to be an offshoot opposite the postcentral region directed towards the petrous area.

Apart from this abnormality, the upper surface of the skull base departs very markedly from normal configuration: there is no definite delineation, no sella turcica, no frontal bed visible. The frontal and sphenoidal sinuses, and the antrum, appear abnormally capacious, and the calvarium is thinner than normal.

[The case is exhibited by the kind permission of Dr. Beresford, Medical Superintendent of Tooting Bec Hospital, and I owe the discovery of this example to Dr. Brown's assiduity.]

Dr. F. PARKES WEBER said that these were very fine illustrative cases of the syndrome in question, and he had published a still more striking example (1922) in the *Journal of Neurology and Psychopathology*, with a photograph.[1] At that time he could not discover any record of a case exactly like it. Probably, however, it would be found that such cases had been published in various countries; they could scarcely fail to attract special notice. Last summer he had the opportunity of again seeing his case. The patient was now aged 28, and although she had not learned to read, she was far from being an idiot, and her emotional life was apparently normal.

It seemed that the intracranial pathology of this "Brushfield and Wyatt's syndrome" consisted mainly in the presence of a more or less diffuse meningeal vascular nævus, sometimes at least of the racemose angioma type. He recently had the opportunity of having a new Roentgen skiagram of the brain of his case taken, and the appearance was as

1 F. Parkes Weber, "Right-sided . . . Congenital Spastic Hemiplegia," *Journ. Neur. and Psych.*, 1922, iii, 134-139.

if the whole of the diseased side of the brain were bound down by something, e.g., a diffuse angiomatous condition in the meninges, which had partially calcified, so that it could be clearly defined by ordinary X-ray photographs. In a case recently published in Germany of a meningeal racemose arterial angioma the post-mortem examination showed a deposition of calcium in part of the growth (Nielsen, *Klin. Wöchenschr.*, Berlin, 1928, vii, 2013).

He (Dr. Weber) would group cases of meningeal vascular nævus into the following classes: Those in which the symptoms were due to a unilateral irritative lesion, with epileptic attacks of Jacksonian type, which might first occur a few or many years after birth: such cases might or might not be associated with nævus of the skin; (2) the cases in which the angiomatous condition was much more extensive, the lesion being a paralytic rather than an irritative one: in this second group there was congenital or infantile cerebral hemiplegia, which, if associated with the presence of more or less extensive cutaneous vascular nævus, constituted "Brushfield and Wyatt's syndrome"; (3) a type like one of the two preceding, but in which, instead of ordinary vascular nævus there occurred Pringle's telangiectatic type of adenoma sebaceum of the face. An excellent example of this third type had been published by D. M. Greig (*Edinburgh Med. Journ.*, 1922, New Series, xxviii, 105). The facial adenoma sebaceum would have led one to expect tuberous sclerosis, rather than meningeal angioma, as the cause of the cerebral symptoms.

In his (Dr. Weber's) own case prognathism of the simian or anthropoid type was a marked feature, and in at least two other cases there was unilateral congenital glaucoma or buthalmos.

Dr. BRUSHFIELD: The patient in this case was prognathous, and both the children had congenital glaucoma.

Specimens from a Case of Congenital Deficiency in Abdominal Muscles.—ALAN MONCRIEFF, M.D.—*Clinical Notes.*—J. R., male, aged 6 months, admitted to hospital, under the care of Dr. Donald Paterson, suffering from pneumonia in both lungs, from which death occurred. First child of healthy parents, born at seven months by forceps delivery. It was noticed at birth that "he had no muscles in his stomach" and he was always very constipated. No difficulty with micturition. He presented the curious appearance indicated in the photograph (fig 1). There was apparently no abdominal musculature at all in the front and sides, though the spinal muscles seemed to be normal. No resistance to palpation; the abdominal viscera were easily felt. The bladder was felt as a globular mass above the pubes, and remained easily palpable, even after urine was passed. The testicles were undescended and could not be felt in the inguinal canal. The chest showed a marked funnel-shaped deformity, the lower end of the sternum being drawn inwards and upwards. The abdominal surface was wrinkled in appearance, with vertical linear markings, and at the bottom of one of these folds the umbilicus was present as a vertical slit. The skull was asymmetrical.

Post-mortem Findings.—Abdominal wall consisted mostly of skin, thin layers of fat and connective tissue and the peritoneum, a few strands of muscle in upper part of the rectus sheath and also scattered in other situations; pneumonia in both lungs; heart normal; cæcum had a mesentery and was lying on left side of abdomen, with appendix in front of ileo-cæcal junction; large Meckel's diverticulum; liver and spleen normal. The genito-urinary apparatus (figs. 2 and 3) showed most abnormality; bladder attached to anterior abdominal wall by well-marked mesentery and urachus still attached to apex of bladder; ureters grossly dilated and convoluted; obstruction to passage of probe into either ureter from bladder and no obstruction in urethra; no phimosis; kidneys enlarged, but with very little dilatation of pelvis; testicles situated in the iliac fossa on each side, external to ureters.

Comments.—The first case of this curious condition appears to have been described by R. W. Parker in 1895. Since then various authors have contributed examples, and in all sixteen cases have been put on record. [L. Guthrie (1896), W. Osler (1901), Stumme (1903), Garrod and Davies (1905), Mollison (1909), Thatcher (1913), Bellingham Smith (1913), Ikeda and Stoesser (1927).] Three theories

have been put forward to account for the deficiency of the abdominal muscles and the abnormal condition of the urinary apparatus:—

(1) That both sets of abnormalities are due to the same cause, namely a developmental error. Osler favoured this theory and quoted the opinion of Bardeen in support. He held that there were disturbances in the circulation of the embryo which could interfere with the development of the bladder and interior wall.

(2) That the condition of the bladder and ureters is secondary to the failure of development of the exterior abdominal wall (Guthrie). Because of the absent abdominal muscles the bladder was unable to contract properly and always contained excess of urine which found its way back into the ureters.

FIG. 1.—Congenital deficiency in abdominal muscles.

(3) That the condition of the exterior abdominal wall is secondary to the dilatation of the bladder. This theory was put forward by Stumme and accepted by Garrod and Davies, who pointed out that the lower, central portion of the abdominal wall was the part most affected, and that the abdomen gave the appearance of having been greatly distended. Stumme suggested that while the child was still a fœtus there was some temporary obstruction to the flow of urine which disappeared before birth. Garrod and Davies found it difficult to understand why the obstruction had not persisted and they were more inclined to accept Stumme's alternative view that the attachment of the bladder to the anterior abdominal wall produced an upward drag which kinked the intramuscular portion of the ureters, thus causing obstruction to them and possibly also causing kinking of the urethra.

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FIG. 2.—Showing genito-urinary system.

FIG. 3.—Showing "mesentery" attaching bladder to abdominal wall.

Discussion.—Dr. JULES TAILLENS said that early in the year he had seen a case of this kind in a boy a few months old. Death had taken place from pneumonia, the usual ending in such cases. Almost without exception, the cases were in boys. Out of seventeen cases in the French and German literature only two were in girls. It was nearly always possible to find traces of muscles, and this seemed to show that at the beginning of life, muscles were present, and that the trouble developed later.

Dr. ERIC PRITCHARD said that he had had two such cases. In the first the diagnosis had been very difficult. It was in a male baby, and there was a tumour in the left inguinal canal, cystic and fluctuating. The child had died the following day, and the post-mortem examination had shown a condition much like that in Dr. Moncrieff's case; the left ureters had prolapsed. There was obvious hypertrophy of the bladder wall and almost complete absence of the recti abdominis.

A further explanation had been put forward (he thought, by the late Dr. Leonard Guthrie) that the association of want of muscular fibre in the abdominal wall and the double hydro-ureter was one of cause and effect; the bladder, not having the assistance of the recti in urination, had to do all the work itself, and so became hypertrophied. Owing to that and the oblique opening of the ureter into the bladder, the orifice became obstructed, hence the double hydro-ureter. In passing a probe through the bladder wall, the thickness of muscle tissue was found to be about half an inch. The obstruction would explain the backward pressure and the double hydro-ureter. In both cases there were small stigmata of degeneration.

Renal Dwarfism.—C. E. KELLETT (for Dr. DONALD PATERSON).—V. M., male, aged 10; 35 in. high and weighing 24 lb. (Normal height 50 in., weight 60 lb.)

Patient was a full-time baby; breast-fed for the first three months, and seemed, until the age of 2 years, a normal child, if somewhat backward, his first tooth cutting during his tenth month.

Renal dwarf. Photographed with normal boy for comparison.

From the age of 2 till that of 5 he had frequent fits, two or three a day. These began with vomiting and would sometimes last for an hour or two; during these fits the child was cyanosed and unconscious. Since the age of 5 the fits have been much milder and less frequent, occurring only about once a month.

The child has always suffered from occasional bad headaches, and from time to time has attacks of drowsiness His appetite has been good, he sleeps well. He has been very thirsty.

The urine is copious, he passes about 30 oz. a day, of low specific gravity—1006-1008—and containing a small amount of albumin, only 0·6 per cent. of urea is present.

The blood-urea is 158 mgm. per 100 c.c., the inorganic phosphorus 3·82 mgm. per 100 c.c. (normal 4 to 5·5), and calcium 10·4 mgm. per 100 c.c. (normal 10 to 11). Though there is thickening of the radial vessels, the heart is not enlarged and the blood-pressure remains in the region of 120 systolic and 80 diastolic.

Kidneys not palpable. Skiagrams revealed bony changes typical of the malady.

In the cases described by Parsons[1] the phosphorus content was found to be considerably increased and the malady aggravated by the application of ultra-violet light therapy. In this case with a low blood-phosphorus content, this form of treatment has roved of benefit.

Dr. HELEN M. MACKAY said she had been interested to hear that the blood-phosphorus in this case was subnormal, as in the ordinary case of rickets, and not, as in some of the cases of "renal rickets" published, above the normal. At present she had a patient, aged 9, who had renal disease with rickets, and who also showed blood phosphorus slightly below normal. Parsons believed that the cause of the development of these bone changes in renal disease was the retention of phosphorus, and a consequent disparity between the level of the blood calcium and the blood phosphorus, from which resulted the failure in proper deposition of calcium phosphate in the growing bones. Obviously some other explanation must be sought to account for the development of rickets in such cases as the one shown.

Meningo-Encephalitis.—I. M. TITCOMB (for BERNARD SCHLESINGER, M.B.).—A boy, aged 6, admitted to hospital complaining of back-ache, drowsiness and blurring of speech for one week. No past illnesses except measles; vaccinated at birth.

August 9, 1928.—On admission: Temperature 99, pulse 90 respirations 24. Pale and drowsy, but easily roused and speaks sensibly. On examination: Head retraction and rigidity. Kernig's sign present. Knee-jerks and ankle-jerks accentuated. Both plantar reflexes extensor. Discs normal.

August 13, 1928.—Less drowsy and less head rigidity. Kernig's sign still positive. Plantar reflexes still extensor. Abdominal reflexes all absent.

August 15, 1928.—General condition improving. Abdominal reflexes all present.

August 20, 1928.—Quite comfortable; no head retraction, but Kernig's sign still slightly present. Plantar responses: right, extensor: left, flexor.

September 13, 1928.—Plantar responses both flexor. Kernig's still slightly present. General condition very good, except for occasional attacks of pain in the back, relieved by lumbar puncture.

September 29, 1928.—Discs blurred and slightly swollen, especially nasal side on the right, where the margin is very indistinct.

October 10, 1928.—General condition still good. No Kernig's sign. No headaches, but occasional attacks of pain in the back and side. Slight coarse nystagmus to the right, otherwise nothing abnormal found in the central nervous system.

Reports on Cerebro-spinal Fluid.

August 9, 1928.—Under pressure. Fifty-two cells per cubic mm.: lymphocytes. Glucose present. Chlorides 0·72 per cent. No organisms seen; sterile on culture.

August 30, 1928.—350 cells per cubic mm.; 90 per cent. lymphocytes; Wassermann reaction negative, not under pressure; sterile.

September 29, 1928.—Fifty cells per cubic mm.; lymphocytes; sterile.

[1] *Arch. Dis. in Child.*, ii, 1.

Discussion.—Dr. F. J. POYNTON (President) said there had been many curious cases in which there had been a difficulty in deciding whether they were polio-encephalitis or encephalitis of some sort, or a tuberculous or other form of meningitis, or encephalitis lethargica. In some there was a curious tremor of the hands, and the present case seemed to belong to this group.

This was an instance of the remarkable way in which recovery from encephalitis could take place.

Dr. SCHLESINGER said that this condition somewhat resembled the meningo-encephalitis that might follow closely after vaccination. This was not an isolated case. He himself had had another patient with a similar condition, and his colleague (Dr. Bruce Williamson) had kindly allowed him to mention one of his cases, with similar clinical manifestations, in which the cerebro-spinal fluid showed as many as 950 cells per c.c. with polymorpho-nuclear cells and lymphocytes in equal proportions. Two views were held as to the exact ætiology of vaccinal encephalitis: (1) that it was a particular malady arising only in connexion with vaccinia; (2) that this type of encephalitis, which differed in many ways from encephalitis lethargica, occurred in epidemics. At such times the virus of the infection might be lurking in many children, who might, however, escape encephalitis; but vaccination in these cases might, in some unexplained way, provoke the virus into activity. The cases he had described, one of which had been shown, were possibly such cases of encephalitis. It was interesting to note that they had all occurred over a period when cases of vaccinal encephalitis were also prevalent.

Two Cases of Fibrocystic Disease of Bone.—C. JENNINGS MARSHALL, M.S.

—(I) A boy, L. A., aged 8 years. Admitted to Victoria Hospital for Children, December, 1926, for spontaneous fracture of humerus. Radiographic report by Dr. Rawlinson: "Avulsion of the greater tuberosity of the humerus: cystic appearance of the upper end of the humerus."

On Examination.—The upper end of the humerus was obviously expanded and there was clearly a solution of continuity.

Operation.—The bone was exposed through the delto-pectoral interval: the contents of the cavity—blood-stained fluid and fragments of granulation tissue containing sandy particles of bone—were scooped out; the thin shell of bone was resected subperiosteally, and the medullary cavity for a short distance below this level curetted. Above, the metaphyseal side of the epiphyseal cartilage was completely exposed by the disease. Internal splinting was now effected by a length of rib, resected subperiosteally; one end was inserted into the medullary cavity below, the other was split apart so as to support opposite sides of the epiphysis.

The operation was followed by oozing of blood from the wound and slight infection; two and a half months later two small sequestra were removed from the graft.

June 28, 1927.—*A subsequent radiogram* showed regeneration of the resected area of the humerus, the bone being irregular and sclerosed; there was a small central area of relative translucency, representing the site of the sequestrum. There was no extension of the fibrocystic disease down the humerus.

Movements of the shoulder good, apart from some limitation of abduction and rotation.

(II) A boy, J. W., aged 5 years. Admitted to Victoria Hospital for Children for pain in the left hip. Stated to have been twisted twelve days previously. Clinically no abnormality except expansion at the root of the trochanter. Radiographic report by Dr. Rawlinson: "Marked cystic condition of the left femur from level of epiphyseal line to lesser trochanter."

The diseased area was approached by gouging the femur on the outer side immediately below the trochanter. The contents of the cavity, straw coloured fluid, were evacuated; the walls were strongly curetted, small masses of gelatinous granulation tissue being removed. The scoop seemed to pass through the outer bony shell at no point. Particular attention was paid to the lower limits of the disease

in the femoral shaft. There was a moderate amount of bleeding from the cavity. This was now filled by pieces of subperiosteally resected rib, driven firmly in and wedged in place by slivers of bone. The opening in the femur was closed over by suture of the fascia.

Fibro-cystic disease of bone.

Radiograms (two months later) show little new bone formation—the grafts appear to be rather opaque.

Microscopic section shows blood-clot; striæ of bone embedded in granulation tissue and in process of erosion.

COMMENTS.

It is hardly necessary here to discuss the ætiology of fibrocystic disease of bone; but its site of election in the metaphyseal regions renders its treatment comparatively difficult, and the mechanical problems involved in the management of a case after eradication of the disease are particularly troublesome.

This is especially the case with the neck of the femur where any formal resection of disease would certainly result in crippling deformity in the region of the hip. Here it was essential to preserve bony continuity as far as possible in order to avoid the trouble of splinting bony reconstruction, etc. Whether this has permitted efficient treatment of the fibrocystic process is perhaps open to question. It is to be noted, from the radiographic evidence, that bone reformation so far, is much less active than in the case in which subperiosteal resection was performed. It may well be that the bony shell left behind is signally defective in osteogenesis, although not actively diseased. It must also be admitted that the grafts do not yet appear to have caused ossification of the clot filling the cavity around them. A further lapse of time, however, is desirable before pronouncing finally on this point.

Analysis of the radiograms in the case of the humerus shows that, without question, the regeneration has occurred in a preponderating part from the deep surface of the periosteum, although the secondary operation to remove small sequestra proved the survival, in the main part, of the graft.

The grafts were introduced in these cases for two reasons: (1) for internal splinting and (2) for the purpose of bone replacement and the stimulation of the osteogenesis. It seems clear that they have been more useful for the former object than for the latter; their value in the case of the humerus is obvious, and in the case of the neck of the femur they have at least given a feeling of security against collapse of the hollow femoral neck, weakened by the disease and by the curettage. Their use is fully justified by these results alone. The involved question of the survival of bone grafts crops up here, and it may well be asked how one can expect their survival under such conditions—where considerable parts, at least, of their surface are separated by clot from tissues capable of rapid union with the graft by granulations. In the humeral case it was possible to obtain a large area of contact with the periosteum; in the femoral, the area of contact must have been limited. Still it must be remembered that warm blood-clot reproduces the conditions of tissue culture, in which free tissues may live for extended periods without active blood supply; also that the invasion of blood-clot by granulation tissue is a much more rapid process than usually imagined.

While aware that sometimes the condition clears up spontaneously after fracture, I still feel that where subperiosteal resection does not lead one into insuperable mechanical difficulties, it is the most effective method of treatment, but that it should not be used in the neck of the femur, also that bone grafting is of distinct value as an accessory, though not an essential.

Late Result in Case of Suppurative Arthritis of the Knee, Treated by Transverse Subpatellar Incision and Secondary Suture.—C. JENNINGS MARSHALL, M.S.—A girl, E. W., now aged 8 years, was admitted to the Victoria Hospital for Children when aged 1½ years, with suppurative arthritis of the knee. No notes are available, but from the charts it is clear that the child was desperately ill; the infection was streptococcal, the temperature remaining at from 103° to 104° F., and the pulse at from 140 to 150. Lateral incisions were made, and after a few days the joint was opened by a transverse subpatellar incision and fully flexed. Infection subsided by lysis in a fortnight, and extension was then applied until the knee was straight. Secondary suture of the divided parts was performed after a month, bipp being smeared on the surfaces. Healing occurred without incident. Shortly afterwards the child disappeared from observation. She was brought back to hospital on account of pain in the knee of some months' duration and increasing inability to get the joint out straight. The mother stated that the movements had been full until the child had diphtheria a year ago, when they began to get painful and stiff, and the condition had been greatly aggravated by a fall shortly afterwards.

The range is about 70 to 150 degrees. There is very little creaking. At the first examination the synovial membrane was hot and swollen and the joint line tender. It is clear that extension of the joint is not performed by the ligamentum patellæ, which is not united to the patella; the radiogram shows that it must have been divided very close to the bone, as there is a small osseous hole in its substance well below the knee-cap.

Further, the radiogram shows posterior luxation of the tibia and a beak in the anterior part of its epiphysis.

No stigmata of congenital syphilis. Wassermann reaction not tested. Tonsils have been removed.

The case is shown chiefly "for information." There is at present an infective inflammation of the soft joint tissues, and to this a proportion of the disability must be ascribed. Yet the formation of a flexion deformity must in part at least be ascribed to the imperfect union of the extensor apparatus. The lateral quadriceps extension, however, are very efficient; a point that seems of importance in the mechanics of the knee that one has never mentioned. The vasti come particularly into action during the last degrees of full extension of the joint (this is, of

course, easily capable of verification). Hence one can credit the statement that the range, until the onset of the reinfection, was satisfactory.

I do not think I would again resort to this method of dealing with an arthritis of the knee. A consideration of the numerous forms of treatment of suppurative arthritis reveals one striking fact—that their "be-all and end-all" is drainage of the lumen of the joint. I believe this is entirely wrong. The basal feature in the disease is a cellulitis of the soft joint strictures, the changes in the lumen and in the bone being secondary. Whatever we do to the lumen of a joint affects the synovial cellulitis only in virtue of the incisions made in the soft tissues to approach the lumen. The "joint drainage" incisions that, incidentally, result in wide openings in the soft tissues, prove the most successful in treating arthritis. Nearly all the pus we "drain" from joints is due to the tubes we introduce for that purpose: it is common experience that a most destructive arthritis may proceed to its conclusion almost entirely without synovial effusion. Cases of septic arthritis recently treated by free incision of soft tissues, subsequently kept well open, have seemed to me to give results infinitely superior to those obtained by dealing with the lumen.

Empyema of Maxillary Antrum.—DAVID LEVI, M.S.—G. P., female infant, admitted July 12, 1928, aged 18 days. Weight 6½ lb. Normal delivery.

History.—Swelling of left side of face and round eye for previous five days. Discharge from nostril and mouth. Also some diarrhœa.

Examination.—Temperature 100°. Circumscribed inflammatory swelling in region of left lachrymal sac. Eyelids swollen and puffy but no discharge from eye. Two sinuses present; one in roof of mouth in region of canine tooth and one behind this; both discharging creamy pus.

Treatment.—The antrum was opened through the nose and the sinuses were scraped. The nose was irrigated periodically and the swelling in the region of the lachrymal sac which filled with pus frequently was kept empty by digital pressure whenever the child was changed.

August 15, 1928.—Large sequestrum (shown) removed from nose.

August 31, 1928.—All sinuses healed and child well.

This case is the twenty-sixth of its kind to be recorded. Bass in the *American Journal of Diseases in Children*, 1928, describes two cases and gives a review of the literature.

Large Hairy Mole of Face. To illustrate a New Method in the After-Treatment of Skin Grafts in Children.—DAVID LEVI, M.S.—D. H., male, aged one year and a half. Admitted May 26, 1928, with large hairy mole of face.

FIG. 1.—Before operation.

FIG. 2.—After operation.

The mole was excised in two stages and a large Thiersh graft was applied in the ordinary way. After the graft was in position the whole area was sprayed with ambrine and left for ten days. When the ambrine was removed, the graft was found to have taken well.

This is an easy and clean method of fixing a graft in position and one that dispenses with the need for after-dressings. It is particularly suitable for small infants

Clinical Section.

[*November* 9, 1928.]

CASES.

Swollen Knee: for Diagnosis.—H. A. T. FAIRBANK, D.S.O., M.S.—T. G., aged 43, boilermaker's labourer, formerly in Army. Attended hospital in February, 1928, for swelling of left knee of two months' duration. Gradual onset. Knee knocked several times at work, but no definite injury. Syphilis denied. Knee was greatly swollen, hot, and slightly tender. The skin was red rather below the patella and also to its outer side. Synovial membrane appeared thickened, but no fluid in joint. Swelling seemed mostly peri-articular and of brawny consistence, except under area of redness, where there was fluctuation. Marked wasting of thigh. Wassermann reaction, negative. X-ray examination negative, except for slight osteo-arthritic changes.

A large extra-articular abscess opened below patella. Pus thick. No organisms. Cultures sterile. Wound refused to heal and swelling only slightly reduced. Three months afterwards a course of anti-syphilitic treatment was begun, with questionable benefit. Knee gradually improved, but still remained considerably swollen.

September 21, 1928, large subcutaneous collection of bloody fluid, with flakes of pus, aspirated from inner side of knee. October 3, 20 c.c. of similar fluid aspirated; October 17, 30 c.c.; on each occasion no tubercle bacillus or other organism found in pus. Cultures sterile.

Knee still shows much peri-articular thickening and marked limitation of movement. Coagulation time 2 min. 6 sec. (normal time 1 min. 45 sec. by Dale and Laidlaw's method).

The fluid aspirated is now largely blood, with flakes of pus in it. Actinomycosis has been suggested, but examination for that disease has not been carried out. I have no knowledge of actinomycosis affecting extra-articular tissues without having some connexion with the hollow viscera or the mouth. The effusion has not been injected into a guinea-pig. Unless we do something for him the patient will, I fear, lose his leg.

Discussion.—Dr. DONALD BRIMS said that on being questioned the patient had admitted that some years ago in India he had had a "wart" behind the glans penis (which he burned off), but he declared that he had not had gonorrhœa. He (the speaker) doubted this, and thought the condition, therefore, might be a gonococcal arthritis. Although the swelling did not appear to be in the joint, yet the patient experienced tenderness when both semilunar cartilages were pressed at the sides. The supra-patellar fossæ were filled, but not apparently the two, but the two infra-patellar fossæ apparently were not. He (Dr. Brims) would release the fluid (which he understood was hæmorrhagic) and replace it with oxyradon under the same pressure. Also he would give, by the Danysz method, Danysz' last production, the anti-gonococcal vaccine, No. 25.

Dr. F. PARKES WEBER thought the question of the disease of the knee being actinomycosis should be carefully entered into, as that disease could occasionally attack an extremity. A good instance of similar organisms attacking an extremity was the condition of so-called Madura foot, though that was of course not usually met with outside the tropics. The question was one of practical importance, owing to the possibility of trying iodide treatment.

Dr. HUGH S. STANNUS said the appearance of this case was not unlike one of Madura foot, but he had never seen a case of Madura knee. The case certainly resembled one of infection with a mycetoma or actinomycotic-like fungus. He would suggest trying the exhibition of heavy doses of iodide, one drachm four times daily.

Dr. J. W. CARR (President) said that this might be a case of actinomycosis; it recalled to his mind a case which he had seen some years before, in which, what appeared to be acute pneumonia had suddenly developed, followed in a few days by an empyema, which had been opened and drained, but obstinately refused to close up permanently. Eventually it was found that the condition was due to actinomycosis.

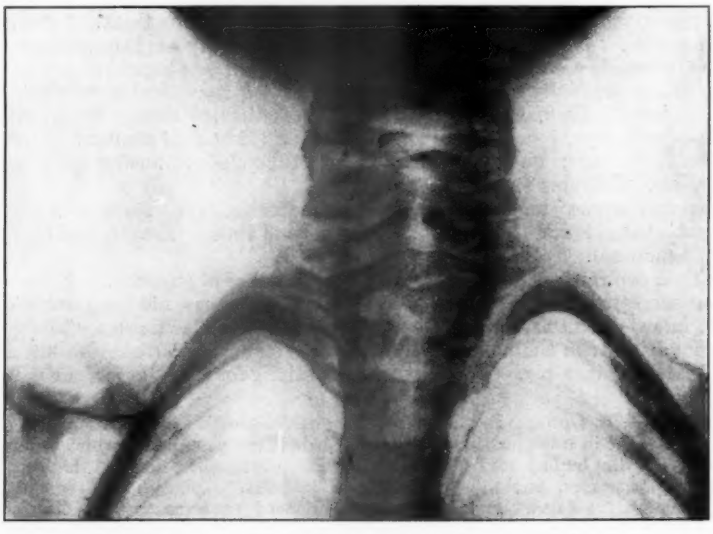
Congenital Deformity of the Spine.—CECIL P. G. WAKELEY, F.R.C.S.—T. H., a boy aged 4, was treated for a fracture of the right clavicle four weeks ago. This has united soundly in good position. At the age of one year he was satisfactorily operated upon for hare-lip.

There is some kyphosis and a slight left torticollis.

A definite hard lump about the size of an almond can be felt at the back of the neck on the left side, opposite the spinous processes of the seventh cervical and first dorsal vertebræ. It is not painful and appears fixed to the spinal column.

Skiagrams reveal a very incomplete and irregular development of the posterior neural arches from the third to the seventh cervical vertebra.

I thought this might be a case of congenital elevation of the scapula. As he has some torticollis on that side, what should be the line of treatment: should one take off that piece of bone or leave it alone?



Cervical spine showing irregular development of vertebræ.

Mr. H. A. T. FAIRBANK suggested that this case was of the type known as Sprengel's shoulder; it should be called congenital elevation of the scapula. He had had many cases of the kind; several with a bridge of bone or unossified cartilage anchoring the scapula to the spine. In some others there was an exostosis attached to the scapula, i.e., an attempt at a bridge, though an incomplete one. He believed that a case had been exhibited by Mr. Maynard Heath, in which there was an exostosis from the spinous process of one of the lower cervical vertebræ, and elevation of the scapula, and he thought the case shown was of the same type. He would have thought it was worth while to manipulate the shoulder every morning and evening to try to reduce the elevation.

In one specimen, now in the Museum of the Royal College of Surgeons, he had found the trapezius poorly developed. The child in that case had had cleft palate and absence of radius, bilateral elevation of the scapulæ, suppression of some cervical vertebræ, and wry neck.

His (the speaker's) dissection in that case had shown that the trapezius was absent on one side, and was represented by an aponeurotic sheath running downwards and outwards, more or less in a line with the rhomboids.

Acromegaly in a Woman.—C. WORSTER-DROUGHT, M.D.—H. G., aged 28, complains of headache, amenorrhœa and increasing weight.

History.—Gradual increase in size and weight for past five years. At onset her weight was 10 st., and it is now 17 st. Until five years ago, periods were always

regular; amenorrhœa since. Hands and feet have increased in size. Formerly she took gloves of size $6\frac{3}{4}$, now size 8; shoes formerly $6\frac{1}{2}$, now 8. Two years ago had to wear a larger wedding ring and since then it has been enlarged twice. Voice has become louder and deeper than formerly. Headache developed about two years ago; at first was occipital in distribution, more recently has become generalized and occurs almost daily.

Previous History.—Married at 17: one child, aged 8, alive and well; no subsequent pregnancies: no family history or previous illnesses of importance.

Physical Examination.—Skull massive, with exaggerated frontal malar and occipital prominences. Face large and full, with large upper and lower jaws, the

Skiagram showing enlargement of sella turcica.

lower projecting beyond the upper. Tongue large and flabby, wide throat, lower teeth not separated. Hands enlarged and square, with tapering fingers. Feet short and square. All bones of hands and lower limbs increased in size. General obesity, with maximum distribution in shoulder and pelvic girdles. Breasts full and pendulous and abdomen well-covered. The fat padding of limbs decreases distally. Skin, fine and smooth. Hair normal on scalp; very little on limbs and scanty in axillary and pubic regions. Optic discs are normal but visual fields show bilateral temporal restriction. Voice husky with tendency to masculine type. Pupils and all other cranial nerves normal. Deep and superficial reflexes normal. Heart and lungs normal;

blood-pressure: 120 systolic, 80 diastolic. X-ray examination of skull shows sella turcica considerably enlarged, measuring 17 by 10 mm. (normal average 12 by 8 mm.). Cerebro-spinal fluid shows no abnormality. Wassermann reaction both of C.S.F. and blood is negative. No glycosuria. Blood-sugar curve: resting, 0·075; half an hour after 50 grammes of glucose, 0·112; one hour after, 0·100; one hour and a half after, 0·093; two hours after, 0·086. No sugar in urine. Blood-count shows no abnormality.

The diagnosis of acromegaly has been made on the progressive enlargement of the hands, jaws, and soft parts of the face, together with the much enlarged pituitary fossa shown on X-ray examination. It is suggested that there is a functionally-active adenomatous enlargement of the anterior lobe of the gland. Such an enlargement, occurring in a closed space, must of necessity compress the adjacent soft parts, the pars intermedia and pars nervosa. This interference with posterior lobe function, as generally understood, is indicated by the obesity, by the smooth and comparatively hairless skin, and probably by amenorrhœa.

Discussion.—Dr. W. J. CARR (President) asked whether the colour vision had been investigated, since the field of impairment for colour might be more extensive than that for ordinary vision.

Dr. BERNARD MYERS said it had always to be remembered that there could be over-activity or under-activity of the pituitary, and perhaps at times opposite action of the two lobes of the gland. The result of the glucose-tolerance test in this case did not seem to favour the idea of pituitary deficiency. An interesting feature of the case was the amenorrhœa. Some time ago he had shown before the Section a case of adiposis dolorosa, in which amenorrhœa had been present for three years, with marked general weakness. The present patient told him she felt a little weak. The weakness and the amenorrhœa in his own case had ceased when whole pituitary gland was given. Two years afterwards the menses were still pursuing a normal course. Some patients appeared to pass from excessive to diminished action. In his case the pituitary extract had been administered orally. Had it been tried in this case?

Dr. WORSTER-DROUGHT (in reply) said that his patient did not show a complete bilateral hemianopia, there was only some limitation of each temporal half of the visual field. A colour-test of the visual fields had not yet been made, but he agreed that it would probably show more restriction for colour than for white.

The benefit the patient said she had derived from treatment must be, he feared, largely the result of suggestion. She had had one application of deep X-ray therapy and it was only tentative, but further applications were proposed.

The blood-sugar curve was subnormal and there were two antagonistic influences in its production: (1) The tendency of the increased anterior lobe secretion to lower sugar tolerance, and (2) that of the diminished posterior lobe secretion (or pars intermedia) to increase sugar tolerance. The result suggested that the latter influence was slightly predominant.

He thought that any benefit accruing from the administration of whole pituitary gland was entirely due to the posterior lobe element in it.

Congenital Polycystic Kidneys in an Adult with Arterial Hypertension and Angina Pectoris.—D. EVAN BEDFORD, M.D.—Male, aged 55. Heavy drinker. For six months has suffered from pain at the lower half of the sternum on exertion, especially when hurrying, going uphill, or after meals. The pain radiates across the chest and down the left arm, he has to stop quite still until it goes. He is not breathless. No abdominal or urinary symptoms.

Examination.—Apex beat in nipple line. Aortic systolic murmur and accentuation of aortic second sound. Arteries thickened; blood-pressure 205/115 mm. No signs of heart failure. Electrocardiogram shows normal rhythm and rate; left ventricular predominance and flat T-wave in lead II, negative T in lead III. Urine: S.G. 1010. Albumin present. No casts. Blood-urea: 112·7 mg. per 100 c.c. Non-protein nitrogen: 1'0 mg. per 100 c.c. Wassermann reaction, negative.

There is an irregular painless tumour in each flank, palpable bimanually in the loin. Patient was unaware of the presence of these abdominal tumours and has no pain or other symptom from them.

When first seen the abdomen was distended and I only felt the tumour on the right flank; on re-examination later, the left one was felt: the former is the larger. The urine has not been measured as the man is at present an out-patient.

He is at present able to do his work, which is clerical, in spite of his angina. I think that the nitrogen retention settles the question of what is going to happen to him. The pain has diminished since he took trinitrin tablets.

Discussion.—Dr. W. J. CARR (President) asked what quantity of urine was being passed daily by this patient, and whether a fatal termination was more likely to result from the heart condition or from uræmia, the latter being in his experience the more frequent cause of death in these cases.

Professor F. S. LANGMEAD said that a long period sometimes elapsed in these cases before symptoms arose. A patient of his, who had served during the South African War, had, at the age of 46, undergone an abdominal exploration for tumours, but during this time had been in apparently good health. Symptoms began at the age of 48. Death occurred about two years later.

Dr. F. PARKES WEBER said that to him it was not very astonishing that these cases of congenital cystic kidney carried on until the time when they rapidly went downhill. It seemed natural in the animal (including the human) organism for compensation to go on until suddenly a point was reached at which Nature gave up the struggle and the patient sank. It was nevertheless surprising how much renal glandular tissue was left between the cysts, even at a late stage, that was to say, in an enormous cystic kidney. It was possible this man's death, when it came, might not be due primarily to the condition of the kidney, but to the heart disease.

Dr. KINGSTON BARTON said he had always taken the view that the high blood-pressure and the so-called heart disease constituted the natural cure for the chronic destruction of the kidney. When Sir John Rose Bradford was experimentally investigating kidney conditions, by gradually excising more and more of the kidney, he brought it down to almost mathematical precision, and ascertained that not until only one-sixth of the kidney substance remained did the creature die. He agreed with the remarks of Dr. Parkes Weber. One thought of cases in which the contraction and thickening of arterioles was so great that the vessels seemed to have scarcely any calibre. When the present patient came to the end of his compensation, death would be sudden.

Dr. EVAN BEDFORD (in reply) said these patients usually die from uræmia or from cerebral hæmorrhage. Almost invariably there was a high blood-pressure. The clinical picture tallied with that of so-called chronic interstitial nephritis. This man's coronary arteries (and aorta) were probably affected by atheroma, for there was a systolic murmur, and a loud aortic second sound. The patient appeared now to have reached the stage at which the kidney substance was insufficient, and death would probably occur from uræmia.

Aplastic Anæmia.—LESLIE WITTS, M.D.—A. M., male, aged 34.—Complains of eruption of purple spots into skin, especially of legs. Had rheumatic fever, diphtheria and scarlatina in childhood. Was in the Army from March, 1915, to March, 1922, serving first in France, then in Malta, Egypt and Syria; had sand-fly fever and dengue; gonorrhœa, March, 1924; generalized syphilis, August, 1924; Wassermann reaction + +.

Treatment: One course of novarsenobillon (eight injections, 0·6 grm.) with contramin 0·25 grm., intramuscularly, between fourth and sixth N.A.B. Course of mercury, intramuscularly, four months; course of potassium iodide, one month; course of mercury, four months, intramuscularly; mercury with chalk pill, six months; course of mercury, three months, intramuscularly, to August, 1926.

Wassermann reaction not tested. No jaundice or other intolerance during treatment.

Two years ago (1926) brown patches appeared on both shins, extending from knees to ankles. Nine months ago he began to be short of breath; this symptom became very noticeable. Four months ago began to have successive crops of purpuric spots on arms, chest, legs and back; these faded gradually. About six months ago he scratched his hand at work and it bled for over half a day. Since then if he cut himself while shaving, the wound bled for about two hours; bleeding would often recur when he shaved the next day. He bruised very readily, and was so

much impressed by this that he hit his hand on the table to demonstrate it to a friend. During the last few months has had several attacks of epistaxis and oozing of blood from the gums. There has been no hæmaturia, hæmatemesis, or melæna. Weight unchanged. He is a fitter's assistant, working with iron, lead and copper; he does not handle any chemicals. No glossitis or acroparæsthesia.

A pallid but well nourished man, with numerous purpuric hæmorrhages, varying from pin-point size up to 1 cm. in diameter, and particularly thick over the legs and chest. A few petechial hæmorrhages also in the mucosa, palate and throat. Pulse about 90; regular. Temperature normal. Respiration 22. Blood-pressure 115/75. Tongue clean; pyorrhœa. No enlarged glands and no œdema. The nervous system is normal, but there are numerous hæmorrhages in both retinæ, with one large hæmorrhage in the left macular region. Heart normal in size and position; a systolic murmur at the base. Lungs clear. Abdomen normal. Spleen not felt. Urine normal. Fractional test meal: complete achlorhydria. Wassermann reaction negative.

Blood-count.—Red blood-corpuscles 1,600,000; hæmoglobin 28 per cent.; colour index 0·9; white blood-corpuscles, 4,240. *Differential:* Neutrophils, 30·5 per cent.; eosinophils, 2·0 per cent.; small lymphocytes, 58 per cent.; large lymphocytes, 4·5 per cent.; large hyalines, 5 per cent.; poikilocytosis and anisocytosis; polychromatophilia in a few cells. Reticulocytes 3·0 per cent. Fragility normal, also coagulation time. Van den Bergh reaction 0·2 mg. bilirubin per cent. (normal). Platelets 9,900. Bleeding time 15½ minutes. Tourniquet test: the armlet of the sphygmomanometer was applied to the left upper arm with a pressure of about 70 mm. for twenty minutes, the forearm being kept warm in a water-bath; a number of petechial hæmorrhages appeared, about 0·2 cm. in diameter, only two spots being as large as 0·5 cm.

Treatment: Liver extract equivalent of 500 grm. daily for eighteen days without effect. Fresh liver juice now being tried. Dilute hydrochloric acid 30 minims t.d.s. Slight epistaxis on two occasions stopped by intramuscular injection of 10 c.c. normal blood.

Discussion.—Dr. BERNARD MYERS said that treatment must depend on the actual diagnosis. Possibly in this case there might be two factors — (1) essential thrombo-cytopenic purpura hæmorrhagica; and (2) syphilis. It was not necessary in essential thrombo-cytopenic purpura hæmorrhagica to have an enlarged spleen, as in some of the worst cases which he had seen, there had been no enlargement of the spleen. If, after another blood examination, essential thrombo-cytopenic purpura hæmorrhagica was further suggested he advised doing a splenectomy, and, in order to give the patient a chance, the operation should not be too long delayed.

Dr. J. W. CARR (President) asked whether the complete achlorhydria, from which the patient suffered, was of any value in pointing to a diagnosis of pernicious anæmia.

Dr. EVAN BEDFORD said he remembered a severe case of thrombo-cytopenic purpura hæmorrhagica in which the spleen was not palpable. From his reading, his impression was that at one end of the scale was aplastic anæmia, and at the other the intermittent thrombo-cytopenic condition. He agreed with Dr. Myers that splenectomy should be considered.

Dr. F. PARKES WEBER said this was not a typical case of aplastic anæmia, which was usually either acute or subacute, whereas this case was chronic. He asked when the anti-syphilitic treatment was carried out.

The question arose of discontinuing the arsenical anti-syphilitic treatment for the present, since it was possible that the bone-marrow, owing to the treatment to which it had been subjected, failed to respond in the ordinary way. In fact, the case might be one of thrombo-penic purpura, analogous to the cases in children, but complicated by a failure of the bone-marrow to respond, owing to a toxic effect of the arsenical preparation.

Dr. DONALD BRIMS said that he had seen a case in Paris which was diagnosed as aplastic anæmia, but the diagnosis proved to be wrong. It was found in that case that the marrow cavity (although not in the radius and ulna) was filled with bone. Syphilis might solidify a bone. How far, he asked, had the alteration in the radius and ulna in this case affected the blood-count?

Dr. WITTS (in reply) said he did not think the fact that there was no free hydrochloric acid in the stomach helped in the diagnosis, because that might be so in many severe anæmias. The normal Van den Bergh reaction and the absence of response to liver treatment were against a diagnosis of pernicious anæmia. The purpura was dependent on the thrombopenia, but he did not think this was a case of essential thrombopenia. The spleen was not enlarged and the patient had leukopenia and an anæmia of high colour index, which was out of all proportion to the blood loss, and had not responded to treatment. The purpura first appeared after anti-syphilitic treatment. His view was that the arsenical treatment had damaged the bone-marrow, and that splenectomy could not be expected to do much good.

Syphilitic Disease of the Radius.—HAROLD C. EDWARDS, M.S. (introduced by CECIL P. G. WAKELEY, F.R.C.S.).—Patient, male, aged 51, fractured the left humerus in November, 1927. During treatment for this condition he noticed that the corresponding radius was deformed at the lower end.

Syphilitic disease of the radius.

The radius is markedly thickened, with a nodular surface. A skiagram shows a diffuse osteo-periostitis of the lower two-thirds of the bone.

The patient admits having had syphilis in 1916. The Wassermann report is "doubtful."

He had a course of treatment which might have been regarded then as sufficient, but would not be so regarded now. The Wassermann reaction is now negative, and

he has no other signs of syphilis. I think the radius is the only bone which is enlarged.

There is the suggestion of a double contour of the bone, which is regarded as typical of syphilitic osteo-periostitis. It is not absolutely certain that it is specific disease. I shall be glad to hear of alternative diagnoses. Is it worth while to give a course of arsenic?

Discussion.—Mr. CECIL P. G. WAKELEY said he regarded this as a case of syphilitic disease of the radius. In Paget's disease the only bone affected singly was the tibia, or possibly the clavicle alone might be involved in the disease. Moreover, if it were Paget's disease, the whole bone would be affected. This case had a demarcated area, with dense sclerosis on one side and normal bone on the other.

With regard to treatment, when once syphilitic sclerosis of bone had set in, no treatment was of avail, though the periostitis might clear up.

A Case of Fatty Atrophy following Insulin Injections. — HAROLD AWROUNIN, M.B.—Mrs. M., aged 56, has suffered from diabetes mellitus for fifteen years. Her urine has been intermittently sugar free, but she will not keep to a strict dietary. No abnormal symptoms; appears to be quite healthy.

Insulin therapy was begun in March, 1927; twenty units twice daily being injected into the arms, more or less in the same area each time. In March, 1928, she noticed a dimpling of the arms in the injection area, which began to extend deeply. Synthalin treatment was attempted unsuccessfully, and therefore insulin therapy was continued by injections into the thighs.

On examination: well-covered arms; a deep depressed area on each side at point of injection below insertion of deltoid, where subcutaneous fat seems to have entirely disappeared; skin and underlying muscles appear perfectly normal and are not adherent; no sensory nervous disturbance, function of arm not impaired and condition gives rise to no symptoms. Thighs show an early stage of same lesion.

Discussion.—Dr. W. J. CARR (President) said a notable feature in this case was the rapidity with which the change had come on. Dr. R. D. Lawrence, in a paper published in the *Lancet* last June, had discussed the whole question, and said the only solution was to make the injections in all parts of the body in areas as widely separated as possible.

Dr. HUGH STANNUS pointed out that this patient was a very fat woman, and asked whether such a result ensued only in these persons. Was it possible that the fat was of a pathological nature in this case before the insulin was given?

Dr. F. PARKES WEBER said it would be interesting to learn whether patients who had shown this local subcutaneous fat atrophy after insulin injections were always fat, or were sometimes relatively thin subjects. He suspected that when there was a thick layer of subcutaneous fat the nutrition of that fat was less stable in the ordinary thin person, as if persons were not intended by Nature to be very fat. In the fat person the blood-vessels of the panniculus adiposus probably became abnormally widely separated and any slight injury to their walls would seriously interfere with the nutrition of the large fat-cells, only the thin protoplasmic shells of which were really living. When the fat became inflamed, or was damaged in any way, the large fat globules contained in the fat-cells were treated as foreign bodies, and were gradually absorbed by the phagocytic histiocytes, which under the microscope could be seen loaded with fat granules (portions of the large fat globules) in specimens from panniculitis of various kinds.

Mr. C. P. G. WAKELEY said that a curious feature about the case was the lack of fibrous tissue. He had seen traumatic fat necrosis in the breast following the subcutaneous injection of glucose; it was very hard and fibrotic. A year later the condition had been diagnosed as scirrhous carcinoma. In the present case there was practically no fibrous tissue formation.

Dr. HAROLD AWROUNIN (in reply) said that in March, 1928, Carmichael and Graham had contributed a paper to the *Lancet* on the subject, and in the following issue there had appeared a letter from Dr. Chapman, who said that he had had a similar case, and that two and a half years after stopping the injections the condition had cleared up. Lawrence described infiltration of the tissues with the hystiocytes, as Dr. Weber mentioned, but in his case the local fat atrophy had not developed.

Section of Laryngology.

President—H. Bell Tawse, F.R.C.S.

[*November* 2, 1928.]

CASES AND SPECIMENS.

Laryngofissure for Epithelioma. Case shown Six Years after Operation.—Sir StClair Thomson, M.D.—Patient, male, aged 86, previously shown on May 4, 1923, six months after operation.

First seen October, 1922, with the right vocal cord replaced, except for a small portion posteriorly, by an irregular, pink, fleshy growth. Cord moved freely, but clinical appearances were typical of malignant disease; had been diagnosed by the late Dr. Permewan, of Liverpool. Patient had smoked all his life and still smokes three to four ounces a week.

Usual laryngofissure on October 21, 1922; thyroid removed; tracheotomy tube left in only seven hours. Same evening patient swallowed easily and was out of bed next day, playing chess. He returned to Wales eighteen days after the operation.

The chief interest lies in the age of the patient and the absence of shock. It was only after careful consultation that it was decided to operate, Dr. Frederick Price having reported fibroid degeneration of the myocardium, arterial disease, probably a rather dilated aorta and attacks of angina during the last five years.

It is now six years since the operation, and the patient, though 86 years of age, has followed the hounds every winter until last season, when he told me that he would still gladly do so, but found it difficult to mount.

Discussion.—Mr. Lionel Colledge said that besides the accurate diagnosis, faultless technique and ceaseless vigilance later, there was also another factor regulating success or failure, and that was the histological character of the growth. Sir StClair had given to him the slides from all cases submitted to thyrotomy, and of these sixty were suitable for this investigation, being from cases in which patients had survived the operation and had lived long enough to show whether they could be considered cured, or whether a recurrence had taken place. Examination of those slides had been carried out absolutely independently of the clinical history on the lines indicated by Dr. Broders, of the Mayo Clinic, who had begun his investigations in epithelioma of the lip. Dr. Broders divided epitheliomata into four classes, depending on the degree of differentiation of the cells of the tumour. Grade 1 was the least malignant, showing about 25 per cent. of undifferentiated cells. Grade 2, with about half the cells undifferentiated, was next in severity. Grade 3 showed from 50 per cent. to 75 per cent. of those cells, while Grade 4, the most malignant, showed from 75 per cent. to practically all cells undifferentiated. Special attention was paid to the amount of keratinization and the number of mitotic figures.

He (Mr. Colledge) found it difficult to draw a sharp line between Grades 2 and 3, and therefore, as Broders suggested, for some regions he had classified the tumours into three groups, with the results shown in the table. When he came to examine the history of the only recurrence in Grade 1 he thought the case was one which Sir StClair would not now have submitted to laryngofissure, as it was so advanced. In the intermediate group of this series there were 26 per cent. of recurrences, and in the third group 72 per cent.

Grade	No. of Cases	Recurrences	Percentage of Recurrences
1 ...	15 ...	1 ...	6·6 per cent.
2 ...	18		
3 ...	16	... 7 ...	26 per cent.
4 ...	11 ...	8 ...	72 per cent.

recurrences. The conclusion was inevitable, that if a piece of growth removed showed that the tumour belonged to Grade 4, laryngofissure would be scarcely justifiable, and some other form of treatment was indicated. It explained why, in some cases, when Sir StClair had been doubtful about the prognosis, as, for example, when the growth was extensively subglottic, the patient, nevertheless, survived. A case in point was that of the admiral, now well known to members of the Section, who was operated upon eleven and a half years ago for a fairly extensive subglottic growth. He belonged to Grade 1, and this explained why he had survived so long without recurrence.

Professor M. HAJEK (Vienna) said that one must operate in such cases as early as possible, and, remembering that the cancer was probably more diffuse than was suspected, a wide sweep should be made. In the present case he saw no vocal cord, but there was a thickening in that position, a ragged appearance which might be taken for granulations or for tuberculous infiltration, but Sir StClair had proved it to have been cancer. He (the speaker) might not feel sure that it was perfectly healed. Some attributed a good deal of significance to these subsequent thickenings of the cord. An enlargement might be merely hypertrophic, or might also indicate some deep recurrence of the disease. If laryngofissure with excision of the cord was performed, the case should be seen every month or two in view of possible recurrence.

Dr. JOBSON HORNE (by way of illustrating the frequency of errors made by the most experienced observers in the diagnosis of epithelioma of the vocal cords, and also the importance of the histological aspect of the subject to which Mr. Colledge was giving attention) cited the following extract from the article on malignant growths of the larynx by Sir Felix Semon in Sir Clifford Allbutt's "System of Medicine"[1]:—

"Out of thirty-three patients on whom thyrotomy was performed by one of us (F. S.) for presumably intrinsic malignant growths, in eight the diagnosis was either found to have been at fault or remained doubtful."

That is, approximately 25 per cent. of errors in cases in which thyrotomy had been performed. In the present day, perhaps, some of that 25 per cent. would be in Grade 1 of malignancy.

Mr. ERIC WATSON-WILLIAMS said that it was now generally advised that no biopsy should be made before operation. But this work of Mr. Colledge's showed that important information might be obtained from histological examination: one might learn to distinguish growths suitable for operation, growths for which operation involved considerable risk of recurrence, and growths which would be better treated by radiation or otherwise owing to the great chance of recurrence after operation. If so, we should have to revert to preliminary removal of a portion before deciding on our line of treatment.

Dr. J. S. FRASER asked what was the average age of the patients in the several groups classified according to the degrees of malignancy. In twenty cases of intrinsic cancer of the larynx he had found that cancer which occurred in younger subjects—aged from 35 to 50—was apparently of a more dangerous nature than that in older patients.

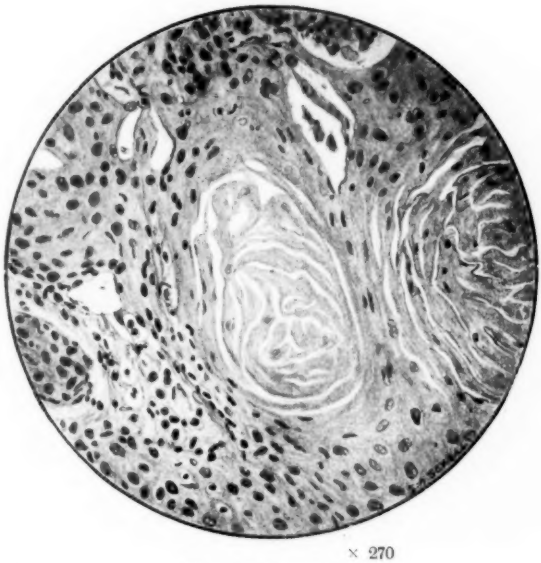
Sir STCLAIR THOMSON (in reply) said that clinically the age of the patient had little to do with the prognosis. Some of the rapid recurrences had been among elderly people, while some of the younger patients had done very well. Sometimes a very limited case would surprise him. Hence he was looking forward to the result of this scheme for histological investigation; it would, as Mr. Watson-Williams said, make for histological examination by an expert beforehand. What Professor Hajek had said repeated what he (Sir StClair) himself had recommended, namely, that after the operation the patient should be inspected once a month for at least a year by the surgeon who had operated. Two colleagues had asked him if his case did not show a recurrence. It did not; what could now be seen was only compensatory hypertrophy and scar tissue, which he (the speaker) had been watching for six years. By watching cases the eye was in time trained to distinguish between actual recurrence and hypertrophy or scar tissue formation, which had sometimes caused needless alarm.

Mr. LIONEL COLLEDGE (in reply) said that he had been able to follow Dr. Broders' directions precisely because that gentleman had kindly sent him a box of graded slides, and he wished to take the opportunity of acknowledging this courtesy. In reply to Mr. Watson-Williams: he believed that in the Mayo Clinic epithelioma of the tonsil, if found to be in Grade 4, was regarded as inoperable, quite apart from any clinical consideration, and treatment was by radium or X-rays. There were parts of the body in which epithelioma of Grade 1 did not occur, and that explained why cancer appeared to be more malignant in some parts than in others. The value of biopsy might thus be extended beyond a matter of plain diagnosis. He had not yet calculated the average age of the patients in the various grades. The investigation showed that cancer of the larynx was not always of such a benign form as had been thought. In the whole series the recurrences numbered 25 per cent., but in Grade 4 the recurrences reached 72 per cent.

[1] "System of Medicine," edited by Sir Clifford Allbutt, K.C.B., 1908, Vol. IV, Part II, 251.

× 90

FIG. 1.—GROUP I.



× 270

FIG. 2.—GROUP I.

UNIV. OF MICH.

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FIG. 3.—GROUP II.

× 90

FIG. 4.—GROUP III

UNIV. OF MICH.

× 90

FIG. 5.—GROUP IV.

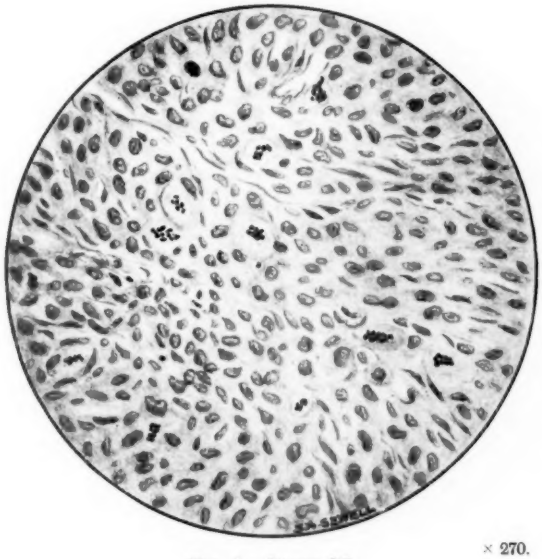

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FIG. 6.—GROUP IV.

UNIV. OF MICH.

Complete Occlusion of Posterior Choanæ.—C. A. S. RIDOUT, M.S.—Male, aged 16, sent to hospital with suggested diagnosis of adenoids as cause of nasal obstruction. Has never breathed through nose. Adenoids removed when he was aged 2, tonsils and recurrent adenoids removed when he was aged 9. Otherwise has always had good health. Family history negative.

On examination: nasal passages and mucous membrane of turbinals and septum have a rugose and leathery appearance, and lately there has been purulent accumulation.

Posterior rhinoscopy shows complete occlusion of both posterior choanæ by a whitish membrane. Radiograph shows frontal sinuses and antra of normal size.

I propose to remove the end of the vomer, and any other bone in the vicinity of the membrane, to try to secure a free air-way.

Discussion.—Mr. FRANK ROSE said this condition was uncommon, but he had seen several examples. What had impressed him about cases of congenital occlusion of the choana was that none of the patients he had examined had ever had adenoids. Was Mr. Ridout quite sure that there were adenoids when he had first operated on the child?

Dr. JOBSON HORNE said the case illustrated how well one could get on in life with even both nasal choanæ completely occluded. This patient, moreover, had no impairment of hearing, in fact he had had to pass severe tests of hearing to obtain his present position. The case further suggested that occlusion of the choanæ by membrane might be a safeguard against worse nasal troubles. Treatment in the first place should not be too drastic. Before the removal of a part of the vomer he would prefer perforation of the membranes and plugging of the posterior nares, his purpose being to prevent the last state from being worse than the present.

Mr. HAROLD BARWELL said that in some of these cases the partition was hard, thick and bony, and so not easily removed. To avoid re-contraction at the original site, he would remove the posterior part of the vomer, so throwing the choanæ into one large cavity, as re-contraction of such a cavity was less likely to occur than in the case of a smaller one. None of these complete occlusions was accompanied by deafness; it was the partial obstructions which seemed to lead to catarrhal disease spreading up the Eustachian tubes.

Mr. J. F. O'MALLEY said that a few years ago he had shown an example of this condition, with empyema of the right antrum. The fact that there was no through ventilation of the nose might give this boy trouble, and therefore an attempt to give him a clear passage was well worth while. With regard to the effect of the choanal obstruction on the voice resonance, he thought any operation would be useless.

Mr. A. J. WRIGHT said that it was essential to take away a portion of the vomer. The mistake of trying to remove existing or non-existing adenoids could be avoided by the simple precaution of testing in all cases to see whether there was any nasal respiration. Adenoids never caused total obstruction or total mouth-breathing. It was curious that these mouth-breathing cases did not show the typical changes in the facial skeleton naturally associated with adenoids, thus suggesting that total obstruction did not cause marked changes in either the facial skeleton or the chest; such changes were caused by breathing through an obstructed air-way.

The question of the recovery of the sense of smell was interesting. Some years ago he had shown two sisters with this condition, and after operation one had recovered the sense of smell, but the other had not.

Sir JAMES DUNDAS-GRANT said that a remarkable point was the good development of this patient. A girl with this condition whom he had shown some years ago, was a poor miserable specimen, and after the operation she brightened up and developed, and her mentality showed such improvement that she was able to become a teacher. She had previously been operated on for "adenoids" but naturally without benefit. Removal of the posterior part of the septum was first suggested by Sir Charters Symonds; it was an essential part of the procedure. The operation could be conveniently carried out, a finger being inserted into the naso-pharynx for a guide, while the gouge was used through the anterior nares. In cases with complete occlusion he had found the nasal cavity in front filled up with a starchy-looking material.

Mr. L. GRAHAM BROWN said that if there were congenital occlusion by a membrane, one would expect the membrane to be smooth, but in this case a dimple or depression could be seen and this suggested that the occlusion was cicatricial.

Mr. RIDOUT (in reply) said he could not remember whether there had been adenoids. When the patient had come to him last September there was thick, starchy, purulent material in the nose.

Chronic Dacryocystitis treated by Mosher's External Operation.—HERBERT TILLEY, F.R.C.S.—Male, aged 70. Six years ago left eye began to "water," and three years ago "began to discharge a yellow matter." An ophthalmic surgeon prescribed injection of a solution of argyrol; which alleviated but did not cure the symptoms. About two years ago the suppuration increased, and an operation was performed. A second operation was performed two months before I saw the patient.

On Examination (June 12, 1928).—Regions round the lacrymal sac swollen and tender. The free discharge of pus necessitated a frequent change of dressings and consequently, an external bandage. External fistula surrounded by granulations leading into the suppurating sac; some conjunctivitis. Nasal sinuses normal, but anterior half of left middle turbinal had been removed.

Operation (July 2, 1928).—General anæsthetic. Incision from immediately below inner end of eyebrow, downwards in front of internal palpebral ligament, curved slightly outwards and downwards in front of lower inner angle of floor of orbit. Sac exposed from above and everted from natural position in front of the lacrymal crest. Lacrymal bone in front of crest and neighbouring portion of ascending process of maxillary removed so that a wide opening into the nasal cavity was made. Inner half of lacrymal sac was then removed, and margins of outer half placed in the borders of the naso-lacrymal opening. Fine external sutures closed the skin wound.

Rapid and complete recovery.

The advantages over the West operation were obvious, as every step of this operation was visible, and it was easy to see the inner wall of the sac and remove as much as appeared to be necessary.

Discussion.—Mr. H. BELL TAWSE (President) said that he had operated by West's method in several instances, but the result had been so unsatisfactory that he had now discontinued it.

Mr. HAROLD BARWELL said he also had found a difficulty about West's operation; the difficulty lay in making a sufficiently large opening to remain permanently. His cases had usually shown improvement for about twelve months and had then relapsed. He had been performing the Mosher operation, in conjunction with Mr. R. R. James, with good results. He had varied the procedure for one of his early cases, as the duct had seemed to be in a suppurative condition, and in that case he had cut the inner wall of the duct away, making a long grooved opening into the nose, so as to establish drainage of both sac and duct.

Mr. GEORGE WILKINSON said he had always considered West's a useful operation, giving fairly good results. In the present case the canaliculus had been slit up. Was this done before or at the time of the operation; and did Mr. Tilley think it a necessary procedure? In West's operation the cases did not seem to do well if the canaliculus had been slit beforehand.

Mr. TILLEY (in reply) said that the three or four operations of slitting the canaliculus had been performed before he (the speaker) had seen the patient.

Frontal Pain, with Absence of the Frontal Sinus. Operation. Cure.—HAROLD KISCH, F.R.C.S.—Patient, bus-conductor, aged 26, has suffered for seven years from severe and gradually increasing pain in the region of the right frontal sinus. There was no obvious disease in the nose, but a skiagram showed absence of the right frontal sinus. A general medical, neurological and ophthalmic examination revealed no cause for the pain. At the patient's urgent request, an exploratory examination of the right frontal region was undertaken. The frontal sinus was represented by a narrow chink, but the ethmoidal cells were well

developed, and the mucous membrane of these cells was thickened and reddish in colour. No pus or fluid. The bone in the frontal region was normal in texture and showed no increased vascularity.

Discussion.—Mr. J. F. O'MALLEY said that the problem of the symptoms in relation to the finding seemed a difficult one. He had had patients complaining of frontal pain, but X-ray examination had not revealed any sinus trouble. The cause of the pain was pressure of the middle turbinate in the upper part of the nose. If his judgment was correct in this case, removal of the middle turbinate might give relief. In this case the removal of bone necessary to demonstrate the position of the frontal sinus must have given relief from long pressure caused by the middle turbinate.

Mr. KISCH (in reply) said that he had tried to exclude the middle turbinate before operating, by shrinking it up with cocaine, but the pain had not been relieved.

Lymphosarcoma of Tonsils.—W. M. MOLLISON, M.Ch.—M. G., female, aged 48, came to Guy's Hospital, September 25, 1928, complaining of sore throat during past three weeks.

Teeth removed twelve months previously. Since then constant sore throat.

The tonsils were ulcerated and covered with exudate which extended on to the anterior pillar and uvula. There was an enlarged, tender mass of glands in the left side of the neck. Smears from the tonsils showed large numbers of Vincent's organisms. Wassermann reaction (two tests) negative. No tubercle bacillus in sputum. Mercury and potassium iodide were administered and the tonsils were painted daily with 15 per cent. silver nitrate solution.

At the present date (about ten weeks since the onset) the condition of the throat is unaltered.

The condition was found, on histological examination, to be lymphosarcoma, and the patient was treated by deep X-ray therapy. Within a few days the growth was only half its former size. The swelling in the glands of the neck has also disappeared.

Discussion.—Mr. T. B. LAYTON said it was difficult to criticize such a case as this when it was only seen after apparent recovery, but he suspected that it was of the type to which Professor Hajek had referred in his Semon lecture. The result was so striking that he wondered whether the pathologist was correct in calling the condition lymphosarcoma. The difference as seen under the microscope between lymphosarcoma and some of the less usual forms of inflammation was so slight that there was a possibility of error even on the part of the most skilled microscopical anatomist. This case might have been one of an unusual form of inflammation and not a neoplasm. If it had really been a neoplasm, the result of his deep X-ray therapy was most striking.

Dr. JOBSON HORNE said he agreed that it was difficult to express an opinion upon the nature of the disease of the tonsils after exposure to deep X-rays. The clinical appearances did not suggest sarcoma. In the past " sarcoma " of the tonsil had been a pitfall for the morbid histologist.

Mr. MOLLISON (in reply) said that Dr. Nicholson was the pathologist, and he did not doubt that the diagnosis was correct. No result had been produced by antisyphilitic treatment and the patient was becoming worse. No new growth responded more rapidly to deep X-rays than did a sarcoma.

Chronic Subglottic Laryngitis in a Child.—W. M. MOLLISON, M.Ch.—A girl, aged 15, attended hospital in November, 1927, on account of earache. Tonsils and adenoids were considered to be the cause and were removed. Subsequently hoarseness developed and slight subglottic thickening was observed.

Later the arytænoids becames swollen and are now so greatly swollen as to hide all but the anterior halves of the cords. Wassermann reaction was $\pm$, and a course of novarsenobillon was given without affecting the local appearance. Sputum not obtainable.

Discussion.—Sir JAMES DUNDAS-GRANT said thyroid treatment might be tried; in some of these cases the condition proved to be localized myxœdema.

Dr. BROWN KELLY suggested that this might be an instance of diffuse hyperplasia due to congenital syphilis. He had written a paper on the subject in 1906.[1] The outstanding features were diffuse hyperplasia affecting the larynx and sometimes also the pharynx. This might be symmetrical and unaccompanied by ulceration. The underlying disease was congenital syphilis, although no other sign of that diathesis might be evident. The hyperplasia did not respond to antisyphilitic treatment. There was a great tendency for the tissue to become rapidly œdematous, and in a large proportion of the cases œdema was responsible for the death of the patient.

Cystic Tumour of Pharynx.—J. A. GIBB, M.D.—Patient, male, aged 74. For ten years has complained of slight discomfort on right side of throat but has no difficulty in swallowing or breathing, and no change in voice.

There is a large swelling apparently originating in the lateral wall of the pharynx and causing the right half of the epiglottis to be folded inwards and backwards towards the mid-line. On palpation the tumour is cystic.

Tumour of Base of Tongue.—W. H. JEWELL, M.D.—M. A. S., female, aged 40. In 1914 first seen at hospital complaining of difficulty in breathing and swallowing, due to a tumour of the base of the tongue in much the same position as the present one. As the symptoms had not been urgent and there was a doubt as to there being a sufficiency of thyroid in the neck, operation was not advised at that date.

In 1920 at another hospital the tongue had been split and the removal of the tumour attempted. Respiration and swallowing were better for twelve months afterwards, notwithstanding the great difficulty she had in moving her tongue during that period.

May, 1928, patient returned to me at hospital complaining of difficulty in breathing and swallowing, becoming much worse during the past two years.

There is a globular tumour of about the consistency of thyroid on the dorsum of the tongue in the vicinity of the foramen cæcum. It is about the size of a small walnut, but varies slightly. No evidence of thyroglossal tract in neck.

Discussion.—Mr. HERBERT TILLEY advised puncture of the swelling with a fine trocar as a possible help in diagnosis. If no fluid came out, it would remind him of a case which Sir Felix Semon had shown many years ago, in which a swelling of similar appearance proved to be a fatty tumour. The swelling in this case would, he thought, prove to be a cyst. Professor Hajek had said he thought it might be a thyroid tumour, and had advised a subhyoid operation which he had employed in three or four such cases. He had also enjoined that special care should be taken during the removal of the tumour not to perforate the pharyngeal mucous membrane, and thus endanger septic infection of the wound.

Mr. NORMAN PATTERSON said that some years ago he had shown a somewhat similar case. It had been diagnosed as a cystic tumour, but proved to be an angioma. He therefore agreed that the swelling in this case should be punctured.

Sir JAMES DUNDAS-GRANT said he had treated such a case by transfixing near the base of the projecting part with two needles, beneath which he placed an écraseur, and then slowly detached the tumour. There had been no great hæmorrhage, and sufficient thyroid tissue had been left for functional needs.

Mr. H. BELL TAWSE (President) said he thought Mr. Jewell's case should be left alone. The condition did not seem to be causing much trouble.

Dr. E. B. BARNES said he had had a similar case in a child who had already been operated upon, and had had a recurrence. He had obtained a view of the tumour from the mouth and had removed it with a diathermy needle. When he saw the patient again, eight months later, there was no recurrence.

Mr. A. R. TWEEDIE said he had shown a similar case two years before and had been advised to leave it alone. Nothing adverse had happened since.

1 *Glasgow Medical Journal*, November, 1906.

Mr. JEWELL (in reply) said that the patient now complained of difficulty in respiration which made it necessary for her to be propped up with a bolster and three pillows at night; she also complained of the return of her food if she attempted to take solids. She requested that something should be done to relieve her. Following her operation in 1920, it was said to have been necessary to give thyroid extract for a long time. He did not think it would be advisable to carry out subhyoid pharyngotomy for fear that myxœdema might result. He proposed to use diathermy or the cautery. The swelling had been punctured but no fluid had been obtained.

Tumour of Carotid Body.—H. LAWSON WHALE, F.R.C.S.—Male, aged 54, admitted to hospital, May, 1928, with a swelling of the neck.

Wassermann reaction negative. Repeated examinations, including two endoscopies, failed to reveal any primary focus. Provisional diagnosis: primary neoplasm of lymphatic glands. At operation, the mass was found to be so extensively adherent as to be inoperable. A portion was removed for microscopic examination.

Pathological Report (May 24, 1928), by Dr. RICKWOOD LANE: The tumour consists of areas of hyaline tissue in which are numerous spaces containing blood and capillaries with prominent endothelium, surrounding masses of cells originally polygonal in shape, but becoming spindle-shaped from pressure and evolution. In these are capillaries and more spaces containing blood particularly in the central parts. The tumour is an endothelioma, possibly a carotid body tumour.

Ten days later, two cartridges of radium were inserted for twenty hours.

Mr. T. B. LAYTON said that twenty years ago he had seen an identical case. He had taken the section to the late Mr. J. H. Targett, who was then an acknowledged authority upon microscopic morbid anatomy, and he vividly remembered what he had said: "This tumour is certainly epithelial in nature and if no primary tumour can be found, I suppose we must class it as branchiogenous." He (the speaker) thought this report might also apply to the section of the case under discussion and he saw no reason to class it as an endothelioma.

Were they, however, sure there was no primary growth? He suspected that the tumour in the neck would prove to be secondary to a carcinoma of the sinus pyriformis. Already on that side there was mucus hanging about at the opening of the sinus which was not present on the other side, and this might be the first symptom of such a tumour. In the case to which he had referred, the patient had died eighteen months after the operation, from dysphagia almost certainly due to a carcinoma of the pharynx, though no post-mortem evidence had been obtainable.

Sections of Tonsils, one of which formed a Large Tumour filling the Pharynx.—LESLIE POWELL, M.B.—Patient, male, aged 59. During the past year has noticed swelling of throat, without symptoms.

First seen August 17, 1928, with difficulty in swallowing, thick speech, constant clearing of throat and slight wasting. *Left tonsil* enormous, covered with epithelium except tip, which was ulcerated, apparently from friction with other side of throat; base not fixed and general substance firm but not hard; glands not enlarged. *Right tonsil* moderately enlarged and of similar consistency. Wassermann reaction, negative.

September 26, 1928.—Operation: tonsillectomy. The left tonsil being so large was removed piecemeal before enucleating. No excessive bleeding. Tonsillar fossæ normal. Convalescence uneventful.

Macroscopic Appearance.—Both tonsils consisted mainly of a homogeneous friable mass of the colour and consistency of honeycomb. Pieces could be punched out without any tearing. No pus or caseation. Suggestion of localized Hodgkin's disease.

Pathological Report (by Dr. S. Keith).—(i) *Left tonsil* (that is the larger one) contains many crypts full of unorganized débris. There is chronic inflammation, but not more than is common in any tonsil.

(ii) *Right Tonsil.*—The crypts in this tonsil contain purulent material. The tonsil as a whole shows signs of moderate infection.

FURTHER REPORT ON CASE PREVIOUSLY SHOWN.

Membrane on Palate, Pharynx and Larynx.—W. M. MOLLISON, M.Ch.—(Shown at the meeting held February, 1928.) The patient, a man, aged 63, has completely recovered.

This case is, however, very obscure, and the suggestion has been made that the condition was a malignant growth covered by a yeast on the surface. One application of deep X-rays was given, but so far as could be seen without effect. There is now no trace of the membrane left anywhere.

Discussion.—Dr. E. A. PETERS asked whether any serological test had been made, and whether there was any acquired immunity.

Mr. M. VLASTO said he would like to know whether Mr. Mollison had asked a dermatologist for a diagnosis.

Mr. MOLLISON (in reply) said that he had not shown the patient to a dermatologist, because he had not appeared at the out-patient department for a long time. The Wassermann reaction had been negative on two occasions. The only bacteriological finding was a yeast grown from the surface of the membrane on two occasions. He (Mr. Mollison) regarded it as an unusual case of thrush.

Section of Medicine.

President—Dr. A. F. Hurst.

[*October* 23, 1928.]

DISCUSSION ON HYPOCHONDRIA.

Dr. R. D. Gillespie.[1]—There are persons whose behaviour exhibits certain similar features in a sufficiently uniform combination to warrant classifying these individuals together as people who react predominantly in a way that for convenience may be designated "hypochondriacal."

The usefulness of so classifying them is twofold. First, the knowledge that a patient displays the marks of a hypochrondriacal form of behaviour enables us to make certain statements about the course and treatment of his disorder. Secondly, by placing different patients in juxtaposition, and by collating the facts obtained from all of them, we are able to formulate more easily a notion of the basis of hypochondriacal behaviour in general. This is important, since in any single case, clues as to the origins are so scanty that it would often be mere guesswork to discover the underlying determinants of the symptoms, if the case were considered in isolation.

The history of the term "hypochondria" as given by the Oxford English Dictionary, is itself worth a preliminary consideration. *τὰ ὑποχόνδρια* meant the soft parts of the body below the costal cartilages (*χόνδρος* = gristle), and the Latinized singular form "hypochondrium," connoted the viscera situated in the hypochondria, i.e., the liver, gall-bladder and spleen. Culpepper, for example, in 1652, wrote of "the liver, gall-bladder and spleen, and the diseases that arise from them, as the jaundice and the hypochondriac." By a misapprehension that hypochondria was a feminine substantive singular, and by a transfer of meaning the term came to be used for the mental distemper itself.

Hypochondria can be defined as a persistent pre-occupation with the bodily health, out of proportion to any existing physical justification, and with a conviction of disease. No other effect than the belief involved in conviction need be apparent; but there may be some accompanying depression, not out of proportion to what would reasonably be expected to accompany the knowledge of the existence of bodily (or mental) disease, supposing such existed. The conviction (like all convictions) defies direct attack.

This concept gives the basis for the nosological isolation of hypochondria as a clinical entity in its own right. For a clinical definition there are added observational data regarding the course, which is chronic but not (over a long period) deteriorative intellectually or emotionally; the age and sex of the person involved; and the response to therapy. They are nearly always men. This preponderance of the male sex is in accord with the traditional description of hypochondria. It is perhaps explicable on a psycho-pathological basis. Inaccessibility to therapy is the clinical aspect of the firmness of the conviction. This also is illuminated by psychopathological analysis. Exact nosological definition enables a distinction to be made from what can now be called pseudo-hypochondrias, hysterias, and anxiety-states. It also enables us to speak of a hypochondriacal development of an abnormal personality; it is still hypochondria, and it is not correct to consider the hypochondriacal ideas in such instances as part of a larger syndrome. It is more accurate to speak of hypochondria in a schizoid personality: the additions to the pure hypochondriacal picture depending not on a new concurrent development but on pre-existing oddities of personality.

Psychopathologically, from the content of the hypochondriacal complaints (their

[1] Summary of a preliminary report on work done during the tenure of the Pinsent-Darwin Studentship of Cambridge University. A fuller account will appear in an early number of the *Guy's Hospital Reports*

localization, etc.), and from the patient's other utterances, an anal-erotic basis for hypochondria is strongly suggested in some cases. The frequent association of hypochondriacal and paranoid ideas has evidently, in some cases at least, a common basis in feelings of guilt. It is suggested that the strong, usually unconscious, feeling of guilt is in some cases the component which clinically makes the conviction of disease so profound, and produces in these cases the therapeutic inaccessibility.

The consideration of the indications in certain hypochondriacs, of the possible unconscious trends involved, helps therefore to explain not only the fixity of the hypochondriacal beliefs, but their nature (damage to the bodily health) and, in some instances, the actual localization of the physical complaints.

An egocentric personality, the alteration in psychological outlook with advancing years, the presence of some actual physical disease, external difficulties and disappointments operate in various degrees with different patients to produce a hypochondriacal reaction.

Dr. R. S. Allison.—The view that hypochondria is a definite entity and not merely a variation of some other disease has a particular appeal for us as we are frequently confronted with patients showing no other sign of mental disorder than this abnormal concentration on the bodily sensations.

The difficulty of clearly defining the malady has already been noted. The definition given in most text-books is inadequate. Hypochondria is more than undue attention to one's bodily state. In this it does not differ from anxiety and neurasthenic states. Dr. Gillespie is to be congratulated on his definition in describing it as " interest with conviction," the fact of disease being established in the patient's mind and not apprehended by him. A further point, which may lay claim to be distinctive, is the patient's conduct. This consists of a persistent search for cure, leading him to extremes to which even a sick man would not go, and this conduct is no doubt based on the firm conviction of the presence of disease. This conduct is typically shown in the case of a man aged 40, with symptoms of fifteen years' standing. He had taken innumerable medicines, had consulted all ranks and branches of the profession and had undergone five abdominal operations without relief. In a letter he wrote :—

> If a further operation will cure me I am willing and anxious to have it at once. At present I am only kept going by taking medicine. The past operations have done me no harm. I am only 40, strong and capable of really enjoying life, sound in every way bar this. I have done all I can to get well. I know that I am not suffering from a deadly or incurable disease, but I cannot go on in this way.

In the diagnosis of hypochondria, when the symptoms centre round the alimentary tract, we are called upon to decide whether there is anything in addition to hypochondria which might give rise or contribute to the symptoms. In determining this point we can rely, with some confidence, on the results of a physical investigation which includes a barium meal and enema, test meal, cholecystography and examination of the fæces.

In an investigation of this kind it is generally found that the hypochondriac, though willing enough, is a difficult subject to investigate. He is in a great hurry for results and is unwilling, for example, to allow the barium meal time to pass through naturally. He is eager to hasten its progress by means of an aperient. If the investigation is shortened on account of this, one is apt to miss things which may be important or about which it is as well to know when prescribing subsequent treatment. I refer particularly to the unexpected finding of achylia, gall-stones, infected gall-bladder and diverticulosis of the colon.

It is not usual in hypochondria to meet with a complaint of definite pain at a certain point. When asked to indicate the site, the hypochondriac makes an indefinite gesture and may frequently qualify his original description of a pain and

refer to it more as a sense of uneasiness or discomfort. Definite pain constantly in one situation, even in a hypochondriacal subject, will suggest the presence of organic disease. The following is an example:—

A man, aged 65, had since his retirement ten to fifteen years ago, among other symptoms, complained of a definite discomfort in the left lower abdomen coupled with a fear of cancer and undue introspectiveness. He was regarded, rightly, as a case of hypochondria. There was, however, some physical ground for his condition. Had his pain been on the right side of the abdomen it is probable that the appendix would have been suspected and removed. A barium meal and enema showed a state of diverticulosis involving the pelvic and sigmoid colon. With suitable treatment this discomfort was relieved, together with the morbid anxiety connected with it.

In dealing with hypochondriacs who have had previous abdominal operations, and when the presence of local disease may be excluded, we are sometimes called upon to decide what part adhesions may play in causing symptoms. Most patients who have had abdominal operations have recovered from all after-effects which might be attributed to adhesions, within a year or so of the time of operation. What then are we to suppose of symptoms persisting for from two to five years after operation? Further operations designed for the relief of such symptoms do not always succeed in their object, and frequently the symptoms are intensified rather than relieved. The key to the problem is often discovered on careful inquiry, when it will be found that the discomfort or pain is, in many respects, similar to that complained of before the original operation.

In considering the treatment of hypochondria we may review our aims. We are setting out on a forlorn hope (1) to remove a false impression in the first place, (2) to check harmful tendencies based on that false impression, and (3) to diminish the sensations from which the false impression arose. If organic disease co-exists, whether contributing to the symptoms or not, continued treatment will be more successful and the outlook generally better than in those cases where physical examination proved entirely negative.

The harmful tendencies which the hypochrondriac shows include the abuse of aperients and other medicines, the habit of frequent intestinal douching, and the craving for abdominal operations. The patient may therefore be instructed in the formation of a daily habit, weaned off the use of aperients, and instructed in the intelligent use of liquid paraffin. A vegetarian diet is useful to begin with, adding bulk to the intestinal contents and in this way producing regular evacuation of the bowel.

The third aim is to diminish the sensations of which the patient complains. It seems rational to give small doses of bromides spread over the day for prolonged periods. Much may be done to diminish symptoms by promoting regularity of habits and by giving up aperients. Regularity of habits may lead to regularity of sensations, and regularity of sensations may lead in turn to their being disregarded.

Dr. W. R. Reynell said he agreed that there was such a clinical entity as described by Dr. Gillespie, but he looked upon it as a somewhat rare condition.

According to the opener's definition hypochondria was "persistent." This would make the term inapplicable to cyclothymia and manic depressive cases in which the morbid state of mind was periodic.

According to the definition the preoccupation was "in excess of what is justified." It was difficult to say how much preoccupation with health was justified in, e.g., a case of visceroptosis; hypochondria, as defined by Dr. Gillespie, appeared to be a rare, incurable, non-progressive illness, and therefore chiefly of academic interest. He (the speaker) regarded it as a "morbid preoccupation with health in the absence of demonstrable organic disease." Such a definition would include a large number of cases, with which all were familiar, and which might be divided into three classes in order of frequency and curability:—(1)

Patients having a health "complex," but retaining other interests. (2) Patients whose sole interest was their health. (3) Patients with a delusion of illness. The last class was a small one, and for the most part incurable. Classes (1) and (2), however, contained many cases in which a rational psychotherapy was of the utmost value, and which, in the absence of treatment, might be progressive until the patient's personality was practically destroyed.

What type of person was most liable to become a hypochondriac? He (Dr. Reynell) believed that a predisposition often existed in childhood in a general lack of vitality. The "toxic debility," "atonic" type of child was much more liable to feelings of dysæsthesia than the normal child and was often extremely hyperæsthetic when tested, for instance, with a tuning fork. These patients could often feel the vibrations of the tuning fork two or three times as long as a normal subject. If the wider definition of hypochondria were accepted, there was no reason for therapeutic nihilism; on the other hand most of these patients could be shown the way to normal health by means of a rational psychotherapy. The most effective drug was the spoken word, but it must be spoken in the right way.

Dr. T. A. Ross said that a prognosis as to the kind of psychoneurotics likely to benefit from treatment, and the kind not likely so to benefit, would be of great value. The time required by each case was necessarily considerable, and attention could be conserved if it might be devoted to the right kind of case.

He agreed that there was a class of old men—sometimes old women—who were absolutely inaccessible to treatment, and on whom any attempts at radical procedure would be wasted. But that was not to say that nothing could be done for them; what was needed was to safeguard them from quacks.

He did not feel certain, however, that there was any great difference between the hypochondriac and the hysteric. In hospital, out of 1,500 patients, he had found nineteen people over 60 years of age, of the male sex, with psychoneurotic symptoms—he excluded those with psychotic symptoms such as melancholic depression—who had a clear insight, with no mental deterioration and who complained of something wrong with the body. Of these nineteen, only one, a patient under Dr. Gillespie, had recovered.

With regard to these people having continuous preoccupation, that was true as far as the doctor was concerned; the patient talked about his symptoms all the while he was in the doctor's room, but not necessarily all the time afterwards. He (the speaker) had lived with a man who was a first-class hypochondriac in the clinical sense, and for a quarter of an hour one had to listen to his symptoms, but his conversation afterwards ranged over many other subjects. Yet this patient's doctor had said he was the worst hypochondriac he had known.

He did not agree with Dr. Gillespie that the anxiety patient did not believe he suffered from a malady but only feared it. Some of these patients believed they had a damaged state of the brain; they were reassured by the doctor, and apparently accepted that reassurance, but had still a subtle kind of conception that something was wrong. One patient had said that his brain was affected as the result of an accident. Later he had said there was no disease in the brain but a film had grown over it. One must be sure that the conception of a macroscopic disease was not supplanted by a concept of a microscopical one.

Dr. Gillespie had quoted Dr. Hutchison's description of the "abdominal woman," but he (the speaker) was not sure that was not an example of "hypochondria cum materia." Could one be sure that these were not people mentally hypersensitive? Nobody cured them, because nobody as yet had satisfactorily cured visceroptosis. It was fashionable to say there would be no trouble with visceroptosis if doctors did not find it out, but he doubted that statement. Patients went to their doctor feeling ill and he found there was visceroptosis, therefore they must have been feeling something wrong before it was suggested.

Dr. Gillespie had also said that the hypochondriac had no affectionate dependence on the physician, as had the hysteric; but old gentlemen did not show their affection as young people did; old gentlemen were self-contained, dominant and obstinate. That was why the hysterical old woman was more easily cured than the old man.

The patient whom Dr. Gillespie had cured had been ill for twenty years, but had been cured in a month, and had remained well for two years. When he talked for ten minutes or more he had a headache. After hearing a sermon he had had a headache lasting three days. Dr. Gillespie would probably say this patient was hysterical, and had "an affectionate dependence on his physician." In a young hysteric that would probably be the case. This patient, if affectionately dependent, might be assumed not to have wanted to do anything without Dr. Gillespie's permission. But, instead, two days before he was due to leave he said he wanted to go to London. It was found that his purpose was to consult Sir John Thomson-Walker about his symptoms behind Dr. Gillespie's back!

He (the speaker) agreed that this condition might begin early; he had known patients as young as 33. A man, aged 36, had complained of feeling tired and as if his heart was damaged, and for ten years he had done nothing. No doubt he was anxious to escape from enlistment twelve years ago. Another patient, aged 70, whose hypochondria had begun when he was 33, was now under his (Dr. Ross's) observation. He had had abdominal symptoms (for which he had had an operation) during the past four years; these symptoms had begun apparently from a desire to escape from his wife. When he was suffering abdominally his wife's tongue was stopped, and ultimately he escaped from her for eighteen months. There was no evidence of anxiety, but there was conviction as to the presence of disease.

He (Dr. Ross) considered that hypochondria was a description, not of a disease, but of a type, i.e., of an old man with certain clinical symptoms.

Dr. C. P. Symonds said that Dr. Gillespie had established his thesis, in so far as he had described a very definite clinical picture, but he (the speaker) was not certain that this clinical type represented a definite form of mental reaction, separable from manic-depressive, schizophrenic, or hysterical groups. He confessed, however, that he felt half convinced.

He would like to discuss briefly the differential diagnosis and treatment of hypochondriacal symptoms as they appeared in a variety of mental disorders. As Dr. Gillespie had pointed out, these symptoms were most commonly met with in the manic-depressive psychoses. That type of hypochondriac was more often missed by the general practitioner, and also by the physician. Such patients came to the neurologist mainly diagnosed as neurasthenics, and perhaps by that time one or more operations had been performed and unnecessary investigations made. The point most often missed, in simple clinical investigation of the type complaining of bodily symptoms without obvious physical cause, was the family history. He frequently found that there had been no special inquiry as to family history of nervous breakdown, though a strong support for diagnosis might have been obtained from such history. Symptoms and signs of value (apart from family history) were self-depreciation and self-reproach, with a slowness of thought and difficulty in concentration and a constant diurnal variation—a tendency to a marked increase in the symptoms in the early part of the day, with a definite lifting of the cloud as the day wore on. In these cases the symptoms must be treated as part of the disease; this was important in differentiating this group.

In the anxiety conditions, as Dr. Gillespie had said, there were definite features. The hypochondriacal symptoms were associated with fear rather than depression; and it was this association with fear which led the psychiatrist to regard these cases as distinct from the manic-depressive group. He (Dr. Symonds) doubted whether this distinction was altogether valid. There were cases of agitated depression, usually accepted as belonging to the manic-depressive group in which

hypochondriacal symptoms were associated with fear rather than depression. These were mainly inaccessible to psychotherapy. Hypochondriacal symptoms associated with anxiety in younger people were more responsive to treatment. There might, he agreed, be fear and conviction of disease in the same patient. He had notes of a striking case, in a boy aged 16, with a history of having done well at his first school and then entered a public school, where also he did well. Six months before he (the speaker) saw him he had begun to worry on account of discomfort in his testicles, in consequence of which he would not sit down at meals, and would only sleep propped up in a certain posture, because lying down increased his distress and made him feel weak. He was taken away from school, and treated with electricity, etc., but without improvement. When seen, he said that disease of his testicles had made him all weak and silly and had altered the colour of his blood, and he felt that something drastic in the way of operation was needed. He had seen a number of consultants, who had all assured him there was nothing physically wrong. He (the speaker) felt that the prognosis was unfavourable, and that the boy would not readily respond to psychotherapy. Next day he saw him, and took the line of refraining from asking questions, giving him, instead, a sort of compressed lecture on sexual physiology and psychopathology. He heard two days later that the boy was practically well, and he was surprised, because of the bizarre symptoms.

Then there was the schizophrenic with hypochondriacal symptoms. His might be a difficult case to diagnose in the early stages; some of these patients in the early stages showed only a settled conviction of possessing physical ailments, and not until later, a general deterioration of personality, and increasing absorption in their own diseases and states. There might here be something analogous to true hypochondria. Might not the hypochondriacs described by Dr. Gillespie be mild schizophrenics whose malady was of the arrested non-progressive type?

He agreed that hypochondria, as defined or isolated by Dr. Gillespie, was a rare condition. Looking over the notes of cases which he had classed under hypochondria, he had found one or two examples corresponding to Dr. Gillespie's description. In one of these, the patient being an old man inaccessible to psychotherapy, there seemed to have been an "expiation" process such as Dr. Gillespie had suggested. The patient had been ill for fifteen years and had a settled pain in one place; he stated that he had a web tied up in the middle of his left groin, which was dragging and trying to get through; he also had a "buzzing" in his head, and said that his symptoms were shaking his whole body. He had spoken of committing suicide. He possessed and had read a budget of literature, and had undergone several abdominal operations. Still, the pain had always been in the same place. Eventually laminectomy had been performed on the assumption that the pain might be due to some infective condition of the posterior nerve roots. All those operations had failed. He had been to an eminent psychotherapist, and the note afterwards written to his doctor was worth quoting as evidence of inaccessibility: "I have now had three interviews with Mr. L. Have you ever tried to shape a piece of flint with a tooth-pick? If so, you know what I feel like." Finally, the patient was sent to himself (Dr. Symonds) by a doctor who hoped that some line of treatment could be suggested and added that if surgical procedure was likely to bring relief, it would be justified. His (the speaker's) reply had been that he did not think any operation—short of decapitation—was likely to help! The patient went to another doctor, and was persuaded to undergo another operation, but, in his own words "it proved a dismal failure, like the others." Recently his wife, to whom he had always been devoted, had become seriously ill, and had had a severe operation. Since her illness, though he was not well, he had been a changed man. His wife declared that the operation did her husband more good than it did her. Having received some stimulation on the affective side, the man came out of his condition to some extent.

The case recorded by Dr. Ross was very interesting; it illustrated the danger of making inaccessibility to psychotherapy a criterion of diagnosis.

Dr. F. Parkes Weber said he had been surprised to hear so many classes of cases referred to. The patients in these cases had certainly had what might be termed hypochondriacal symptoms, but he would have thought that the term hypochondria should be restricted to the kind of case, mostly in old men, to which Dr. Ross alluded. An equally typical example was an elderly business man in London, who had consulted him (Dr. Weber) long ago, because he believed or feared that he had commencing cancer of the tongue. In reality he had no signs or symptoms of it, and was perhaps for a time persuaded that he had not. He consulted doctors at intervals and seemed to be temporarily reassured. He liked to meet a doctor in Hyde Park and discuss his condition with him. He could, however, talk on other subjects and carry on his life in the ordinary way, and was a shrewd man in business. He (Dr. Weber) would restrict the term "hypochondria" to cases of that type, and would exclude cases of women who had undergone repeated abdominal and pelvic operations for imaginary diseases or misleading symptoms.

Dr. Frederick Dillon said two points had often impressed him in regard to hypochondria, and he hoped Dr. Gillespie would deal with them in his reply. He referred in the first place to the sadism which was often expressed in the hypochondriacal state. The amount of hostility which could be got rid of in this way was extraordinary. This type displayed somewhat different psychological mechanisms from the patients which insisted on operations. It was difficult to conceive that the man who underwent half a dozen operations did so from direct egoistic motives. The most probable explanation seemed to be that he was identifying himself with someone else, and so gaining antagonistic satisfaction in that way.

Dr. John Carswell said it had been his personal hope that the term hypochondria would be restricted to the involutional case, which sometimes had dramatic, even tragic sequelæ. He had known the symptoms which had been described, in the early stage of general paralysis of the insane; before the theory of the syphilitic origin of that disease he had been puzzled by the prominence of hypochondriacal symptoms in occasional cases. Sometimes the conditions were evanescent, and they might appear in early stages of dementia præcox. Throughout the whole range of mental disorders, in fact, even at the onset of an attack of acute mania, there might be a short period during which the patient was preoccupied with his own depressive bodily feelings; it was so in delirium tremens before the actual delirium stage. He had never known a person recover from the physical and mental conditions outlined, who had not taken in hand his or her own medical treatment, even to the extent of defying the doctors. There was one patient whom he had been watching for nearly forty years. Thirty years ago she had passed through a distinct condition of depression, associated with wasting and various abdominal complaints and insomnia lasting for months. She had recovered, but since then had had several attacks of acute abdominal symptoms, two associated with rise of temperature and vomiting; on two occasions operative measures were considered but were "turned down." She had taken her own treatment in hand, and for thirty years she had been using purgatives of every description; her age was now 73 and she looked 60. She took purgatives habitually in doses that would alarm most doctors.

During this discussion he had been wondering why patients with morbid references and ideas about bodily functions were spoken of in terms of depression. The valetudinarian, who shut out of his consideration all life's interests except looking after the welfare of his own body, was frequently, at least, a "jolly person," but he was equally disturbed about being too fat and afraid of being ill. One such man lived well, golfed and enjoyed himself; he had been offered a more strenuous post with a larger salary, but he preferred his present mode of life. Any instructions about himself he managed to circumvent by various subterfuges, and he remained quite well. Possibly on reaching 60 years of age, or thereabouts, he might realize that he had misspent his life, and might develop a bad phase of hypochrondria, i.e., the senile form. His (Dr. Carswell's) contention was that the symptoms which had

been referred to in this discussion would be found spread over the whole domain of psychiatric work.

Dr. Robert Hutchison said that as a general physician he was not familiar with the terminology of the modern psychiatrist, and therefore some of this interesting discussion had not been very intelligible to him. He saw the hypochondriac—as every general physician saw him—as an ordinary harmless person, who certainly was not insane. He agreed with Dr. Gillespie's description of him as usually an elderly man, who had retired from business and who collected symptoms, very much as another elderly man might collect postage-stamps or old china. In fact, his health had become a hobby for him. True, most of the items in his collection were fakes, but now and again he came upon something real ; this the physician was apt to overlook, being under the impression that all his symptoms were fakes. Such a person was not only harmless, he was incurable. And he (the speaker) did not want to cure him ; indeed, from the patient's point of view, to do so would be unkind. Why should the physician deprive such a man of the interesting study of his symptoms?

It had been said that the hypochondriac was egocentric; but that was not always the case, hypochondria might be vicarious hypochondria. There were numbers of parents who were desperately hypochondriacal about their children, always imagining symptoms and ailments for them, not for themselves. The same thing might also be seen in the case of an unmarried daughter who lived with aged parents, and was always fussing about their health.

Hysteria seemed to him to be something different, though it was not easy to put that difference into words. The hysteric seemed to be more "ad hoc," to have a definite object and purpose about his manifestations, and for this end it was an advantage to have an illness. The hypochondriac, on the other hand, simply collected symptoms, and fussed about them in a purposeless kind of way, his only object being to give himself something to do.

Dr. Gillespie (in reply) said he was not sure that the pain in hypochondria was not a "real" pain. Dr. Allison's remarks savoured of the old scepticism as to the reality of hysterical pain. As to preoccupation being "justified," the term was a little unfortunate; he should have said "disproportionate preoccupation." He did not think that the person who complained of the symptoms of visceroptosis was the same kind of person as the hypochondriac. Dr. Reynell had suggested that these cases were more accessible to psychotherapy than he (the speaker) was inclined to believe; but Dr. Reynell said he wanted to make a wider definition of hypochondria, and it was within the wider limits of such a definition that he would probably find patients accessible to that form of treatment. His impression of "the one cured case" referred to by Dr. Ross was that the patient was an "old woman." He agreed that there was fear of microscopical pathology, just as there was of macroscopical pathology.

With regard to the boy with testicular pain, mentioned by Dr. Symonds, the peculiar nature of the complaint might have been largely accounted for by the inadequate physiological knowledge of the populace, even the supposedly educated portion of it. His cure in two days was remarkable, and told against the "settled" conviction which Dr. Symonds assumed. It was true that schizophrenic patients might have only hypochondriacal complaints at first, but he wondered how often they expressed feelings of guilt, especially about masturbation, at an early stage ; the ordinary hypochondriac did not express such feelings. He had had cases exhibiting intermediate stages between hypochondria and schizophrenia. Schizophrenia had such a wide connotation that many psychiatrists looked upon hypochondria as one of its subdivisions. Dr. Dillon had raised a question about sadism in reference to these people. He (Dr. Gillespie) considered that they were comparatively inaccessible to the special methods of psychological investigation which alone would allow of probable conclusions on such a point. He had tried to exclude depression from the picture, as not being part of the pathology, and that would answer a point of Dr. Carswell's.

Section of Neurology.

[Cases shown at the Clinical Meeting at the West End Hospital for Diseases of the Nervous System, November 8, 1928.]

Familial Ataxia Resembling Disseminated Sclerosis.—Fergus R. Ferguson, M.D.—(I) C. P., female, aged 45, complains of unsteadiness in walking, dizziness, and mistiness of vision.

History.—About nine years ago noticed that she began to stagger when she walked—her legs were heavy, weak and unsteady. Unsteadiness and weakness have become gradually more evident; no remissions. For two years has had some difficulty in talking; twelve months ago noted weakness of hands; for the past six months has been troubled with pins and needles, tingling and numbness in the arms and legs.

For two years past has had numerous attacks of mistiness of vision—"things seem to run into one another"—and for twelve months there has been transient diplopia. Precipitancy of micturition, with occasional incontinence, since a confinement two years ago.

Family History.—Sixteen members of the family appear to have been affected with a disease, of which the signs and symptoms closely resembled those exhibited by the patient. Eight of these have died, but no post-mortem information can be obtained.

On Examination.—Speech indistinct, deliberate and hesitant.

Visual acuity: $\frac{6}{6}$ right and left. Fundi: discs and vessels normal. Fields: full, no scotomata. Pupils normal, deficient upward movement of the eyes; fine sustained nystagmus to right and left. Intention tremor—upper and lower extremities right and left. Diminution to pin prick and heat and cold over both legs, trunk and arms, more marked on the left side. Vibration sense and sense of passive movement and position diminished over both legs.

Abdominal reflexes absent. Plantar reflexes: very slight responses—indefinite extensor on right side. Arm and leg jerks brisk and equal. Well marked Rombergism. No scoliosis. Slight bilateral pes cavus.

(II) W. D., brother of C. P. (Case I), aged 50, complains of difficulty in walking and inability to talk properly.

History.—Ten years' weakness and numbness of legs, which have gradually increased; for the past eighteen months has been unable to walk unaided. Six years ago legs began to tremble and he noticed weakness in arms. Diplopia and hesitancy of micturition for the past three years. One year ago speech rapidly became impaired; for past six months has had some difficulty in swallowing food.

On Examination.—Facile, pathological laughter: speech indistinct, slurring and lalling. Visual acuity $\frac{6}{12}$ right and left. Fundi: Definite pallor on temporal side of right disc—left disc shows a deep physiological pit with a small ring of pallor around. Fields: Slight peripheral constriction in both fields, no scotomata. Pupils normal, deficient upward and lateral movement of the eyes. Diplopia on looking to the left; well marked nystagmus to right and left. The facial muscles are weak, especially on the left side. Tongue protrudes poorly and is very tremulous.

Power in arms fair and equal; slight ataxia right and left. Power at hip- and knee-joints, good; dorsiflexion and plantar flexion at ankle-joints much weaker than normal. Constant adductor spasm and frequent flexor spasms of legs. Rhythmical tremor of Parkinsonian type affecting both legs. Diminution to pin prick and heat and cold over both legs, trunk and arms. Vibration sense diminished both legs and arms. Sense of passive movement and position diminished over both legs.

Abdominal reflexes present. Plantar responses: both extensor. Arm-jerks

sluggish. Knee-jerks present; every fourth or fifth attempt eliciting a response. Ankle-jerks absent. Well marked Rombergism. Thoracic kyphosis.

Wassermann reaction (blood) negative.

Chronic Progressive Hypertrophic Interstitial Neuritis.—W. ESMOND REES, M.B., B.S. (introduced by Dr. MACDONALD CRITCHLEY).

C.B., male, aged 22, complains of weakness and wasting of arms and legs, dating from age of 2 years and 7 months. A maternal cousin died at the age of nine months from "paralysis."

History.—Patient, apparently a normal child up to the age of 2 years and 7 months—then right leg and arm seemed to be weak. Right foot became inverted and an unsuccessful attempt was made to reduce this deformity when patient was aged 5. About this time left arm and leg were similarly affected and from this point slowly progressive weakness and wasting have been noted. Little or no pain. Eighteen months ago "corn" appeared on sole of left foot—this has since ulcerated.

Present Condition.—Strand of congenital connective tissue seen over left disc—fundi otherwise normal. Deficient outward movement of both eyes more marked on right. Pupillary reactions normal—some weakness right lower half of face. Gross wasting and weakness of muscles of arms and legs, more marked distally than proximally; contracture of left wrist; right foot fixed in inversion, left foot shows equino-varus deformity. Tendon reflexes all absent, abdominals present, plantar responses not elicited; some scoliosis and tilting of pelvis. Some peripheral sensory loss in upper and lower limbs to pin prick and cotton wool. Both ulnar nerves swollen and palpable. Both external popliteal nerves palpable. All nerves painless on palpation.

Encephalitis Periaxialis Diffusa (Schilder's Disease).—E. D. MACNAMARA, M.D.—D. I., male, aged 4 years and 4 months, admitted to hospital June 22, 1928.

History.—Patient was well until March, 1927, when he had a convulsive seizure, and since then he has had several attacks suggestive of petit mal; has been unable to walk properly since December, 1927. On examination, May, 1928, the only physical sign observed was a slight degree of spasticity of the left arm and leg, but it was noticed that he had a wandering look in his eyes. On admission, June, 1928, he was unable to walk without support. Before the end of 1927, he had been able to discern pictures: on admission it was doubtful if he could see at all. The pupils reacted to light. Mr. Lindsay Rea reported "primary optic atrophy, with diminished size of the vessels, and peppering of the retina as in degeneration." The plantar reflexes were extensor, otherwise there were no abnormal physical signs. Hearing appeared normal. Cold air to the right ear gave nystagmus in 30 seconds, and to the left ear in 40 seconds. Jerky movements were frequent, chiefly on the left side. Blood and cerebro-spinal fluid were normal. At the present time his hearing has become very bad, he is wet and dirty, unable to stand without assistance, and shows little evidence of any mental process.

Family and personal history reveals nothing of importance.

Encephalitis Periaxialis Diffusa of Schilder (?).—C. WORSTER-DROUGHT, M.D.—H. G., female, aged 11 years. First seen February 1, 1928, complaining of loss of use of right arm.

Previous History.—Measles, chicken-pox, whooping-cough, and scarlet fever.

Family History.—Nothing of importance. Six other children in family, all healthy.

History of Present Illness.—On December 31, 1927, patient complained that she could not properly move fingers of right hand. In three weeks' time, gradually lost use of right arm from below upwards, and right foot was becoming weak.

On Admission, February 1, 1928.—Cranial nerves and sensation normal: complete motor paralysis of right arm, with increased deep reflexes; weakness at right ankle; knee and ankle jerks exaggerated; bilateral patellar clonus and ankle-clonus; both plantar reflexes extensor. Abdominal reflexes: right absent, left sluggish.

Subsequently the following symptoms were noted: irritability with occasional attacks of hearty and uncontrollable laughter for no apparent reason, and similar, less frequent attacks of crying. Slow and deliberate speech, sluggish pupillary reactions; conjugate deviation of eyes to right, with inclination of head to left; weakness of both external recti, without obvious paralysis; sluggish left corneal reflex. Right-sided facial weakness on voluntary movement; emotional movements on left side of face less free than on right. Increasing spasticity of right arm, with irregular movements on attempting to use it. Loss of power and proximal spasticity in left arm, right arm-jerks being greater than left. Clonic movements on attempting to use left arm. Spastic paresis of both legs, right more than left. Loss of both abdominal reflexes. Knee and ankle jerks increased, right greater than left, with bilateral ankle-clonus. Right plantar, invariably extensor; left, variable. Impaired sense of position in right fingers and toes, with diminished vibration sense over right tibia; sensation has otherwise been normal throughout.

During July, elevation of both left and right arms above the head occurred when she attempted to look up, to speak or to make any movement with the limb of the opposite side. Early in August the attacks of crying ceased, but the outbursts of uncontrolled laughter continued. The optic discs have been normal throughout. Wassermann reaction negative in blood and also in cerebro-spinal fluid, which shows no abnormality.

Present Condition.—Mentally is moderately alert, but laughter is exaggerated and difficult to control. Pupils: moderate in size and react normally; optic discs clearly defined with bilateral temporal pallor; visual fields normal. Ocular movements now normal, but difficulty in maintaining fixation on an object. No nystagmus. Some rigidity of facial muscles. Speech normal.

Some rigidity of neck muscles with slight lateral flexion of body to left. Right arm spastic with hyperpronation of forearm and acute flexion of wrist and extension of fingers. Arm-jerks brisk, left greater than right. Lower limbs: Spasticity mainly affecting extensors, the limbs being in position of full and rigid extension. After a pause, knees can be flexed voluntarily but only slight dorsiflexion at ankles is possible. Knee-jerks and ankle-jerks brisk, left greater than right. Bilateral ankle clonus. Plantar reflexes variable, usually extensor. Abdominal reflexes sluggish, left more so than right. The muscular rigidity appears to be a mixture of pyramidal and extra-pyramidal type.

Progressive lenticular degeneration (Wilson's disease) is excluded by the signs of pyramidal involvement, also by the absence of involuntary movements and the family history.

Encephalitis Periaxialis Diffusa of Schilder (?). C. WORSTER-DROUGHT, M.D., and L. MANDEL, M.D.—H. G., female, aged 4, brought to hospital with complaint that she had been unable to walk for one year and nine months.

History of Present Illness.—One year and nine months before, she had a cough for four weeks and became very weak. Shortly afterwards a squint developed in the right eye, and about the same time there appeared some difficulty in walking. At first, the child got about with the support of the furniture, but later she crawled from place to place. About two months before admission the vision was thought to have failed and headache and vomiting were present.

Previous Health.—Full-term child; delivery normal; walked at fifteen months; talked at eighteen months; developed normally both physically and mentally. Went to school at three years. No illnesses.

Family History.—There are two sisters, aged 6 and 2½, both healthy.

Condition on Examination.—Well developed and moderately intelligent; answers questions when confidence is gained. Head abnormally large, but nothing grossly abnormal on X-ray examination of skull. Cranial nerves: pupils moderate in size, equal, react sluggishly to light and briskly to accommodation. Vision: fails to see people until quite close; can see fingers. Double optic atrophy. Ocular movements: deficient lateral deviation to right and left, but can sometimes complete the movement in both directions. Right corneal reflexes sluggish. Voluntary movements of left side of face weak. Other cranial nerves normal. Sensation normal. Upper limbs: motor power good; tendon-jerks present and equal. Lower limbs: motor power fairly good; knee-jerks exaggerated and equal; ankle-jerks exaggerated, right more active than left; no ankle clonus; both plantar reflexes extensor. Abdominal reflexes present, left greater than right. Gait: stands only with support, and then on a broad base; attempts at walking result in the legs being thrown forward stiffly in turn: there is no attempt in balancing. On attempting to rise from the recumbent to sitting position both legs rise up from the bed. No spontaneous movements. Coördination good in the arms. Sphincters: some urgency of micturition.

Cerebro-spinal fluid: four lymphocytes per c.mm.; protein 0·062; Wassermann reaction negative. Blood: Wassermann reaction negative.

As regards diagnosis, congenital hydrocephalus appears excluded by the fact that the child was normal up to the age of one year and nine months, while the illness at that time does not seem to have been sufficiently severe to justify a diagnosis of meningitis with secondary hydrocephalus; further, her mental condition is practically normal. Hereditary ataxia of the Marie type is possible, but this condition scarcely develops at so early an age as four years, and there is no definite family history.

Encephalitis Periaxialis Diffusa (Schilder's Disease) [?].—R. EVELYN LUCAS, M.B. (introduced by Dr. C. WORSTER-DROUGHT).—F. H., male, aged 9, first seen June, 1928, complaining of deafness.

Family History.—Eldest of four. Rest healthy. Father alcoholic; no other abnormality in family history.

Previous History.—One fit, of epileptic type. at two years, otherwise healthy.

History of Illness.—Apparently sudden onset of almost complete bilateral deafness, March, 1928, immediately following shock. Deafness variable in degree. June, 1928, dragged left leg in walking and vision became impaired. July, fundi normal.

On admission, July 16, 1928.—Deafness, ataxia, deep reflexes sluggish in upper limbs, brisk in lower limbs, incoördination of left arm and leg, abdominals brisk and equal, light and accommodation reflexes sluggish, speech normal, behaviour normal except for occasional apathy.

Present Condition.—Increase of deafness and impairment of vision. Light and accommodation reflexes sluggish; no nystagmus; external ocular movements normal, slight right facial paresis, increased ataxia, cerebellar posture, sluggish arm reflexes, brisk knee and ankle jerks, no knee clonus, slight ankle clonus; both plantar reflexes extensor; incoördination of both legs. Abdominal and cremasteric reflexes present. Slight pallor of optic discs (? primary optic atrophy) insufficient to account for degree of visual impairment. Blood: Wassermann reaction negative. Cerebro-spinal fluid: Wassermann reaction negative, cells 4, protein 80 mg. per cent., Lange 5554432100 (repeated and confirmed).

Gradually increasing apathy alternating with noisy excitement.

Disseminated Sclerosis with Argyll-Robertson Pupil.—C. WORSTER-DROUGHT, M.D.—A. L., female, aged 39, admitted on October 4, 1928. *Complaints.*—(1) Difficulty in using right arm and leg. (2) Nervousness. *Duration.*—Eighteen years.

History of Present Illness.—Was healthy until aged 21. Then, over a period of three months right leg dragged; right arm was affected later and shook whenever an attempt was made to use it. Some improvement in leg for a short time. Condition remained stationary until two years ago when symptoms increased in severity. Since then, there has been difficulty in using right arm and tremor on trying to do so. In lower limbs, disability has apparently been confined to right leg. For two years there has been urgency of micturition. Patient becomes depressed and excited easily, and is irritable at times. *Previous History.*—Negative. *Family History.*—Father died aged 39 from "epileptic fits." Mother died aged 70 in a mental hospital where she had been for many years.

Condition on Examination.—Cranial nerves.—Pupils: moderate in size and equal; right pupil reacts very sluggishly to light and consensual stimulation, and briskly to accommodation; left pupil does not react to light or consensual stimulation, but reacts to accommodation. Ocular movements: defective lateral deviation, more obvious on looking to left than to right. No nystagmus. Voluntary movements of face are weak on right side. Other cranial nerves normal. Sensation: vibration not appreciated in legs; sensation otherwise normal. Upper limbs: motor power less in right arm than in left; deep reflexes exaggerated in right arm and more active than in left. Lower limbs: motor power decreased in right leg; tone increased slightly in extensors; foot inverted; knee- and ankle-jerks exaggerated on right side and more active than on left; no ankle clonus; both plantar reflexes extensor. Abdominal reflexes absent. Gait: dragging of both legs, more obvious in right. Coördination: intention tremor more obvious on right than left, slightly increased with eyes closed. Rombergism present. Sphincters: urgency of micturition. Blood-pressure: systolic 100; diastolic 70. Other systems normal.

Mental: There is some lack of control of emotional expression.

Cerebro-spinal fluid: three "small lymphocytes" per c.mm.; total protein 0·035 per cent.; globulin a trace in excess; Wassermann reaction negative; Lange test, 5444421000. Blood Wassermann negative.

Subacute Combined Degeneration of Spinal Cord without Anæmia.—I. M. ALLEN, M.D.—T. D., male, aged 51, admitted October 10, 1928.

Complaints.—(1) Difficulty in walking. (2) Lack of sensation below waist. (3) Painful defæcation.

History of Present Illness.—Ten months ago patient had a feverish cold and stayed away from work for two months. Following this, legs felt weak on walking; this difficulty gradually became worse, and seven months ago he staggered and reeled as he walked. During the past seven weeks there has been difficulty in beginning micturition. If constipated, defæcation was painful. There has been a "cold feeling" below waist and extending down both legs. *Previous Health.*—Malaria sixteen years ago; gonorrhœa twenty years ago. *Family History.*—Nothing of importance. *Condition on Examination.*—Cranial nerves normal. Sensation: subjectively, cold feeling below waist, and occasional slight tingling in fingers; objectively, all sensation in arms normal; in legs, vibration not appreciated, sense of position impaired, and other types of sensation normal. Upper limbs: motor power poor; tone decreased; all deep reflexes very sluggish. Lower limbs: motor power poor; tone decreased; no wasting; knee-jerks exaggerated and equal; ankle-jerks present and equal; no ankle clonus; plantar reflexes, right flexor, left extensor. Abdominal reflexes absent. Gait: stands with difficulty: can walk with help of sticks, but movements of legs are uncontrolled. Coördination: finger-nose test, poor with left hand, somewhat better with right hand; almost impossible to carry out heel-knee test with eyes closed. Romberg present. Sphincters: difficulty in beginning micturition. Heart and pulse normal. Blood-pressure: systolic 130; diastolic 90. Poor expansion of chest, breathing shallow. Other systems normal.

Cerebro-spinal fluid.—Normal; Wassermann reaction, negative.

Blood.— Red blood-cells 5,210,000; white blood-cells 10,100; hæmoglobin 86 per cent. (Gowers); colour index 0·82. Differential count: polymorphonuclears 80 per cent.; lymphocytes 19 per cent.; large mononuclears 0·5 per cent.; mast cells 0·5 per cent. Only very slight anisocytosis and no poikilocytosis; no nucleated or stippled cells seen. Wassermann reaction, negative.

Fractional Gastric Analysis.—No free hydrochloric acid in resting gastric juice or for two hours after test meal; no acid in specimens up to one hour, and in subsequent specimens total acid varied from 6·0 to 9·0; in each case specimen gave positive reaction for organic acid.

Skiagram.—Spine normal; stomach, after bismuth meal, negative.

Syringomyelia and Syringobulbia.—I. M. ALLEN, M.D.—E. P., female, aged 31, was admitted on August 9, 1928, complaining of stiffness of both arms and left leg of eight months' duration.

History of Present Illness.—Eight months ago, when patient was in poor general health, left arm and leg gradually became stiff. The right arm became numb and latterly has become stiff as well. During past six years painless blisters have developed on both hands. Seven years ago she suffered from septic conditions of both hands. The speech has become thick. Attacks of giddiness occurred eight months ago.

Previous and Family History.—Nothing of importance.

Condition on Examination.—Cranial nerves: pupils small, oval and eccentric, left smaller than right; do not react to light or consensual stimulation, do not dilate on shading; react to accommodation. Speech: slight slurring of syllables. Hearing decreased in left ear more than in right; bone conduction more prolonged than air conduction. Trigeminal nerve: pain sensation absent, and heat and cold sensation diminished over distribution of nerve on right side. Voluntary movements of face are stiff. Other cranial nerves normal.

Sensation: Cotton-wool sensation normal. On left side, pain and heat and cold are lost over distribution of the cervical nerves; on right side down to the level of fifth cervical nerve and over right hand. Deep pressure is diminished in both arms, more in left arm than in right. Vibration is not appreciated in arms nor in left leg.

Upper limbs: spasticity of both arms, right more than left; wasting of interosseal, thenar and hypothenar eminences, more on right side; claw hand on right side; deep reflexes all absent. Lower limbs: left leg spastic and motor power decreased in flexors; knee-jerks exaggerated, more active on left; both plantar reflexes flexor. Abdominal reflexes absent. In walking left leg moves stiffly. Coördination normal. Sphincters normal. Blood-pressure: systolic, 145; diastolic, 90. Diminished vaso-motor control in the analgesic areas. Scoliosis of the upper dorsal spine with the spines to the right. Other systems normal. Cerebrospinal fluid normal. Wassermann reaction, (blood) negative.

This patient is being treated with deep X-ray therapy. Following three exposures the sensation in the trigeminal area became normal.

Section of Obstetrics and Gynæcology.

President—Dr. J. S. FAIRBAIRN.

[*October* 19, 1928.]

Case of Primary Carcinoma of the Vagina.—A. C. PALMER, F.R.C.S.—Patient, aged 24, had complained of a constant blood-stained discharge for two months. Inspection revealed an ulcer 1½ in. by 1 in. on the posterior vaginal wall, just within the hymen. Histological examination proved the condition to be a solid, trabecular, squamous-celled carcinoma. Treatment consisted in local removal, with the idea of using radium at a later date. Three months later the patient was found to have a large malignant ulcer on the anterior rectal wall, with infiltration. Radium was inserted, but she died about two months later.

Specimen of Pregnancy in a Completely Detached Left Horn of a Uterus Bicornis Unicollis.—Shown by Sir EWEN MACLEAN, M.D..—The patient exhibited a normal menstrual history, and had had one child and one miscarriage. A six months' amenorrhœa was recorded, and routine examination showed a tumour apart from an apparently unimpregnated uterus. A mummified fœtus of about four months' gestation was found lying in the sac, which appeared to be a completely detached horn. It seemed that fertilization had taken place by the internal migration of spermatozoa from the right tube to the left, and it was questionable whether a minor degree of torsion of the pedicle, which was found at operation, was not the cause of intra-uterine death.

Genito-Urinary Fistula in the Female: with an Appreciation of Sims and his Work.

By DOUGAL BISSELL, M.D. (New York).

THE compliment which you pay me in inviting me to address you upon the subject of genito-urinary fistula in the female I recognize to be in reality paid to the "Woman's Hospital in the State of New York," and as an introduction, may I be permitted to recall briefly the dramatic origin of this institution, which is to-day a living and growing monument to its founders, Sims and Emmet.

From the southland of the States to the Metropolis of the nation there came, in 1854, the surgeon J. Marion Sims in search of health. His theoretical education and practical training in the art and science of medicine had been only that acquired by the average American student of his day. Although poorly equipped, he possessed mechanical ability, imagination, good judgment and an ardent desire to serve humanity. The great lesson taught him by the hardships of the wilderness was self-reliance; with this, ingenuity, and the love of scientific adventure, he met his daily problems. In the district from which he came he had been a pioneer and leader in the field of surgery, but the metropolis knew him not. In his great struggle for recognition in New York he showed not only the spirit of youthful enthusiasm, but a rare knowledge and skill in surgery, acquired by his experience in the backwoods of Alabama. Single-handed he fought to arouse interest in the operation he devised for the cure of vesico-vaginal fistula, but professional jealousy overwhelmed him and his efforts seemed doomed to failure, when the timely sympathy and aid of a few far-sighted noble women of influence and means came to his rescue.

In a small private dwelling, during the year 1855, the "Woman's Hospital" was launched upon its career. The profession objected to its existence on the ground that the field of work in which it proposed to engage, viz., destructive injuries of the vesico-vaginal septum, was too limited to warrant an institution for their special care and treatment. Sims' experience, however, had awakened him to the realization of the fact that throughout the land there had existed, for many years, an

accumulated number of women made derelicts by this tragedy of child-birth; and still more important was the fact that the surgical staff of no institution then existing in New York was capable of relieving a single victim of vesico-vaginal fistula. The element of particular interest here is that the Woman's Hospital was organized solely for the purpose of curing vesico-vaginal fistula, and the justification of its founding soon became apparent, for, as its reputation kept pace with Sims' and Emmet's remarkable operative achievements, it outgrew its capacity and expanded into an imposing structure, to be known wherever surgery was known.

That you may better visualize the sufferings of those whom Sims sought to relieve, permit me to relate briefly the story of Mary Smith, the first patient upon whom Sims operated at the Woman's Hospital. She had but recently arrived in America as an immigrant from the Western Coast of Ireland, a pitiable, ill-smelling, repulsive creature, with an extensively excoriated vulva, the result of a continued escape of urine. When the anterior vaginal wall was exposed, a greyish mass came into view, projecting well into the vagina. It appeared to be a very large stone, but on examination, after its removal, it proved to be a wooden float from a seine, about the size of a goose egg; this had been introduced into the bladder, through a large vesico-vaginal opening, by her medical attendant at home, to support the superior surface of the bladder and to prevent it from protruding through the fistulous opening, forming, with the intestines, a true vesical hernia. The float was, of course, completely encrusted with phosphatic deposits, and its removal was thereby rendered difficult and extremely painful. Most of the base of the bladder and of the urethra was destroyed, but, through the combined efforts of Sims and Emmet, these tissues were reconstructed and restored to function, and for six years or more this woman was employed as a helper in the hospital.

During the first year of the hospital's existence, its surgical records were kept with indifference. This failure, however, soon became apparent to Emmet, who possessed hospital training, and to him is due the credit for their eventual accuracy and for the invaluable information which may be garnered from them to-day. When reviewing these statistics from a numerical standpoint, it should be remembered that before the work on vesico-vaginal fistulæ was systematically begun, it was generally considered too restricted for special consideration; also that the hospital was closed three months of the year, for reasons then thought valid, viz., that wounds did not heal well during the summer.[1]

Emmet states that during his early career as surgeon to the Woman's Hospital, 1856-1861, he operated for the repair of vesico-vaginal and urethro-vaginal fistulæ on an average of twice a week. But a better idea of the actual number of cases which came under his care is obtained through his report at a later date, 1867, analysing 275 cases of injuries to the vesico-vaginal septum.

In reviewing the statistics of the Woman's Hospital during the past ten years (the hospital being open throughout the entire year, and eighteen surgeons operating), we find that only fifty-eight patients have been operated upon for the repair of vesico-vaginal and urethro-vaginal injuries. The question naturally arises, Why this great difference in the number of patients suffering from this condition, namely, between 275 under the care of one surgeon, the hospital being open only nine months of the year during approximately ten successive years; and fifty-eight patients under the care of eighteen surgeons, the hospital being open throughout the entire year during approximately the same length of time? The answer seems apparent, viz., improvement in obstetrical management and the division of material among a great number of surgeons. It would, however, be erroneous to conclude that because the art and science of obstetrics has greatly advanced since Sims' and Emmet's time, the vesico-

[1] Emmet, on another occasion, reported 171 cases, and Sims, eight years after his initial success, reported a series of 261 cases of vesico-vaginal and recto-vaginal fistulæ, of which number 216 were permanently cured; of the remainder 36 he considered curable, and 9 incurable.

vaginal fistula will soon be a thing of the past. The poorly-equipped professional attendant and the attendant who has no scientific training constitute, in reality, a large majority of the workers in the field of obstetrics and, like the poor, will be with us always; but, even under the guidance of trained and competent attendants, vesico-vaginal injuries occasionally occur. It is therefore reasonable to conclude that genito-urinary fistulæ in the female, arising from obstetric causes, will remain with us to tax the ingenuity of the surgeon for generations yet to come. But, while obstetrics will always be the chief ætiological source of vesico-vaginal fistulæ, gynecological surgery upon the pelvic organs of the female will rank next in importance. What was the status of the vesico-vaginal fistula operation in America prior to the advent of Marion Sims? The malady had occasionally been cured, but the successes had been spasmodic, and no surgical effort had yielded a definite plan that could be followed with reasonable hope of success. Of the early American surgeons, George Hayward, of Boston, Massachusetts, contributed perhaps more towards establishing a technique for the closure of vesico-vaginal fistula than had any of his contemporaries. His position as surgeon to the Massachusetts General Hospital brought him in daily contact with advanced medical thought throughout the world, and assured him extensive surgical opportunity. But in speaking of vesico-vaginal fistula and the efforts which had been made to cure it, we find him saying: "It is by no means settled what is the best mode of treating this distressing infirmity." This discouraging attitude, however, was not confined to America, for Henry Earl, in 1829, stated that "under the most favourable circumstances these cases present the greatest obstacle, and are certainly the most difficult that occur in surgery." Hayward began his work on the repair of vesico-vaginal fistulæ in 1839, and continued to be actively interested in it until 1851. During this period he operated twenty times on nine patients, with only three successes. Hayward operated in the lithotomy position, and access to the fistula was gained by lateral wooden retractors; further to facilitate approach, he passed a rubber bougie through the urethra to the base of the bladder near the fistula, and, by raising the handle of the bougie, he forced the fistula towards the vaginal outlet and into view.

With regard to preparing the orifice, he suggested that to avoid injury to the bladder "the membrane of the vagina be dissected up to a considerable extent around the orifice, and that the needle be carried through this at some distance from the edge of the wound."

Hayward's object, it would seem, in separating the bladder and vaginal walls in a limited way, was to produce a broader surface for apposition. I interpret his description of the passing of the needle to mean that the suture should be placed well beyond the angle formed by the separation of the two structures, so that when the sutures were tied the separated surfaces would be forced apart and the broadest surfaces made to appose. But, judging from his poor results, his theory did not prove practical. The probability is that some of the sutures penetrated the separated surfaces, occasioned irregularity in apposition, and prevented a wide contact, thus encouraging seepage with failure of union.

Hayward used dental silk, which offered several objections. Under tension, silk cuts tissue, and its capillary action, in a field surgically unclean, encourages infection of the wound. Hayward candidly admitted that his technique could not be applied where great loss of tissue had occurred. It would seem, therefore, fair to conclude that although Hayward antedated Sims by thirteen years in the surgical feat of curing vesico-vaginal fistula, he failed to develop a technique which yielded satisfactory results, or could be applied to any form of fistula but the very simple.

Hayward may be awarded priority in separating the vaginal wall from the bladder, but as his purpose in doing so was not to unite these layers separately, he cannot be considered the originator of the bladder mobilization or flap-splitting technique of to-day.

The next important effort by an American surgeon was that of Dr. Mettauer, 1847, and the feature of particular interest in his technique is the use of lead sutures.[1]

Before Sims' successful work, European surgeons had taken but little interest in the cure of this disease.[2] Lillemand, of France, 1825, records perhaps the first cure. This was done by the combined use of nitrate of silver and approximation of the cauterized edges of the fistula by a form of stilette passed through the bladder wall. Of the fifteen cases treated, he reported four cures. This technique was applicable only to very small fistulæ.

Dieffenbach's attitude, according to Hayward, was most pessimistic, for he describes the sufferers with fistulæ as being "forced to exclude themselves from society; the very atmosphere surrounding them is polluted by their presence, and even children shun them. Thus rendered miserable, both morally and physically, they yield themselves a prey to apathy; or a pious resignation saves them from self-destruction."

Hayward thought that Dieffenbach's technique was dangerous, because it permitted of the penetration of the bladder. It is evident that Dieffenbach's successes were few.

M. Jobert introduced his plastic method previous to Sims' initial work. The success he obtained was limited, and among the objections offered to it is this—that the transplanted flap, which was usually taken from the large labia may, and on one occasion did, continue to produce its growth of hair. In 1834, Gosset, of London, used a metal suture in the form of gilt wire. There seems to have been much in common in the methods of Gosset and Sims, for the former used not only the metal suture but a perineal retractor, and also operated in the knee-chest position. Although his results were seemingly far better than those of his contemporaries in this field of work, his technique did not gain wide recognition, was not generally accepted and therefore was not perpetuated.

These were the principal vesico-vaginal fistula techniques before that of Sims was given to the world in 1852, and it will be seen that some of them have features in common with and bearing a close relationship to his.

The method which I was taught by Emmet as being that of Sims differs considerably from that described by Sims in his early work. Even the interesting illustrations in Henry Savage's "Female Pelvic Organs" vary from his original plan. The essential differences between his original plan and that which eventually became recognized as his, and by which the operations herein described were carried out, are as follows: In the former the knee-chest position was used, and in the latter the semi-prone, or Sims' position. In the former a short, imperfect speculum was used; in the latter an improved speculum which now bears his name. In the former the direction of denudation about the fistula was made at a right angle to the general direction of the vagina, and did not extend much beyond the vaginal limits of the orifice; in the latter the denudation was made, whenever possible, in the general direction of the vagina, elongated and greatly widened. In the former he used a long, awl-shaped, spear-pointed needle; in the latter, a short, slightly curved, ordinary surgical needle, with a carrying thread to which silver wire was attached. In the former he used what he called the "clamp or quill" suture, in the latter, the simple twisting of the wire over the line of approximated surfaces, the points of the wire being protected by perforated and crushed shot. I am of the opinion that several of these changes were, in part at least, due to Emmet's influence.

1 Mettauer used lead sutures in two of the six cases reported by him. One of these was successful and one, after eight attempts, was a failure. The remaining four, it would appear from his report, were unsatisfactory.

2 To Simon credit should be given for the development in Europe, to the point of success, of the operation for vesico-vaginal fistula. This was done independently of Sims' work, although immediately following it. It proved practical in extensive lesions, and sufficiently simple to be learned and followed by his colleagues.

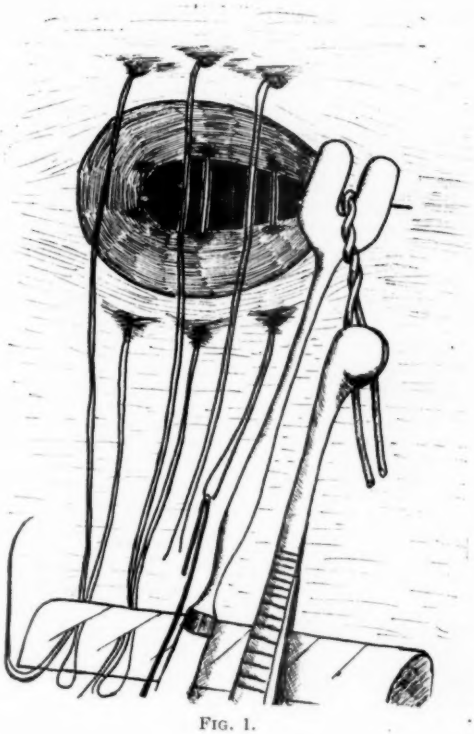

FIG. 1.

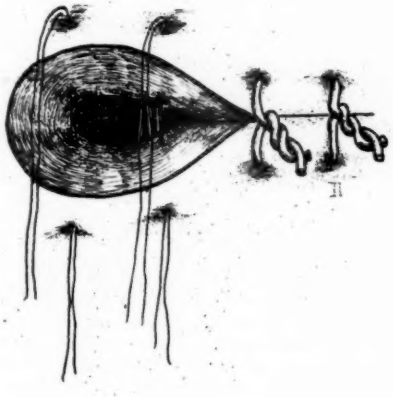

FIG. 2.

FIGS. 1 AND 2.—Illustrating the method of passing and tightening the silver-wire sutures.

The technique used in the cases herein reported was, in brief, as follows: The patient was placed in the semi-prone or Sims' position, with Sims' speculum retracting the perineum.[1] The denudation of the vaginal mucosa was made broad whenever possible, and in the general direction of the vagina. The silver wire suture was attached to a carrying thread which, in turn, was tied to the needle in such a manner as to produce a knot smaller than the heel of the needle; the silver wires were twisted over the line of approximation of the denuded surfaces, and the free ends of the wires were protected by perforated crushed shot.

The features to be emphasized in the Sims technique are the following:—

The denudation surrounding the vaginal orifice of the fistula should be broad and elliptical down to the immediate region of the bladder mucosa, but never involving it.

The needle should penetrate deeply the vesico-vaginal septum, but never involve the bladder mucosa.

The wire loop by which the wire is attached to the carrying thread should be crushed so as to occasion the least possible resistance on being pulled through the tissues (see figs. 1 and 2).

The wires should be twisted only enough to appose snugly the denuded tissues, thus ensuring exactness and uniformity of adjustment which remains constant until the wires are removed.

I present for your consideration, and as the basis for discussion, seven cases of genito-urinary fistula, four of which are vesico-vaginal, one vesico-urethro-vaginal, one uretero-vesico-vaginal and one uretero-uterine.

Six of these involve the vesico-vaginal septum, and are selected from a considerable number that have come under my care during the past thirty-seven years, because they are unusual in type or in origin, and serve to demonstrate the value of Sims' technique in complicated conditions. The seventh has no direct bearing upon the Sims' technique, but is related because of its rarity, and to show how failure may follow any surgical procedure when the true character of the pathology is not understood.

It seemed preferable, in preparing this study, to concentrate upon a few selected cases rather than to report a greater number of simple character which might be cured by any of the varied techniques now in vogue. It is the complicated and unusual type which the surgeon of to-day finds difficult, and for the cure of which he does not always have a method at his command.

My position in this discussion is that the principle in the technique first formulated by Sims is more nearly uniformly applicable to the varied forms of vesico-vaginal and urethro-vaginal fistulæ than any technique used before or since his day.

The following illustrative histories are condensed, and include only such material as bears directly upon the discussion.

CASES.

(I).—*Vesico-Vaginal Fistula complicated by Occlusion of the Urethra at its Vesical Orifice.*

DURING a vaginal hysterectomy performed one year previous to this patient's admission to the Woman's Hospital, the bladder was injured and a vesico-vaginal fistula resulted. Later an unsuccessful effort was made by the same operator to close the fistula. This attempt not only was a failure, but terminated in extensive local deformity and severe interval pain in the lumbar region, occasionally associated with vomiting. A description of the method which had been adopted to close the fistula could not be obtained. Vaginal examination showed a short, vaginal tube with dense scar tissue and a large vesico-vaginal fistula at the vault. Urethral examination showed a complete obstruction of this organ at the vesical neck. The first problem for consideration, therefore, was restoration of the urethral canal.

[1] Neither the Sims' position nor speculum was used in Case VI.

Operation (No. 1) for Restoring the Urethra, October 18, 1923.—Through a transverse suprapubic incision, the bladder was partly detached from the pubic bone. A median longitudinal incision was then made through the bladder wall down to within a short distance of the vesical neck. The vesical opening into the urethra was found to be completely obliterated; there was not even a depression at this point. The base of the bladder was greatly deformed as the result of scar tissue, which began at the area of the neck and terminated at the area of the vaginal vault, where an opening into the vagina was found. The left ureteral orifice could not be determined.

A curved forceps was then passed through the urethra from without the area of occlusion at the neck of the bladder. Its blades were opened and an incision made between them through the bladder wall into the urethral canal. The forceps were then withdrawn and the mucosa of the bladder was stitched to that of the urethra with interrupted No. O catgut sutures. Convalescence was uninterrupted. The reconstruction of the canal was satisfactory, but, as there seemed to be a tendency of the newly-formed orifice to contract, dilating of the canal at intervals was necessitated.

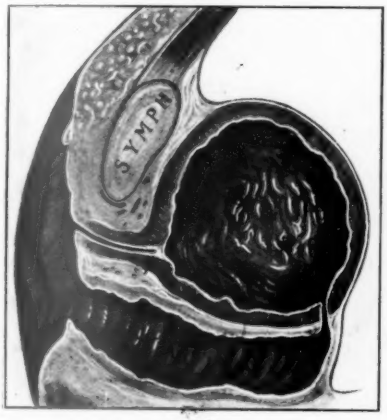

FIG. 3.

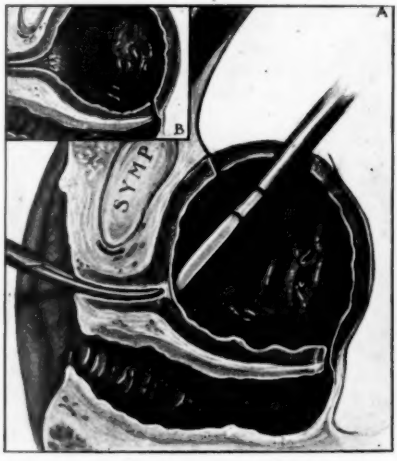

FIG. 4.

Case I (Fig. 3).—Before operation, showing urethral obstruction and fistula.
Case I (Fig. 4).—Urethral barrier being incised. Inset: canal made patent.

Operation (No. 2) for Closure of the Vesico-vaginal Fistula, January 24, 1924.—The vesico-vaginal fistula located at the vault of the vagina was successfully closed by Sims' technique, eight silver wires being used. Sims' catheter inserted. Since then, now nearly four years, the bladder has been functioning normally, but the interval pains in the left lumbar region continue. Result.—Satisfactory.

Comment.—The particular interest in this case is the extent of injury done to the neck and base of the bladder. Not only was the vesical opening leading into the urethra completely obliterated, but the injury along the base of the bladder so involved the left ureter as to hide its orifice, and, judging from the persistency of the interval pain, partly to obstruct the canal. The sequelæ following the first effort made to cure the vesico-vaginal fistula were, therefore, three distinct lesions; complete obstruction of the urethra, partial obstruction of the left ureter, and a vesico-vaginal fistula at the vaginal vault.

The vesico-vaginal fistula at the vault of the vagina was surrounded by dense scar tissue, with the vaginal wall and bladder anterior, and the vaginal wall and peritoneum posterior. It might fairly be said, in regard to the closure of the vesico-vaginal fistula, that, in view of the amount of scar tissue at the base of the bladder involving urethra, ureter and vaginal vault, any method which called for the separation of the bladder from the vaginal wall would,

under these circumstances, have been not only difficult, but dangerous to apply, and if by chance successful could not have accomplished more than the method adopted.

(II).—*Uretero-vesico-vaginal Fistula with both Ureteral Orifices Exposed Vaginally.*

Reported in the *Bulletin of the Woman's Hospital* (October, 1913).

An abdominal hysterectomy was performed by me upon this patient at the Woman's Hospital, January, 1912, because of an extensive carcinoma of the cervix. The malignant growth extended from the right side of the cervix, well into the pelvis, retro-peritoneally. The pathologist's comment was: "This gross specimen is one of the largest cervical cancers I have ever seen successfully removed." The growth completely surrounded the right ureter at the base of the broad ligament, and measured 5 cm. in length and 4 cm. in diameter. The uterus, its adnexæ, the growth and more than 5 cm. of right ureter were removed. In so doing, it was found necessary to remove a considerable portion of the base and posterior wall of the bladder; the free end of the right ureter was anchored, under considerable tension, to the right angle of the bladder opening and vaginal drainage was established. The patient made an uninterrupted recovery, but the bladder wound opened completely at the vault of the vagina. Vaginal examination showed a large vesico-vaginal fistula crossing the vaginal vault from the implanted ureteral orifice on the right side to the normal ureteral orifice on the left side. The orifices of both ureters were distinctly seen *per vaginam*.

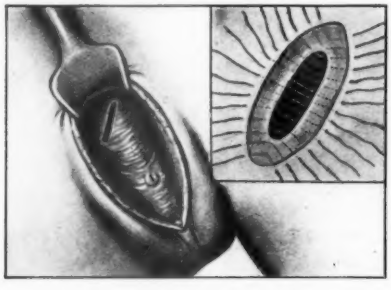

FIG. 5.

Case II.—Both ureteral orifices exposed vaginally. Inset: repair of fistula with fifteen silver-wire sutures.

Operation for Closure of the Vesico-vaginal Fistula, January 25, 1912.—Six weeks after the operation above described the edges of the fistula appeared healthy, and repair by the Sims method was made. Because of the exposed ureteral orifices, denudation of the vaginal surfaces about the fistula was done with great care, so as not to encroach upon the ureteral orifices. Fifteen silver wire sutures were required to ensure proper apposition. Their placement had to be so manipulated as to invert the ureteral orifices into the bladder, yet not constrict them. The wires were removed at the end of two weeks. Primary union resulted. The entire operative field remained healthy until her death, which followed the primary operation in about six months as the result of metastatic developments in the upper abdomen. Result—satisfactory.

Comment.—This is the largest vesico-vaginal fistula which I have ever closed or seen closed. Further, it is the only case I have operated upon where both ureteral orifices could be seen *per vaginam*. It is of interest also from the fact that the initial operation so radically eliminated the cervical cancer which had extensively involved adjacent tissues, that in six weeks time repair was made in a tissue which showed no evidence of post-operative pathology. No other technique could have accomplished more, in this case, than did Sims', and I think that the success of the application of any other would have been extremely doubtful.

(III).—*Vesico-vaginal Fistula formerly Vesico-abdominal Fistula.*

Following the second labour of this patient, pelvic infection occurred, culminating in an abscess at the base of each broad ligament. One abscess ruptured in the region of Poupart's ligament on the left side, and the other ruptured in the same region on the right side. From the left side pus and urine escaped; from the right side only pus escaped. The right side closed spontaneously, but the left continued to discharge pus and urine. At the end of three months a puncture was made at the base of this abscess cavity to drain it into the vagina. The opening over Poupart's ligament then closed, but leakage of urine into the vagina became continuous. This patient came under my care one year after the initial lesion. Vaginal examination in Sims' position showed an opening on the anterior vaginal wall, 2½ cm. from the cervix and to the left of the median line. A cystoscopic examination was made after plugging the opening on the vaginal side, and a slit at the base of the bladder, 1 cm. in length, was seen in close proximity to the left ureter.

Operation for Closure of the Vesico-vaginal Fistula, April 22, 1926.—Immediately before the operation to close the fistula, a catheter was passed into the left ureter, to remain as a guide throughout the operation. In order to bring the field closer to the introitus, a catgut suture was passed through the anterior lip of the cervix, then through the tissues of the posterior fornix. On tying this suture the cervix was forced well back out of view, the

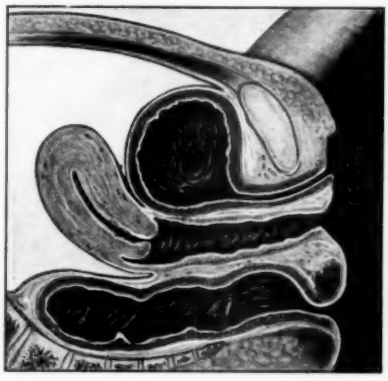

FIG. 6.

Case III.—Catheter in left ureter: demonstrating close proximity of fistulous opening to ureter.

anterior wall put on greater tension and brought closer to the operator. A wide and long denudation was made on each side of the fistulous opening, in the direction of the long axis of the vagina. Eight silver wire sutures were inserted to approximate the denuded surfaces. Throughout the operation the ureteral catheter was moved up and down, to determine whether or not the stitches included the ureter. A cystoscopic examination was made immediately after the operation. No suture could be seen exposed on the vesical surface. No leakage discovered on distending the bladder. Sims' block-tin catheter was inserted. Result—satisfactory.

Comment.—This case presented no special difficulty in regard to the closure of the fistula, excepting that care had to be taken, because of the proximity of the fistula to the ureter, not to include the ureter in the stitches. It is difficult to say whether the original injury was ureteral or vesical, but when an abscess occurs at the base of the broad ligament the ureter is always endangered, as illustrated by Case VII, to be described. It is possible, therefore, that the original injury was to the ureter and, when the blind puncture was made at the base of the abscess cavity, the bladder wall was punctured. The ureteral injury then closed and the vesical injury remained.[1]

[1] Sims performed several hundred operations for the repair of vesico-vaginal fistulæ before he lost a case by death. This death occurred with the first patient upon whom he operated in England, in the early 'sixties, and the autopsy showed that the ureters were encircled by the stitches. As far as I am able to discover, this is the only accident of the kind which he had.

(IV).—*Vesico-vaginal Fistula with Cervix in the Bladder.*

Ten years before this patient's admission to the Woman's Hospital she was delivered normally of a full-term child. Soon after delivery incontinence of urine occurred and was continuous. Menstruation, regular and without pain; amount of flow moderate.

Bimanual examination showed a very considerable shortening of the vaginal tube and narrowing at the vault; the cervix could not be felt; a body could be clearly outlined, fixed in the pouch of Douglas. Vaginal examination in Sims' position, and with the assistance of a speculum, showed an opening on the anterior vaginal wall, about 2 cm. from the vault and to the left of the median line; through this opening urine was seen to escape. Another opening was discovered near the vault, through which the probe passed posteriorly to the depth of 2 cm. The cervix could not be located. There was a considerable amount of scar tissue in the vaginal vault. Cystoscopic examination showed the cervix to be located in the bladder, between and a little anterior to the ureteral orifices. The opening of the fistula was seen anterior to the cervix and a little to the left of the median line.

Operation (No. 1) for the Removal of the Corpus Uteri and Freeing of Adhesions, August 13, 1926.—It was deemed advisable to perform abdominal amputation of the corpus uteri before attempting to close the fistula. The abdomen was therefore opened and the retroverted, fixed corpus freed and removed; also, the right ovary was resected because of a hæmorrhagic cyst, and the omentum, firmly adherent to the anterior surface of the uterus and to the broad ligaments, was freed.

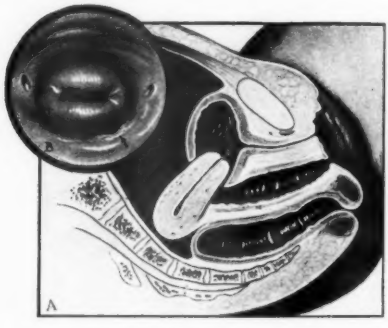

FIG. 7.

Case IV.—Cervix of retroverted uterus in bladder cavity. Inset: vesico-vaginal fistula following posterior wall of cervix.

Operation (No. 2) for the Closure of the Vesico-vaginal Fistula, August 27, 1926.—Four weeks after the removal of the corpus uteri the vesico-vaginal fistula was closed by the Sims' method. Seven silver wire sutures were used. Sims' block-tin catheter was inserted. Result—satisfactory.

Comment.—When we remember that, in an extreme degree of retroversion, the cervix assumes an extreme anterior position in close apposition to the anterior wall, it is easy to understand how, when a large opening exists in the vesico-vaginal septum following labour, and the uterus retroverts, the cervix will slip through the opening into the bladder; and, when the injury partly heals, a fistula will form along the posterior wall of the anteverted cervix. In the early days of gynæcology such cases were more often seen than they are now. This is comprehensible when we recall the fact that vaginal injuries were then common and extensive, and also that, as the result of poor obstetrics, retroversion of the uterus following labour occurred more frequently.

It is my opinion that in this case the flap-splitting method in vogue to-day would have been difficult, if not impossible to apply and, if adopted, could not have given better results.

This case was operated upon by Dr. Albert H. Aldridge, one of the junior surgeons at the Woman's Hospital, and I include it in this report not only because of its present-day rarity, but because I was in counsel with Dr. Aldridge throughout the operation and am therefore indirectly responsible for the results.

The application of any technique would here have been difficult. Success was the result of skill and exactness in the execution of the technique employed.

The reasons for the removal of the corpus uteri were to avoid possible extension of infection from the bladder, if such occurred, to remove a definite pelvic pathology, and to prevent the menstrual flow, or any other uterine flow, from passing into the bladder.

(V) *Vesico-vaginal Fistula with Complete Destruction of the Neck of the Bladder, complicated by an Embedded Broken Needle.*

This patient was delivered instrumentally, five months before entering the Woman's Hospital. Since delivery there was a continuous escape of urine. Examination showed that the vesico-vaginal septum, at the junction of the bladder and urethra, was completely severed transversely. It appeared as if the triangular ligament area below the pubic arch had been torn away from its moorings, leaving a large opening at the neck of the bladder, through which a finger could be passed into the bladder or urethra. The urethral portion of the wound on healing formed a dense scar, which extended to each ramus of the pubes, behind which the fistula, in greater part, was hidden. The tissue in the angles of the wound consisted of scar tissue and periosteum.

Operation (*No.* 1) *for Closure of the Vesico-vaginal Fistula*, July 2, 1926.—In order to facilitate vision of and approach to the fistula, the perineum was incised medianly down to the sphincter, and the dense scar tissue at the base of the urethra was severed in the direction

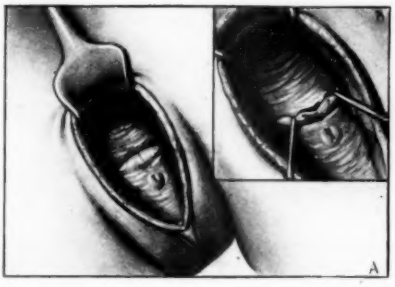

FIG. 8.

Case V.—Destruction of bladder-wall at its junction with urethra, with bridge of scar-tissue.

of the axis of the vagina. These procedures not only permitted good exposure, but facilitated denudation of the edges of the fistula. Sims' technique was then applied transversely to the axis of the vagina, eight silver wire sutures being used to approximate the denuded surfaces. Great difficulty was experienced in passing the needles through the tissue in the region of the rami of the pubes and, because of the unyielding character of the tissue, a needle broke. As the portion of the broken needle seemed completely buried in the tissue, it was deemed advisable not to attempt to remove it then, fearing that by so doing it might result in greater injury to the vesical structures, and hoping also that the needle might become encysted and not interfere with primary union. A Sims' block-tin catheter was not inserted in this case, fearing that, as the catheter would rest on the line of sutures, the tissue between might be pressed upon to the extent of endangering union. The mushroom catheter was not used because it was feared that the resistance, on removal from time to time, would disturb the apposition of the tissues at the neck of the bladder. Eight days after the operation leakage occurred. The stitches were soon removed and a small opening was found. An X-ray picture was secured which showed the needle to be in the immediate area of this opening.

Operation (*No.* 2) *for Removal of Needle and Closure of Remaining small Fistula*, August 20, 1926.—On passing a small probe through the opening it came in contact with the needle partly exposed in the bladder. A small artery clamp was then passed through the opening, the needle grasped, counter-pressure made on the anterior wall of the vagina, the point of the needle forced through into the vagina, and the needle removed. This manipulation necessarily dilated, somewhat, the fistula. The tissues about the opening were

freely denuded in the direction of the axis of the vagina, and five silver wire sutures were used. Sims' block-tin catheter was inserted. Result—satisfactory.

Comment.—There is little to be said in comment on this case. I might be criticized, with some justice, for not having proceeded at the time of the initial operation to remove the broken needle. The difficulties encountered and, as I saw them, the possibilities of causing greater injury by an effort to secure the needle, seemed to me prohibitive of the attempt. This case stands out in my mind as one in which neither the flap-splitting technique nor the transplanting of adjacent tissues was applicable.

(VI).—*Urethro-vaginal and Vesico-vaginal Fistulæ.*

Two years before this patient's admission to the Woman's Hospital, the vaginal outlet had been severely injured about the neck of the bladder and urethra during a difficult instrumental delivery. Previous to her admission four surgical attempts to close the large vesical opening were made, but without success. Examination on admission showed that the urethra and neck of the bladder had been completely torn away from the right ramus and

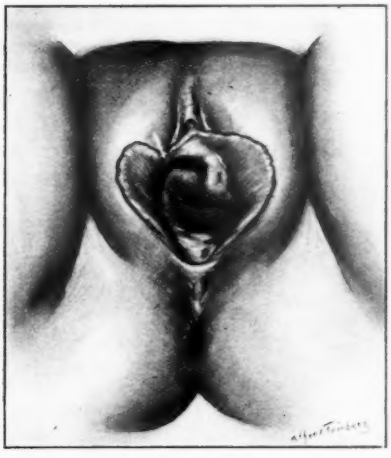

FIG. 9.

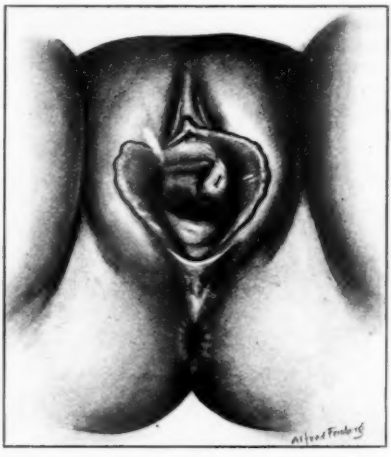

FIG. 10.

Case VI (Fig. 9).—Urethro-vaginal and vesico-vaginal fistulæ. Before operation.
Case VI (Fig. 10).—Vesico-vaginal fistula repaired—urethro-vaginal fistula to be repaired.

pubic arch and displaced to the left. A large opening existed where the neck of the bladder and base of the urethra were previously located. There existed, also, another opening through the right lateral wall of the urethra. These were in close proximity, and their directions were at a right angle to each other. The tissue of the vulva to the right of the vesical fistula showed several scars, which resulted from previous unsuccessful operative efforts made to close the vesical opening, by removing strips from the labia and transposing them to the denuded edges of the vesical opening. Immediately under the pubic arch there was but little tissue remaining, save scar and periosteum.

Operation for the Closure of the Vesico-vaginal Fistula, September 14, 1927.—The denudation about the fistula, particularly under the pubic arch, was rendered difficult because of the extensive loss of normal tissue; and also, the passing of the needles was made difficult on account of the unyielding character of this tissue. Three needles were broken during the operation, but all were easily removed. This necessitated repassing of the needles, which greatly weakened the tissue below the pubic arch. Seven silver wire sutures were used in closing the fistula. It was deemed unwise to attempt to close the urethral fistula at the same sitting. Sims' block-tin catheter was inserted. On removing the stitches the result seemed satisfactory.

Four days afterwards the patient complained of a slight moisture at the vulva. Examination did not reveal an opening, but, on injecting the bladder with an argyrol solution, oozing of the solution could be detected in the tissue between the line of union and the pubic arch, but from an opening too small to be clearly defined. Its location being above the line of union convinced me that it was a stitch opening, the result of injury to the thin tissue by the repeated passing of the needles. The patient was then kept in bed for ten days with a catheter in position. At the end of that time, there being no perceptible leakage, she was discharged in a satisfactory condition. I have had no opportunity since then to see the patient, as she lives in an adjoining State and cannot afford to visit me. I have been informed, however, that while during the night she is not disturbed, during the day, when she is on her feet, the parts become slightly moistened. She is not, however, sufficiently uncomfortable to call upon me for further operative work.

Comment.—While the mechanical result in this case cannot be classified as a complete success, yet the symptomatic results can be recorded as satisfactory to the patient. The mechanical result should not, however, be considered a failure of Sims' technique, for the original fistulous opening was closed, and the source of leakage was a minute stitch opening, due to the injured tissue through which the suture passed. This case is to some extent analogous to Case V. The severe injury to the base of the bladder was, in each instance, repaired satisfactorily, excepting for a small post-operative fistula.

In Case V the fistula occurred on the line of union because of the presence of an exposed and corroded needle. In Case VI it occurred between the line of union and the pubic arch as the result of repeated passing of the needle at this point, where the tissue was thin and weak. The repair of the small opening in Case V was done without difficulty, and in Case VI, should the closure of the minute opening be deemed desirable, it can be done with the same certainty of success.

The closure of the urethral fistula was not attempted in this case, as its direction was at a right angle to that of the vesical fistula, and to repair it would have added an unnecessary complication to the operation. If, however, the urethral operation is done, both openings can then be included in the same denudation and the repair made with assurance of success.

(VII).—*Uretero-uterine Fistula.*

(Reported in *American Journal of Obstetrics*, 1903, xlvii, 2.)

The history of this case, as furnished by the attending physician, is as follows: Second labour, March 1, 1900, was instrumental. The first ten days of convalescence were afebrile

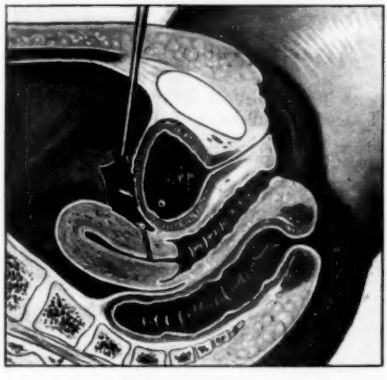

FIG. 11.

Case VII.—Uretero-uterine fistula.

and apparently normal. Then a chill occurred, followed by a sudden rise of temperature with pain in the left pelvic region. Vaginal examination showed evidence of cellulitis at the base of the left broad ligament. A considerable sloughing of tissue occurred in the vaginal vault to the left of the cervix, from which area urine escaped, and incontinence became

persistent with frequent attacks of renal colic. Between the time of this accident and the patient's admission to the Woman's Hospital, December 1, 1901, she became pregnant twice. The first pregnancy continued three months, the second seven; the latter terminated in a living child, which died of convulsions on the tenth day. Two vaginal operations were performed to relieve her incontinence, but both were unsuccessful.

Vaginal examination by me, in Sims' position, showed but little evidence of injury to the cervix, and there was practically no evidence of injury at the vault of the vagina, but from the external os there could be seen a constant stream of urine flowing into the vagina. The first problem was to determine the character of fistula, whether vesico-uterine or uretero-uterine, and if uretero-uterine which ureter was involved. Cystoscopic examination showed no vesical opening. Urine was seen flowing from the right ureteral orifice, but none from the left. A bougie was passed through the left ureteral orifice and met obstruction at about 3 cm. The diagnosis was thus reached of left uretero-uterine fistula.

Operation for Closure of Uretero-uterine Fistula and Vesical Implantation of Left Ureter, December 13, 1901.—The abdomen was opened medianly. An incision was made through the anterior surface of the left broad ligament, near to and parallel with the bladder; through this incision the two surfaces of the broad ligament were separated and the left uterine artery exposed. The ureter was discovered in the immediate region of the artery coursing towards the cervix; it was traced to the area of the internal os, where it had evidently been caught and anchored during the process of necrosis. It was tied distally and proximally, and severed. The ureter, then freed, was implanted into the posterior wall of the bladder, at the base of the broad ligament, in the following manner: A long, curved dressing forceps was passed through the urethra into the bladder until its point came in contact with the posterior wall of the bladder at the base of the broad ligament, the blades of the forceps were slightly opened and a small incision made between, through the bladder wall. The ends of the forceps were then pulled through this opening and the free end of the ureter grasped, pulled into the bladder, and sutured in position; Sims' block-tin catheter was inserted, and convalescence was uneventful. Five weeks afterwards the bladder was distended with water and a photograph taken of the orifice of the implanted ureter. This I believe to have been the first photograph, taken in America, of an implanted ureter in the bladder. Result—satisfactory.

Comment.—It is possible that two fistulæ existed originally, one a vesico-vaginal and the other a uretero-uterine, and that the vesico-vaginal fistula was cured by the second operation, but the uretero-uterine was not. The history, as given by this patient's medical attendant, states definitely that sloughing occurred in the vault of the vagina, to the left of the cervix, from which area urine escaped. At the time when I saw her the vaginal vault appeared normal. If a vesico-vaginal fistula existed and was surgically cured, the technique adopted was probably that of Sims, as the work was done when and where Sims' technique was in vogue.

There is another feature of interest in this case which might be referred to incidentally, viz., the conceptions which occurred following the establishment of the uterine fistula. In spite of the constant flow of urine through the cervical canal, the patient conceived twice, one conception continuing until the seventh month. Emmet mentions the occurrence of pregnancy in vesico-vaginal fistula cases, and Healy relates a case where conception occurred with the cervix in the bladder, and where intercourse could be accomplished only through the urethra. Hayward, also, records two cases of pregnancy during the existence of vesico-vaginal fistula. One became pregnant twice, but miscarried on both occasions; the other became pregnant once, carrying the child to eight months.

It is surprising that a surgical feat which held the attention of a large part of the surgical world for many years, and which brought to its author many national and international honours, should be to-day almost forgotten. The writings both of European and American surgeons of the present era are scant in their recognition of Sims and his work.

It is interesting to review Jellett's estimate of the techniques that have been used in the treatment of vesico-vaginal fistula since the day of Sims. They are applied according to the character of the fistula. He states that "in cases of very small fistulæ the best operation is that described by Mayo, in which the fistulous tract is dissected out and inverted into the bladder, the resulting flap being closed by sutures."

The techniques of Sanger, Dudley and Mackinrod are suggested for special conditions; the latter is considered of particular value when the anterior vaginal wall is badly scarred.

Eden and Lockyer, after discussing the varied techniques which, in their opinion, might be used successfully in closing vesico-vaginal fistulæ, conclude with the following statement: "In the presence of urinary infection, success is very difficult to achieve." This statement is entirely in accord with the scientific teachings of to-day, yet when we consider the facts, that the infected bladder was commonly associated with the vesico-vaginal fistula in the day of Sims, that Sims' technique, in his and Emmet's hands, gave results which, if ever equalled, have not been surpassed, and that these results were attained in a day of pre-aseptic surgery, it is indeed surprising that this technique is not given more consideration. These authors classify vesico-vaginal fistulæ according to their location; first, those located high in the vaginal vault; second, those located in the middle or lower third of the vagina. The first class, they think, "can best be reached by an intraperitoneal suprapubic operation," but they admit that such a procedure "is formidable owing to the difficulty in obtaining access to the parts." This comment is rational, whether the uterus has previously been removed or not. During my many years of practice I have frequently encountered vesico-vaginal fistulæ at the vault of the vagina, but have never found it necessary to resort to so radical a method. Three of the fistulæ included in the article (Cases I, II, and IV) were located at the vault of the vagina. Cases I and II might have been approached *per abdomen*; Case IV could not have been so approached without first removing the entire uterus, and, as the cervix was in the bladder and occupied the region of the trigone, an operation to remove it *per abdomen* would truly have been most formidable. I am unable to conceive of a vesico-vaginal fistula so situated as to make the abdominal route preferable to the vaginal. Such cases may exist, but my experience would, I think, direct me first to a trial of Sims' technique before adopting a far more complicated and dangerous one; and may I here again remind you that fistulæ, located in the vaginal vault with the cervix in the bladder, were met with in the days of Sims and Emmet and were treated successfully by this technique.

With regard to the second class of fistulæ, or those located in the middle or lower third of the vagina, Eden and Lockyer consider two methods applicable; first, "the edges of the fistula are simply rawed and then stitched together; second, a flap-splitting technique, where the bladder and the vaginal walls are first widely detached from one another, and then separately united by individual sets of sutures." The first method of procedure simulates that of Sims, but as, in the opinion of the author, it is not applicable to large fistulæ, and Sims' technique is applicable to both large and small, it would seem that in this first method there is wanting some essential element which I judge to be the silver-wire suture technique.

The second or flap-splitting method is considered applicable to large fistulæ located in the middle or lower third of the vagina. It is presumed that fistulæ at the neck of the bladder are here included. The flap-splitting method is applicable to a large majority of fistulæ located at the base of the bladder, and should always be chosen by those unfamiliar with the Sims technique. However, when a vesico-vaginal fistula is so extensive as to permit of the cervix entering the bladder and there becoming fixed, thus forming part of the vesico-vaginal septum (as in Case IV), we have, it would seem, a condition not suitable for the flap-splitting method. Again, when destruction of the neck of the bladder is encountered to the extent met with in Cases V and VI, splitting of the tissues of the vesico-vaginal septum is impossible, for in these cases destruction of the tissues extended anteriorly to the limits of the bladder wall, and in each instance, if splitting had been done, it would necessarily have had to be extended into the tissues of the urethra. In both instances, however, the mechanical conditions present made the extension impossible. In Case V, involving

the pubic arch and urethra, there was a band of heavy scar tissue, so extensive and formidable as to completely frustrate such an attempt. In Case VI, in addition to scar tissue about the opening, there existed a large urethro-vaginal fistula, about 1 cm. in length. As the preservation of the bridge of tissue between the vesico-vaginal and urethro-vaginal fistulæ was here essential to success in the repair of the vesico-vaginal fistula, any operation which destroyed its continuity would inevitably have met with failure.

If my opinion were solicited as to the greatest living American authority on gynæcology, I would unhesitatingly say Howard Kelly, for in him there is consummated a knowledge, both theoretical and practical, of all the divisions of the art and science of medicine which, in a broad sense, constitute gynæcology, i.e., obstetrics, urology and the study of diseases peculiar to women. In a recent issue of his "Surgical Gynecology" he devotes considerable attention to the subject of vesico-vaginal fistula, and lays down certain fundamental principles. In the midst of these principles he interjects a comment in which he states that Sims' technique for the cure of vesico-vaginal fistula was applied by Sims and his early followers under circumstances most disadvantageous, yet with almost uniform success. In so doing, Kelly admits that the technique is not only sound in principle but universal in application; he concluded, however, by saying that a knowledge of its use has virtually passed away.[1]

Half a century ago the name of Marion Sims stood foremost among American surgeons. His clinical work was known and appreciated, not only by his countrymen but also by his fellows of Europe. His book on "Uterine Surgery" was the first publication on this subject by an American, and created a great interest in the speciality throughout the medical world. Yet, within a quarter of a century after his death, his outstanding surgical feat was overshadowed by discoveries and achievements in other departments of the field of medicine which redirected the course of surgery.

During the era in which Sims lived, surgical knowledge was limited to mechanics and individual dexterity, and had not knowledge advanced in the direction of pathology and bacteriology, morbidity and mortality would have continued to be what they were in the days before Lister. Sims, however, in spite of the lack of the knowledge of asepsis or antisepsis, succeeded in combating conditions where others failed. The methods (other than that of Sims) employed to-day in curing vesico-vaginal fistula, when successful are so, not because of their superior mechanics but because of a knowledge of sterilization, as applied to material and the operative field; and had not Lister, or some other equally great benefactor, shown the way to successful combating of the causes of infection, the techniques which utilize buried sutures would not be given consideration.

Sims' great achievement was not that which is so commonly but erroneously attributed to him, namely, priority in the cure of vesico-vaginal fistula, but it was that of perfecting and popularizing a technique which was simple in principle and more nearly universally applicable than any technique devised previous to or since his time. The use of the silver-wire suture was the chief feature of this technique, and the almost uniform success attained by Sims and Emmet demonstrated that when it was correctly applied primary union became the rule and not the exception. It is of interest to observe, further, that this fact was of greater import than at first appeared, for, when the silver-wire suture technique was applied to other birth-canal injuries, the results were incomparably better than those which had previously been obtained.

1 Kelly's comment on Sims' technique is interesting: "Our forefathers, especially Sims and Emmet, succeeded under conditions of extreme awkwardness, where we to-day, with less of their special skill wrought out of an abundant experience, would fail lamentably. Like the bow of Ulysses, there is scarcely one to-day who can handle these tools or use their legerdemain in this field. However, we can congratulate ourselves in the possession of methods and devices not available in their day; short-cuts and by-paths over the rough roads to success."

Although Sims did not discover or popularize a single principle which changed the course of surgery, yet his name must always be associated with the advancement of the art and science of medicine, for he raised the operative standard to a higher plane than that on which he found it.

My hope is to awaken interest in a subject which has, seemingly, passed into history as only a stepping-stone in the progress of surgery. Sims' achievement was more than a stepping-stone; and my belief is that while other techniques can be applied under special conditions, Sims' is universally applicable, and he who has it at his command is thereby enriched. It is too valuable to be discarded, and my effort is to arouse interest in it, and to re-establish it in the armamentarium of the gynæcologist.

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Discussion.—Dr. F. J. MCCANN: I am glad to hear Dr. Bissell mention the name of Gossett. Gossett was a London surgeon, who described in *The Lancet* of November 29, 1834, an operation for vesico-vaginal fistula which was completely successful. The position (knee-elbow), the wire sutures, the mode of introducing the sutures, the mode of fixing them and the after-treatment—in short the whole operation in all its details was successfully carried out in England twenty years before Sims' operation was described. Operations for genital fistulæ were from their nature not likely to be successful in the hands of many operators, for they demanded a high degree of technical skill as well as ingenuity. Sims must have developed from his large experience considerable skill in carrying out his methods. I have, however, never regarded Sims' operation as a good operation, for it is, in my opinion, surgically inferior to the flap-splitting method, where no tissue other than scar tissue is removed and where tension is avoided by free separation of the bladder. It is important to stitch the bladder transversely with two rows of interrupted catgut and the vaginal wall (incised longitudinally) is stitched with catgut, or, if the vagina be very septic, with silkworm gut. The flap-splitting operation has long been described and probably owes its origin to Colles, of Dublin, whose name is attached to a well-known fracture, yet it is frequently referred to as Mackenrodt's, although free separation of the bladder was described and adopted long before Mackenrodt's method was published. The two essential details in the technique are the avoidance of tension on the sutures and the necessity for removing scar tissue, for if the anæmic scar tissue be brought into opposition it will fail to unite. The nicety in the adjustment of the sutures will be acquired by practice.

Again, as the President has said, the question of access is important. I have used a para-vaginal incision in cases where the vagina was contracted by scar tissue, the result of sloughing following labour, in order to get access to the fistula, which thus renders an otherwise difficult operation comparatively easy.

Dr. Bissell has not mentioned a not uncommon complication of bad cases, namely, contraction of the bladder, and even if the fistula be successfully closed, the contraction remains, the bladder only being able to hold a small quantity of urine. This condition is benefited by dilating the bladder at frequent intervals by running fluid into it. The closure of the fistula is not always followed by the "cure of the patient," for a certain amount of incontinence may persist for a considerable time, especially where there is a contracted bladder.

Some of the most troublesome cases to deal with are those where the floor of the urethra and adjacent portion of the bladder have been destroyed. In the last case of this kind I treated, the patient had had three previous operations by three different surgeons. I incised the vagina back to the cervix, split the sides of the opening and freed the bladder. The hole in the bladder was stitched from behind forwards, leaving an orifice of sufficient size in the position of the meatus. The vaginal wall was then united in the longitudinal direction, the last stitch constructing the meatal orifice. Although there may be slight incontinence at first, yet in my experience, complete control is ultimately gained—even in these cases.

Sir EWEN MACLEAN said that he had seen in the record room in the Woman's Hospital, New York, the original set of Sims' instruments to which Dr. Bissell had referred.

He (Sir Ewen) had had a case under his care, the details of which had been published in the *Journal of Obstetrics and Gynæcology*. This case illustrated one of the most difficult types of fistula, viz, the juxta-cervical or vesico-cervico-vaginal, in which there was very little of the anterior wall of the cervical canal remaining. Several previous attempts at repair had already been made before the case came under his observation. The fistula was so large and the cicatricial tissue round its margins so abundant that, despairing of success by the usual methods, he had freely denuded the edges of the fistula, the sides and posterior surface of the cervix of their superficial tissues; the cervix as a whole was turned into the bladder, and interrupted, deeply-biting, silk-worm gut sutures were inserted, shutting off all communication between bladder and vagina.

Complete healing had followed and thereafter, until the menopause, which ensued in four years' time, the patient had menstruated through the bladder. This naturally occasioned dysuria at times and some other disabilities, but the patient frequently expressed herself as well satisfied with her condition as compared with her ill-health and locally distressful state before the operation. No bladder calculus formation had occurred.

Dr. HERBERT SPENCER said he hoped that Dr. Bissell's admirable demonstration of Sims' use of silver wire in the treatment of fistulæ would appeal to the younger members of the Section. He (the speaker) had used it exclusively and with the best results during his forty years' practice.

Dr. Spencer was surprised to find no reference to a paper on fistulæ which he had read in 1925 before the American Gynæcological Society, of which both Dr. Bissell and he were Fellows. The paper was published in the fiftieth volume of that Society's *Transactions* (pp. 110, 121).

He agreed with Dr. Bissell that flap-splitting, mobilization of the bladder and abdominal section were not required in these cases. The great advantage of the use of silver wire lay in the ease with which careful adaptation of the edges of the fistula could be obtained, without slackness or tension, by twisting the sutures. For passing the sutures he thought the needle he had designed (see *Proceedings*, 1916, ix. p. 20) was simpler and better than that of Sims.

As he feared that the younger members might be misled to neglect the use of silver wire by the impressive manner in which flap-splitting and suturing with catgut were advocated by the last two speakers, he would relate an incident concerning a senior gynæcologist to a large London hospital, who was not less experienced and skilled than they, and who, some years ago had remarked to him (the speaker) that if he (Dr. Spencer) had such bad cases as he himself had at his hospital, he would not be able to cure them by the use of silver sutures and a tubular needle. Within a few weeks of that confident assertion he (Dr. Spencer) had cured at one sitting, by the use of silver-wire sutures, a case of vesico-vaginal fistula which, after unsuccessfully operating on it with catgut sutures, that gynæcologist had sent on to a distinguished genito-urinary specialist, who had also failed to cure it, and had then proposed to remove the uterus and close the vagina in a young woman who had had only one child.

Section of Psychiatry.

President—Sir MAURICE CRAIG, C.B.E., M.D.

[*November* 13, 1928.]

PRESIDENT'S ADDRESS.

A Survey of Some Conditions conducive to Mental Disturbance, with Suggestions as to their Treatment.

By Sir MAURICE CRAIG, C.B.E., M.D.

ABSTRACT.—Conditions which experience has proved conducive to mental disturbance considered.—Suggestions as to their treatment.—A weakened inhibition, rather than any positive condition, is probably the most important factor in the production of the exhaustion psycho-neuroses or psychoses. This view is supported by the prophylactic value of giving for prolonged periods small doses of bromide to hypersensitive children or to highly-strung persons exposed to stress or tropical climate, etc.—Pavlov's work on the conditioned reflexes in dogs quoted in support of the author's clinical experience: Pavlov states that bromides should not be regarded as sedatives, diminishing the excitability of the central nervous system, but as simply regulating the nervous system by strengthening the intensity of internal inhibition. This agrees with the author's clinical experience, as small doses of bromide taken regularly over a period of many years do not diminish the mental powers but in fact increase them. Question of sleeplessness considered with regard to the way in which sedatives act. Most of these do not act as so-called "sleeping draughts"; research may ultimately show that their action is to strengthen a weakened inhibition and that sleep is only a secondary benefit.—Value of sedatives before and after surgical operation. Importance of toxæmia in the production of mental disorder; insomnia often precedes a toxic process and permits it to become active. The theory of weakened inhibition explains many problems; e.g., why certain brilliant children or adults break down and why at first there is no interference with their normal mental activity which only becomes involved as sleep and other bodily functions become affected; why a toxæmia may affect the nervous system of certain people; why a breakdown may follow over-stimulation or occur with advancing years; why some persons relapse when certain treatment is discontinued; why treatment should at times be continuous, and why patients may remain in good health and full mental activity for many years under treatment. Research made from this standpoint may be of much value in the prevention and treatment of functional nervous disorder.

ABRÉGÉ.—Conditions prouvées expérimentalement comme contribuant aux désordres mentaux. Suggestions sur leur traitement. Une inhibition affaiblie, plutôt qu'une condition positive, est probablement le facteur le plus important dans la provocation de la psycho-névrose ou de la psychose de l'épuisement. Ce point de vue est justifié par la valeur prophylactique de petites doses de bromure administrées à des enfants hypersensitifs ou à des adultes excessivement nerveux, exposés au surmenage ou aux climats tropicaux, etc. . . . L'œuvre de Pavlov sur les réflexes conditionnés ces chiens est citée en appui des expériences clinicales de l'auteur. Pavlov déclare que les bromures ne sont pas à considérer comme des sédatifs qui diminuent l'excitabilité du système nerveux central, mais simplement comme régulateurs du système nerveux en fortifiant l'intensité de l'inhibition intérieure. Sa déclaration s'accorde avec l'expérience clinicale de l'auteur, puisque des petites doses de bromures prises régulièrement pendant plusieurs année ne diminuent pas les activités mentales, mais, à vrai dire, les fortifient.

Question d'insomnie considérée en rapport avec le "modus operandi" des sédatifs; la plupart de ceux-ci n'agissent pas comme des soi-disantes "potions calmantes"; des recherches pourraient éventuellement démontrer que leur action consiste en fortifiant une inhibition affaiblie et que le sommeil n'est qu'un avantage d'ordre secondaire; valeur des sédatifs avant et après une opération chirurgicale; importance de la toxémie dans la provocation de désordres mentaux; l'insomnie precède souvent un processus toxique en lui permettant de devenir actif.

La théorie de l'inhibition affaiblie explique bien des problèmes: i.e.—pourquoi certains enfants et adultes extrêmement doués s'affaissent et pourquoi, au commencement, il n'y a aucun dérangement dans leur activité mentale, laquelle n'est affectée qu'après l'atteinte du sommeil et d'autres fonctions du corps.

Pourquoi une toxémie peut affecter le système nerveux de certains subjets; pouquoi un

collapsus peut survenir après une surexcitation ou avec l'avance des années ; pourquoi certaines personnes subissent une rechute avec la suspension d'un certain traitement ; pourquoi parfois le traitement doit être continu et pourquoi les patients peuvent rester sous ce traitement pour beaucoup d'années, en bonne santé et en conservant leurs activités mentales. De ce point de vue, les recherches faites pourraient être de grande valeur dans la prévention et dans le traitement de désordres nerveux fonctionnels.

UEBERSICHT.—Betrachtung von Zuständen, die, wie durch Erfahrung bewiesen ist, zu Geistesstörungen beitragen. Vorschläge für derer Behandlung. Eine erschwächte Inhibition, eher als ein positiver Zustand, ist wahrscheinlich der wichtigste Faktor in der Hervorrufung der Erschöpfungs-Psycho-Nevrose oder Psychose. Diese Ansicht ist bestätigt durch den prophylaktischen Wert der Handhabung von Bromid, in kleinen Dosen und für eine längere Zeitdauer, an hypersensitive Kinder oder an äusserst nervöse Erwachsene, die der Spannung oder einem tropischen Klima ausgesetzt sind. Pavlov's Werk über die bedingte Reflexerscheinung in Hunde wird zur Bestätigung des Verfassers klinischen Experimente zitiert. Pavlov behauptet, Bromid soll nicht als Sedative, welches die Reizbarkeit des Haupt-Nervensystems lindert, betractet sein, sondern einfach als ein das Nervensystem durch die Kräftigung der Intensität der inneren Inhibition regulierendes Mittel. Dies stimmt mit den klinischen Erfahrungen des Verfassers ein, da kleinere Dosen, regelmsäsig über eine vieljährige Zeitdauer genommen, vermindern keineswegs die geistigen Fähigkeiten, sondern " de facto vermehren sie. Die Frage der Schlaflosigkeit, betrachtet unter Bezugnahme auf die Art und Weise der Wirkung des Sedatives. Die Meisten wirken nicht als sogenannte Schlafmittel; Forschung kann eventuell beweisen, dass ihre Wirkung in der Kräftigung einem erschwächten Inhibition besteht und dass in diesem Falle der Schlaf nur ein sekundärer Gewinn ist. Wert des Sedatives vor und nach einer chirurgischen Operation. Wichtigkeit von Toxaemie in der Hervorrufung geistiger Störungen ; die Schlaflosigkeit kommt oft einen toxischen Prozesse vor und erlaubt ihm, aktiv zu werden. Die Theorie der erschwächten Inhibition macht viele Problems klar; z. B. –weshalb gewisse begabte Kinder und Erwachsene zusammenbrechen und weshalb am Anfang keine Störung in ihrer normalen geistigen Aktivität stattfindet welche erst von Schlaflosigkeit und andere körperlichen Funktionen angegriffen wird. Warum die Toxaemie das Nerversystem gewisser Leute beeinflussen kann. Weshalb ein Zusammenbrechen nach übermässigem Antrieb oder im vorgerücktem Alter folgen kann. Weshalb Manche einen Rückfall bei Nachlassung der Behandlung erleiden. Weshalb die Behandlung zeitweise fortzusetzen ist und weshalb Patiente in guter Gesundheit und be völliger geistigen Aktivität viele Jahre unter der Behandlung bleiben können. Nachforschungen von diesem Standtpunkte aus können von grossem Wert in der Vorbeugung und Behandlung von funktionellen Nervenstörungen sein.

There is no branch of medicine that covers so wide a range as that known as Psychological Medicine ; it can be approached by many and widely diverse avenues, and the combined efforts of men working along these varied roads have greatly advanced our knowledge during the past quarter of a century. I have no new hypothesis to offer ; my contribution must be limited to clinical observations and such inferences as may appear to be justified from them. Fifteen years' residence in a mental hospital inspired within me a desire to approach psychological medicine from the preventive side, and the fascination of this has increased with time. I agree whole-heartedly with those who believe that much mental disorder, as we see it to-day, is preventible.

The influence of stresses upon the mind is as important with the normal man as with the abnormal, indeed the mental reaction in any individual may be the determining factor in recovery, whether in general disease, surgical interference, or in any other condition. The study of the mind is a study of interaction and relationship. Scientists profess to investigate the sequence of phenomena and recoil from an expression of opinion as to why they succeed each other. Years ago the aim of experimental medicine was the discovery of causes ; now it would be more accurate to speak of sequences. Certain sequences have become impressed upon my mind both by reason of their frequency and because in a given instance one can foresee the next step in the order of symptoms. If we accept as a basic principle that inherited tendencies largely determine the danger areas in any given man we

should endeavour to find what stresses, external or internal, may so reinforce this tendency as to make it operative.

For many years I have regarded variations in sensitivity, and I use the word in its widest sense, whether congenital or acquired, as one of the most important of potential factors in the production of mental disorder, and with this hypersensitivity there is frequently associated a weak inhibition or lessened control. I am aware that here I shall be confronted with critics who differentiate fundamentally between psychoneuroses and psychoses. I must honestly confess that I am afraid of labels, for it seems to me that there is a danger that by using them we may lose our perspective, which to a certain extent must be restricted by the artificial confines we have set up.

The threshold of minimal sensitivity differs in individuals; for example, some children from their earliest years react unduly to weak stimuli, whereas in others extreme sensitivity arises during the passage of years. Now if this undue sensitivity occurs in a child or man with a neuropathic inheritance it is common to note other changes which, having regard to their frequency, may be looked upon as sequences to the primary sensitivity. These changes may be on either the physical or the mental side; if on the latter, in that the next disturbance will be one of the emotions, bodily changes must evitably result. Thus disturbed emotion must be regarded as a common sequence to hyper-sensitivity, and if this is true it is clear how important it is to treat the hyper-sensitivity before the emotions have become involved. To me this consideration is one of the most important, for it is one of the earliest to be dealt with in the problem of prevention of mental disorder. For over twenty years I have acted on this assumption, and, being of opinion that by controlling the initial hypersensitivity further trouble is prevented, I have made it a practice to keep certain types of children continuously on small doses of bromide. To children under four years of age I give 2½ gr. of potassium bromide and to children over four twice that amount, once a day. I have found that by following this course children who in some cases owing to their over-reaction to stimuli may be almost uneducable become quiet and teachable, and others less hypersensitive are helped in varying ways. It is of interest that children who had been taking this small dose of bromide regularly for long periods and who had ceased to take it, slowly regressed during the following twelve or eighteen months and in some instances the excitability which again became noticeable continued to increase until epileptic fits occurred. This appears to indicate that a small dose of bromide given daily and continuously to certain types of individuals acts as a prophylactic to the development of epilepsy. Further, acting upon the same line, that of controlling the initial condition of hypersensitivity, I have for many years advised certain nervous types of men and women on going to reside in the tropics to take 5 gr. of potassium bromide once every day during the time they are there; and many have told me on their return that they had kept in better health and had experienced less nervous irritability with its concomitant symptoms than in former times when they had taken no bromide. Again, for many years I have prescribed for certain types of active men and women 5 gr. of bromide once daily for varying periods up to five and twenty years with equally beneficial results.

Now that which I have long observed clinically seems about to be confirmed and explained by that distinguished physiologist, Pavlov. In his great work on the conditioning of instincts in dogs he refers to the different types of nervous system that he encountered in the dogs which he used for his experimental work. He found that in excitable dogs the inhibitory function of the cortical elements became extremely weakened, but if bromides were given certain important effects resulted. He adds: "It must be noted that as a result of the administration of bromides there was no diminution in the magnitude of the positive reflexes, on the contrary they were extremely constant. According to these and all previous

experiments bromides should not be regarded as sedatives diminishing the excitability of the central nervous system; they simply regulate the activity of the nervous system by strengthening the intensity of internal inhibition." My clinical experience coincides with Pavlov's observation, for when a small dose of bromide is taken daily, even for many years, there is no dulling of the mental powers, indeed the reverse is noticeable, that the mental capacity is usually increased, no doubt due to the prevention of wastage. I have found that the small dose of bromide long continued is of infinitely greater value than the larger doses administered for a shorter period. Indeed, bromide in the larger doses can be extremely toxic and its beneficial effects are not infrequently lessened, or even may be destroyed, by prescribing it in this way. If, as Pavlov suggests, bromide strengthens a weak inhibition, it is clear that the smaller the dose that will produce the desired effect the better. Is it possible then that much mental disorder, as we know it to-day, is due to the weakening of the inhibition and not to any positive condition? This view of lowered resistance makes inherited tendencies more easy to understand. It is true that there are certain—may I call them malignant?—types of mind disturbance which run a definite course and the syndrome of whose symptoms renders them capable of being classed under recognized names, but opposed to these stands a much larger group, embracing the exhaustion and intoxication types, which are clearly very different from the "malign" group, and which are much more accessible to treatment.

If once the inhibitory function of the cortical elements has became markedly weakened, the next steps on the downward path are almost constant on the mental side. The emotions become involved and the disturbance of these in turn gives rise to pre-occupation, false reasoning and in course of time to failure of adaptation to environment. All this cannot take place without equally important change arising on the physical side. The functions of the various systems of the body are so closely inter-related that one of these cannot become involved without disturbance in another. Indeed, in the course of time the gastro-intestinal, the cardio-vascular, the genito-urinary may all become affected and the defensive mechanism of the vegetative nervous system may give rise to a disturbed balance in the functioning of the endocrine glands, and with all this a fall in the metabolic rate and a change in the body weight takes place. If the physician sees the patient for the first time after these have progressed for a number of weeks or months, we may find it difficult to decide what was the initial cause of the mischief. Some turn at once to the variation in the functioning of the endocrine glands. Physiologists have given warnings against this conclusion being accepted too rapidly, and my experience certainly favours their sceptical attitude. Although it is no doubt true in certain cases that the functioning of an endocrine gland may be so disordered as to call for treatment, year by year I become more convinced that the changes in the vegetative nervous system are usually secondary, though one must admit that when once established they may have an important bearing on the illness. To discover the initial cause, one should follow the only safe rule in diagnosis and make a careful inquiry into the medical history of the patient in addition to the examination of his condition, and this necessitates accurate and careful case-taking. If we do this, and if we have an opportunity of seeing patients in the earliest stages of their illness and control tests are made, it is often found that the endocrine gland changes show themselves for the first time in later tests. Further, it must be noted that if steps are taken to correct over- or under-activity in these glands, and it is not followed by improvement in the mental disorder, whatever this may be, this suggests that both the endocrine change and the mental disturbance may be due to a common cause. It would be conceded by most physicians that after an illness has progressed for a certain time diversity of opinion as to its nature becomes increasingly possible. The psychologically-minded man turns towards mind analysis or investigation; he traces fears to an origin which satisfies him; he may criticize the physician who looks to

physical causes and point to the number of relapses that he has seen follow this man's methods. But relapses occur in the practice of every physician. Experience has convinced me that the would-be pure psychologist cannot permit himself to ignore the physical aspect in a case any more than the man who leans towards the somatic aspect of disease should neglect the importance of the psychical. A patient throughout his life may have suffered from many fears and, whereas each fear may be based on something, the important matter is not the phobia, but the condition which permitted it to become effective, for it is this alone which explains the multiplicity of phobias from which it is possible for an individual to suffer. Much may be done to localize an illness by pointing out to a patient the dangers of false reasoning regarding his symptoms. Indeed, much benefit may accrue from foretelling to the patient the course that his reasoning will take him; or if he has already a distorted outlook, much good may result, both in the treatment of the present difficulties and in the prevention of their recurrence, from going back as far as possible and pointing out how the error of reasoning arose and how it brought about a failure of adaptation to environment.

Sleeplessness is another symptom the importance of which has become more and more impressed upon me; indeed, for over fifteen years I have given it priority over all other symptoms. There are some who are not satisfied that it is as important as I consider it, and others who disregard it if one can judge by the indifference with which they treat it. It is at times argued that sleeplessness in some individuals has no apparent bad effect, but to me the evidence adduced in support of this view is not convincing and does not really touch the clinical aspect of the subject, but we treat different individuals and it is for the physician to discern who are most likely to be seriously affected by insomnia. To quote experiments, such as those of Kleitman, to show that a number of persons have voluntarily deprived themselves of any consecutive hours of sleep with no ill effects, is no answer to the question before us, as these persons were all healthy and capable of adaptation to stress, and even here there is evidence of exhaustion, as McDougall and Smith found that, although voluntary deprivation of sleep for the time increased mental efficiency, when these subjects returned to sleeping normally, they had a period of diminished efficiency which lasted for at least fourteen days.

Again it is argued that it is really the psychical effect of not sleeping which is important and that if people did not worry about it no ill effect would result. Up to a point this no doubt is true, for the psychical effect of not sleeping does play a part with some persons, but it is by reinforcing something else. Animals deteriorate and die when deprived of sleep, and there is no rationalizing here. We are told that it is dangerous to argue from animals to man, but is it so in this matter? Deprivation of sleep causes puppies to die quickly and the younger the animal the more rapid its death, and it is equally true that young children who do not sleep deteriorate more rapidly than adults. With man, provided he is not feeble-minded, whatever the condition the psychical aspect must always have its place, but do not let us regard this as necessarily or even commonly the paramount factor. False reasoning should always be corrected or prevented, and, whatever methods are employed to remedy a condition, the physician or surgeon should never fail to deal with the psychical aspect.

To turn to the other side, the evidence which goes to emphasize the importance of sleep. Physiologists tell us that sleep is the only process which restores fatigued cells whether in the brain or in any other organ. Experiments show that animals kept continuously awake rapidly become exhausted and die. As there must be every gradation from health to dissolution, the nerve cells must suffer, even if the clinical evidence of this is not manifest. Professor Weygandt in his address at the British Medical Association at Cardiff this summer, stated that "in speaking of exhaustion we must distinguish between bodily exhaustion (fatigue) and lack of sleep and lack

of nourishment. Bodily strain has a more harmful effect on the heart than on the nervous system. Lack of sleep can, of course, cause disorder in the normal mind... Mental qualities declined very quickly during a sleepless night." Also "the brain of animals killed by hunger experiments is much less affected than the brain of those dying from lack of sleep." As I have already stated, the absence of sleep acts with greater force in the young than in the old and this again varies with individuals.

Before turning to the therapeutic aspect of the subject I must refer again to Pavlov's observations. He states that he found sleep became abolished under certain conditions and when experimenting with certain types of dogs, that broadly speaking all the dogs he employed could be divided into two groups: (1) those with an excessive or moderate tendency to excitation, and (2) those with an excessive or moderate tendency to inhibition. Indeed his experiments provided him with very clear and definite illustrations of the essential differences between the types of nervous system in certain dogs. In one the inhibitory function of the cortical elements became extremely weakened, whereas in another the excitation of the corresponding cells became affected; two quite different types of neurosis were produced, but in both cases they were persistent, prolonged, and, even after a break in the experiment, showed no tendency to improve. On observing this he decided to try bromides in the case of the dog whose inhibitions were weak. With this type of dog recovery was rapid, whereas with the other variety there was no improvement from the administration of bromides nor from any other therapeutic measures which were tried. But this latter type of animal, when left alone for a length of time, was found to recover spontaneously and completely. These experiments are extremely suggestive, and they may, when further investigations have been made, help to explain that there are different types of nervous instability. We know how little use hypnotic drugs are in the treatment of certain maniacal states, and how at times they seem almost to increase the excitement, and we know also that patients suffering from these states, if left alone, spontaneously recover. But at the moment these latter are not the type with which I am most concerned since we can do so much more for the former, and it is in regard to these that Pavlov's work confirms clinical experience as I have found it, that sleeplessness in these types is quickly corrected by suitable therapeutic methods, and more especially if the treatment is begun early. Opinions still vary on the advisability of prescribing sedatives; some recommend their administration only as a final resort, and even then advise that they should be withdrawn at the earliest moment. This is largely based on the fear that the patient will become a drug addict. I presume that we should all agree that morphia, heroin or opium should never be given for ordinary sleeplessness, indeed in many instances they not only fail to induce sleep but bring about a condition of excitement. These drugs produce a pleasurable sensation and therefore tend to addiction, whereas with the ordinary hypnotics such pleasurable sensations do not occur. When we consider the number of persons for whom sedatives are prescribed, we must conclude that addiction is an exceedingly rare sequel. In my experience there are two types of addict: the degenerate who takes any drug he can obtain, including alcohol, the opium group, cocaine or hypnotics—this type is not common; and those who have become sleepless and from whom hypnotics have been withheld until the patient reached a state of terror that he was either going insane or that some terrible fate awaited him, and when finally the insomnia was relieved by drugs he feared to give them up lest he should again undergo similar terrifying experiences. Therefore the physician who withholds hypnotics may bring about the addiction which he dreads. Further I have known patients who from being practically teetotal became addicted to alcohol from the time they became sleepless and were unable to obtain relief from the treatment prescribed by their medical adviser. It is not always clear what is meant by "addict." It certainly should not apply to a person who under medical advice takes a sedative every night and who thus preserves his sleep, and in consequence

enjoys good health and full mental activity, and who could at any time stop taking that sedative. Yet to judge from statements by some medical men these patients would be regarded by them as addicts. Such a use of language is a travesty and is certainly not applied to other potions, such as alcohol which for some reason seems to enjoy a great freedom from reproach. The term "sleeping draught" is another which is used loosely and in most instances incorrectly. I have frequently drawn attention to this mistaken way of regarding sedatives or hypnotics. Most of them do not produce sleep in the strict sense of the word. Amylene hydrate and paraldehyde are about the only sleeping draughts if one is going to use this term, for whatever time of day or night these drugs are given the majority of persons will sleep. But this is far from being the case, for example, with the barbitone group when given in therapeutic doses; indeed if they are administered by day the patient has no desire to sleep. Knowing this, it is unscientific for medical men to use an inaccurate term. To do so has a further disadvantage, as it implies that sleep which follows the administration of any sedative is artificial sleep which it certainly is not, for the reason above stated. The physiological experiments made by Pavlov have already become very suggestive as to what happens when sedatives or hypnotics are taken, and further investigations may prove that the action of many of them is not sedative in the sense of diminishing the excitability of the central nervous system, but that they act by strengthening a weakened inhibition and that sleep is only secondary. This would accord with my clinical experience and may explain why hypnotics are frequently of little value in certain states of excitement.

In support of the view that the barbitone group may act by protecting a weakened inhibition, it is of interest to note that patients who are given a dose of medinal in the morning will not infrequently tell you that they have had a quiet day and slept well at night, and others, who do not always sleep at night after a dose of medinal at bedtime, will report that, although they did not sleep, they rested quietly and when lying awake did not become anxious or worried. This brings me to another point which would once more be in accord with the view that the prolonged use of the barbitone group is invaluable in preserving the mental stability and the mental values of certain types of persons. Patients who show evidence of exhaustion, with defective sleep, due either to advancing years or to prolonged over-stimulation of the nervous system, or to the lowering of their nervous resistance by illness or surgical operation, may be benefited physically and have their mental stability strengthened, so that their activity of mind may remain on a high level for many years, by taking a small dose of dial or medinal every night. I have had the metabolic rate tested in patients who have been taking medinal for many years and it is always a high normal, therefore there is no evidence that the taking of the barbitone group over long periods is damaging to the physical health. This is borne out by Dr. Edwin Goodall, a very careful observer, who states in his annual report of Cardiff Mental Hospital, two years ago, when writing about the administration of sodium luminal, that "it had no deleterious effect on the physical and mental condition of patients as have bromides and allied drugs." Amongst the many patients whom I have known to have taken medinal or dial for many years continuously, and who, by so doing, have been able to live a full and energetic life, are representatives of every form of mental activity. My attitude towards sedatives has been a progressive one: in my early professional days I used them as little as possible and withdrew them at the earliest possible moment; later, when my work became more especially in the field of early treatment, I found that sedatives were as valuable, if not more so, in the prevention of sleeplessness and mental disorder as in the treatment of them, but even then I limited the time that a patient took hypnotics and seldom advised a man to return to work whilst still requiring them, but still the results were unsatisfactory; to stop them when

a patient has been sleeping well for a short time almost always is followed by a return of the sleeplessness; in other words, one permits the condition that gave rise to the insomnia to become re-established; therefore it was with a view to preventing this that I resorted to what has proved the most efficient method, that of giving sedatives over long periods: neither have I found that there is any difficulty in withdrawing them when so desired; further, I give them to protect the nervous system when unstable persons are exposed to special stress; thus, for many years I have given five grains of medinal every night at bedtime to certain unstable types of women during the last three or four months of pregnancy, and for the nine months after the birth, and I have never known a woman have a mental breakdown when following this treatment. Here again the medinal strengthens a weakened inhibition and in this way protects the nervous system. As to the effectiveness of the protection I have not the slightest doubt, owing to the number of years and the frequency with which I have used it.

There is another very different class of case in which I have been interested for many years, as it does not seem clear why certain patients have recurrent attacks, or why these attacks occur in cycles. The disorder to which I refer is that known as maniacal depressive mental disturbance. Seven years ago I decided to try keeping this type of patient on five grains of medinal daily throughout the period that they were well, and I have four patients who have been treated in this way, two of whom have not broken down in the slightest degree, although for several years before they had broken down regularly and very badly, while the other two, though they have had phases of mild excitement and depression, have had no attack comparable to what they had before taking the medinal. Four cases are too few to draw any inference from, but I give them for what they are worth, and as a suggestion to anyone who would care to investigate this line of preventive treatment further.

It is now recognized that sleeplessness in children is a far more serious symptom than many medical men appreciate. The deleterious effects may be either physical or mental; but whichever side manifests itself first, if the sleeplessness persists for any length of time, there will be both mental and physical deterioration. For younger children I have found five grains of potassium bromide or, if necessary, five grains of chloral hydrate with five grains of potassium bromide every night for a prolonged period, maybe for several years, effect excellent results, while for children over twelve, if chloral is not effective, I have found that one tablet of dial or five grains of medinal is very helpful. Children so treated improve both mentally and physically, and, if treatment is continued for eighteen months or two years, they reach a very high standard of health, but when the medinal or dial is stopped, I continue with one small dose of potassium bromide daily. Some may feel that this prolonged treatment is unnecessary, but my experience is that a mental breakdown, even a short one, in early life is a serious matter, as many of these children remain unstable and tend to have further attacks when between sixteen and twenty years of age and again during later years. I found that a shortened treatment did not prevent these relapses, whereas a prolonged treatment did, and that a longer treatment brought about a very much higher level of physical health and in consequence a greater power of resistance to stresses. Here again Pavlov's investigations would seem to explain why some young persons tend to break down, and why the treatment I have found helpful produces good results.

Now let me turn to another group of cases, patients who have undergone a surgical operation. Some surgeons find there is very little mental disturbance associated with operative treatment, whereas others will tell you that minor mental disturbances are not infrequent. My experience would go strongly to confirm the observation of the latter. The main reasons for this divergence of opinion are that the emotional unrest and the sleeplessness just before or immediately after an

operation may be slight and easily overlooked and the more marked symptoms usually do not arise until the patient is out of the hands of the surgeon. But to overlook any early symptom is an error of no small importance, as the deterioration once started tends to increase. Every year one sees a number of patients who state that they have never slept properly since an operation some months previously. Whereas in my experience, if they had been treated at once and their sleeplessness relieved, the insomnia would have been of very short duration. It seems almost a commonplace to suggest that a person with a keen imagination must be disturbed emotionally from the time that the necessity for operative interference has been made known to him, and his sleep tends to be defective in quality or quantity. But this is not all: the operation itself, including the anæsthetic, may be very upsetting to some persons and, even apart from actual pain, some persons cannot sleep the nights following the operation. Surgeons tend to think only of morphia for the relief of sleeplessness and, when this quite properly has to be stopped, nothing further is usually done except perhaps to give bromide. On the other hand, there are surgeons and anæsthetists who appreciate the importance of emotional disturbance in patients and the wisdom of obtaining for them proper sleep at this time. Mr. E. S. Rowbotham tells me that he gives paraldehyde per rectum after an operation and before the patient is removed from the operating table; the dosage that he administers is about half a drachm of paraldehyde per stone weight of the patient. In the case of very apprehensive persons he gives per rectum one drachm of paraldehyde per stone weight about one hour before the operation in addition to small doses of morphia, which ensures the patients sleeping so soundly that they are often unaware of being put under the anæsthetic. The value of this is very great, and I have known patients who have feared an operation tell me that they experienced no worries or inconvenience of any kind and that their sleep had remained good throughout. As to the value of paraldehyde following an operation I am convinced. One of the most dramatic occasions on which I have seen it used was when I was called late at night to see a man aged eighty-two who had had his prostate removed three days previously, since when he had had no sleep. He was unconscious and apparently moribund. An ounce of paraldehyde in mucilage was administered per rectum. Next morning I heard that he had slept all night and I found him sitting up reading a newspaper. Since then Mr. Frank Kidd tells me "that for some years he has made it a practice to administer paraldehyde per rectum to most of his patients as soon as they are taken back to bed after leaving the operating table. He finds that the paraldehyde acts better in women than in men. As soon as the patient rouses and complains of pain an injection of morphia is given. The patient then usually passes into a deep sleep, which lasts for many hours. When the patient wakes all severe pain attendant on the operation has usually disappeared and the patient is thus saved the mental trauma due to suffering severe pain as the result of an operation." Mr. Gay French tells me that he also gives paraldehyde after an operation before the patient leaves the operating table, and that he does this even with children who have had their tonsils or adenoids removed, while with adults he uses paraldehyde both before and after operation. I have no doubt that there are other surgeons and anæsthetists following the same method. Apart from the value of giving paraldehyde immediately before and after an operation, I have long made a practice of giving medinal, dial or allonal according to which best suits the individual patient. These can be given for some days before and for several weeks after an operation. The convalescence will be more rapid and complete when this treatment is carried out if the patient belongs in the slightest degree to the nervous or unstable type. Some months ago a surgeon told me that he himself had had an operation and could not sleep after it, so he prescribed sodium barbitone for himself and after taking it his sleep quickly became re-established and he made a rapid recovery, and he tells me that he has since made a practice of giving sedatives after operation and has noticed the beneficial effect on his patients.

To sum up this matter, I am of opinion that most sedatives are not "sleeping draughts," but that they act by bringing about a condition conducive to sleep and, what is even more important, they protect the nerve cells from changes which will in time take place if sleeplessness is severe or prolonged. Crile's work supports this, for he states that the use of certain drugs prevents the changes in the nerve cells, even when the sleep which would have resulted from giving these agents has been prevented.

Further, I believe that if medical men prescribed effective sedatives for sleeplessness, there would be less taking of drugs by patients on their own initiative. My experience is that patients do not tend to go on taking drugs after the sleeplessness is fully corrected. When to take hypnotics and when to stop them is a medical matter, but it is incumbent upon medical men to see that everything is done to relieve insomnia and to do this quickly, for, if begun early, small doses are effective and the disability rapidly passes away, while to wait usually means greater deterioration of mind and body, and in consequence a more difficult problem to contend with. The writer of the leading article on sleep and its disorders in the *British Medical Journal* for August 11, 1928, aptly and correctly sums up the clinical position when he says: "It need hardly be said, indeed, that we do not treat insomnia but an individual who cannot sleep," that is to say, one must keep in mind the inherited tendencies and the life history of the man, together with the disability; and the inheritance and life history may be of infinite importance not by causing one to discount symptoms because the patient has always been nervous and unstable, but rather by forewarning one to bear in mind that this instability will tend to accentuate the condition and may determine the access of other symptoms which would not arise were the man nervously stable.

To turn to another matter which has been attracting increasing attention throughout my professional life: the rôle of toxæmia or chronic sepsis in nervous and mental disturbances. For many years I have been convinced of their importance and this conviction increases with years. It is no answer to say that many persons are infected with this or that poisoning and yet evince no evidence of mental change. That delightful book by Sir Humphry Rolleston, "Idiosyncrasies," gives much food for reflection. I am certain that almost anything can act in a toxic manner provided the nervous system is sensitive, or in other words has an idiosyncrasy towards it. I reported a case of T.N.T. poisoning which produced acute insanity in a man after twelve days, and I have known men to become acutely delirious and confused within twenty-four hours of partaking of mushrooms and to have remained so for several weeks. Most of us have seen similar effects from pus in the antrum or in some other locality. A few months ago I saw a business man who became muddled on returning home one evening and this increased during the following few days. Tests indicated streptococcal and staphylococcal infection, and in the subsequent search for a focal area of infection, on transillumination the left antrum gave a shadow. The insertion of a needle produced some foul pus. Three days after the antrum had been washed out the five male nurses who had been necessary on account of the delirious condition of the patient were replaced by two female nurses, and the man made an uninterrupted recovery within six weeks. Similar results have followed the evacuation of pus in the thorax or abdomen. The diagnosis is comparatively easy when the syndrome of mental symptoms is characteristic of an acute toxæmia, but the difficulty arises when the symptoms and the grouping of them are less typical of this condition. During recent years there has been overwhelming evidence as to the toxic effects upon the nervous system of the organism which gives rise to the disease known as encephalitis lethargica. Children and adults who have suffered from this disease may develop a Parkinsonian syndrome or they may exhibit definite mental changes which are both crippling and permanent. The importance of oral sepsis is now proved, or at least would be generally accepted, as an important factor in the causation

of mental disorder in later life. Its effect upon some people is made manifest by the swelling of joints or neuritic pains and in other numerous ways. But in another group of patients the mental activities are affected and the mind becomes confused. That this can be brought about by the presence of a poison such as oral sepsis is proved, for as the infected teeth are extracted the mind may clear and the abnormal mental symptoms rapidly disappear. On the other hand against this it may be argued that after the removal of the teeth many mentally disordered persons do not recover and in some cases may become worse; but this may happen either because the septic teeth have been removed in too great numbers at a time, or, where the mouth is exceptionally toxic, the patient has not been in the first place desensitized by a vaccine. Further, that some patients do not recover after extraction of teeth should not be accepted as conclusive evidence that oral sepsis is not the cause of mind disorder in a given case, as the condition may have persisted for a long time and have led to permanent damage, or there may be an infection elsewhere. Again, it must always be borne in mind that not every person reacts to a poison in this way but only those who have a special idiosyncrasy to that specific poison. I am satisfied that the early recognition and diagnosis of a toxic condition is important, and the longer the condition lasts the more difficult and the less effective becomes any remedial treatment. It is for this reason that medical men should devote more time and study to mind reactions, especially the minor mental disturbances, for in certain predisposed persons these may be the earliest indication of physical disease due to some infection, and in some instances the failure to appreciate the significance of these changes may permit the disease to reach a stage from which the patient may never recover. I have long been impressed by the frequency with which insomnia is associated with toxæmia, and in many cases the former occurs first and seems to permit of the latter becoming active, in other words, a toxic exhaustion psychosis may develop as the result of a vicious circle of biological events set up by sleeplessness. Further, I am satisfied in my mind that where there is an obvious toxæmia it is possible in many cases to prevent serious mental symptoms from developing by treating the insomnia at once, and in so doing one follows the ordinary canon of medical practice which requires the physician or surgeon to localise the disease as rapidly as possible.

When mental and physical disorder has persisted for some time it becomes increasingly difficult to determine the order in the sequence of events as to which was the first to develop. The problem which confronts the physician is made manifest when one reads so instructive an article as that written by Dr. R. D. Gillespie on "The Clinical Differentiation of Psychogenic and Physiogenic Disorder" in a recent issue of *Brain*. I have here emphasized the physical aspects and many may feel that I have overstated my case. With the advent of the "New Psychology" there was for some years a real danger of over-estimating the psychical side, but this seems to be passing away and a more even balance between the various schools of thought is becoming noticeable. I think it will be generally conceded that each person has his own way of reacting to stress, whether external or internal. This is true both of mind and of body. Following stress or with lowered resistance as a result of fatigue or illness, one finds on each occasion when such stress occurs that some persons complain of pain in the joints, or of digestive or intestinal disturbance, more especially if from early life or owing to surgical operation, accident or disease, such areas have been rendered abnormally sensitive; every time the order of regression is the same, and so it may be with other persons that the regression shows itself in variations of the emotions or mental activities. To appreciate this is important from another standpoint than diagnosis, for as the symptoms may be subjectively disturbing to the patient, their medical significance must be explained to him, last he should attach undue importance to them. Much mental disorder is due to false reasoning or rationalizing, and at times this results from a medical man's

not explaining to the patient the true medical condition and thus permitting him to argue it out for himself, and on other occasions it results from a medical man's not appreciating the morbid sensibility of his patient who in consequence may have wrongly construed a remark made to him. At times it is a very great relief to a patient to know that his mental disturbance is secondary to a toxæmia which it is possible to remove. Some would argue otherwise and say that the person's attitude of mind and mode of meeting the stress ensue from a psychic trauma or a wrong way of bringing-up in early life. This no doubt is true of some cases, but if, as I suggest, the condition is due to a weakening inhibition, it is unnecessary and indeed may be harmful, to spend months in an attempt to retrace the workings of the subconscious in the hope of finally reaching the original cause. I do not wish to minimize or disregard the psychological standpoint, for this has its place, but experience has brought a genuine doubt into my mind whether there is not a real tendency at times to make too much of the psychological aspect, or at the least to use unnecessarily abstruse language in describing or accounting for straightforward and commonplace events. I am certain that psychological medicine would gain and not suffer from making the language we use more simple. We are anxious to make our knowledge understandable to the medical profession at large and to laymen and we shall not do this by the constant coining of new words which with the advance of knowledge have to be given new meanings.

In an address such as this one must select and can only deal with one aspect of a subject, and in that clinical experience has year by year been driving me back into general medicine, I have dealt with the subject from the medical side, because it is the way in which experience has led me to think that we can obtain for the patient the greatest benefit. Those who are seeking to prevent disorder of mind, whether minor or major, must seek out, and wherever possible, correct conditions which from clinical experience would appear to be fundamental in bringing about these changes. The theory of a weakened inhibition explains so much: it explains the influence of inheritance in many ways: it explains why certain brilliant children or adults break down, and why at first there is no interference with their normal mental activity, which only becomes involved as sleep and other functions of the body become affected; it explains why a toxæmia may affect the nervous system of certain individuals, or why a breakdown may occur after surgical operation; it suggests why some persons break down from over-stimulation or with advancing years; it indicates to what end our treatment should be directed, and why this should at times be continuous, and how under these conditions persons may remain for many years in good health and full mental activity; it supplies a reasonable explanation why some persons relapse when certain treatment is discontinued. On the other hand, it does not remove from the physician the obligation to explain to the patient the manner in which the mind may become involved by false reasoning. The theory of a gradually weakening inhibition is the one which appears to me to fit in best with my professional experience both in clinical observation and therapeutic treatment; indeed, I verily believe that research from this standpoint would be the means of our acquiring much knowledge of value in the prevention and treatment of what, for want of a better term, we speak of as functional nervous disorder.

Section of Urology.

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Movable Kidney.

By A. JURASZ, M.D.

Professor of Clinical Surgery at the University of Poznań (Poland).

SINCE the first clinical study of movable kidney by Baillie in 1825 the general opinion has been that movable kidney must be looked upon as a mechanical problem finding pathological expression in a kinking of the ureter, a torsion of the kidney or a pressure upon the adjacent organs, with all the well-known sequelæ resulting from such states. The perfection of urological methods of examination and the ever-growing recognition of the necessity to consider and elucidate existing pathological symptoms and physical signs from the point of view of a disturbed physiological function, has taught us, however, that the mechanical theory of movable kidney can only be accepted in part explanation of the disorders connected with it, and that quite a number of disturbances must be differently interpreted. This fact admitted, it seems to me that the clinical significance of movable kidney deserves the chief interest in this item.

It is generally supposed that the diagnosis of movable kidney is easy; but my experience is that it can be one of the most difficult; and I think I am not far wrong in stating that many of the cases of movable kidney are not recognized at all for a long period, during which time they are treated on a wrong diagnostic basis. To illustrate this let me mention that amongst the fifty-eight cases operated on by me, six were previously treated during a period of from one to three years for appendicitis, seven during a period of from two to five years for gall-bladder disease, two during six years for gastric ulcer. Four were operated on for appendicitis before coming under my observation, in two laparotomy was performed for cholecystitis where only adhesions in the gall-bladder region were found and dissected free, the patients in all these six cases finding no relief from their suffering. Most of the others were sent to the clinic for observation for obscure disturbance. Fifteen cases were directed to me and operated on by myself upon a wrong diagnosis, movable kidney being recognized only during operation.

To my mind the reason of such errors in diagnosis is due to the fact that very often in cases of obscure internal disorders the attention is only drawn to a nephroptosis when all the other possible diseases have, after a thorough examination, been excluded. Another reason for this observation seems to me that the undue mobility of the kidney does not always, as accepted, associate with the asthenic constitutional type of the patient, who can show good muscular development and tonus and a large fatty layer in the parieties. In such cases the abdominal palpation of a relaxed kidney is often almost impossible. Still, to my mind, the greatest difficulty in obtaining the right diagnosis lies in the fact that movable kidney does not produce such a regular and typical clinical picture of characteristic symptoms and physical signs, as to be pathognomonic of this disturbance, and further, that the degree of displacement is by no means always in accordance with the severity of these disorders. We all know that sometimes in a much displaced and movable organ there are no symptoms whatever, whereas in other cases, where the kidney is detected only just below the edge of the ribs, we find a most marked, severe complex of general and local symptoms, which Potel and Cordier named "Cachexie nephroptosique." Apart from these extremes, a great variety of symptoms can be present. In my experience the majority of cases adopt the symptomatology of a distinct abdominal disease, while renal symptoms are absent or insignificant. Therefrom results the erroneous diagnosis of gastric or duodenal ulcer, of gall-bladder

inflammation and gall-stone attacks, of common-duct obstructions, of appendicitis and colitis. A very impressive example of this kind was a case of mine, when a famous physician asked me to operate upon his daughter, whom he had unsuccessfully treated for years for appendicitis and gall-stone attacks with occasional jaundice. Absolute lack of appetite, marked emaciation, jaundice, though not very intense, pains in the gall-bladder and appendix region, and bile-stained urine were the symptoms, when the patient came to me. The operation evidenced no disease whatever of the gall-bladder, or any other disease of the abdominal cavity. Only further exploration made it clear that the reason of the troubles must have been the right kidney, which could be felt displaced downwards behind the ascending colon. After nephropexy had been performed—it is now three years ago—the patient kept in excellent health without recurrence of any of the previous troubles and gained in weight about 20 kg. A case like this, in which in addition to my own observation we have also those of the patient's father, who did not previously acknowledge nephropexy as treatment of nephroptosis, may serve as a characteristic example of the occasional difficulty of the diagnosis and of the part which must be attributed to movable kidney in severe troubles. Similar histories of unrevealed mobility of the kidney can be traced in many other of my cases.

With regard to the coincidence of symptoms of abdominal disease with a state of movable kidney, the question must be put, whether such an abdominal disease is not present in a certain number of cases and responsible for the respective symptoms. As this has been found to be the case, the further question arises, whether movable kidney is concerned with the genesis of that disease. If no disease is detected, and yet its symptoms are present, we must ask ourselves if, and how, movable kidney can be responsible for these symptoms. I shall try to answer the latter question first. In doing so I must admit that I am moving on a ground still somewhat hypothetical, where many questions remain controversial and have not yet been settled with any certainty by sufficiently proved physiological investigations; on the other hand, so much experience has already been gained along this line by experimental physiology, and so many respective clinical facts collected, that we can take them as our basis without risking the reproach of making hypotheses into practical questions. I have in mind the vast field of functional disorders evoked by derangements of the involuntary nervous system, in which so much useful work has already been done by Head, Mackenzie, Langley, Gaskell, Bayliss and Starling, v. Bergmann, Westphal, Fraser, Pribram, Steindl and many others.

Of the well-known facts in this respect I shall first consider the viscero-sensory and the viscero-motor reflex. As is known, in all organs supplied by the sympathetic system, in cases of disease, sensory impulses can be conveyed to certain regions of the spinal system and can produce sensations of spontaneous pain, hyperæsthesia towards tactile stimuli and persistent spasmodic contraction of striated muscles. This phenomenon, which has gained considerable value in diagnostic evidence of organic diseases, is effected through the stimulation of afferent sympathetic fibres, and is transmitted over the rami communicantes to the corresponding segments of the spinal cord, in which they produce irritation of ganglionic sensory and motor cells, being connected with peripheral regions. The viscero-motor reflex is most commonly found in the acute processes in the abdominal cavity. The viscero-sensory reflexes occur in gall-bladder diseases, stone irritation in the common duct, in gastric and duodenal ulcer, in appendicitis, in renal affections and so on, producing various hyperalgesic areas. The observation of herpes zoster in a movable kidney (Rosenberg) can be considered one of the most characteristic examples of viscero-sensory reflex. Also headache, which often occurred in my cases of nephroptosis, without renal disease being present, can, according to v. Bergmann, sometimes be interpreted (after Head) by viscero-sensory reflex, inasmuch as sensations pass through assumed centripetal fibres to the visceral vagus nucleus, and from there

radiate to the nucleus of the trigeminal nerve. V. Bergmann gives a similar explanation of the vertigo also frequently present in movable kidneys; he supposes a radiation of sensations from the vagus nucleus to the nucleus of the vestibularis, which, in the sense of Head's area in the labyrinth, produces disturbances of the equilibrium.

Of much greater importance in answering the question, how to understand in the presence of movable kidney, the symptoms of a presumed but unreal disease of abdominal organs, is the work of v. Bergmann and his pupil Westphal on viscero-visceral reflex, interacting between all organs supplied with unstriated muscles. If such a viscero-visceral reflex becomes abnormally stimulated by some mechanical or pathological factor, the functional equilibrium between the motor and inhibitory action of the parasympathetic and the sympathetic can be disturbed in that sense, that one of this group receives a reinforced or weakened impulse, resulting consequently in a hyper- or hypotonus of the organ muscle concerned. The viscero-visceral reflex can start and end in any organ controlled by the involuntary system. Most prevalent seems to be the effect of hypertonus, appearing as spasm of a transient or more persistent nature. This has been demonstrated in its most simple and frequent form through X-ray examination as pylorospasm, regional or total gastro-spasm in connexion with gastric or duodenal ulcer, appendicitis, salpingitis or gall-bladder disease as source of origin of the viscero-visceral reflex.

Further observations of the effects of such pathological stimuli transmitted by viscero-visceral reflex are those of the reflexly impaired outflow of the bile by a spasmodic contraction of the sphincter papillæ and the lower end of the common duct, as occasionally takes place in any abdominal disease. The same is the case with constipation accompanying so many diseases, in which reflex action can be shown in the rather regular X-ray picture, which reveals dilatation of cæcum, ascending colon, hepatic flexure and of the beginning of the transverse colon, while the distal part shows marked spastic contraction.

But not only diseases must be regarded as an origin of a pathological viscero-visceral reflex; even simple mechanical irritation seems to be enough to produce an abnormal remote reflex passing to unstriated muscles as a pulling or dragging action on the organ wall. Let me mention only the functional disorders of the stomach in gastroptosis, those of stomach and large intestine when the omentum is fixed low down as in hernia. Frazer noticed them in a movable cæcum. Remember further the serious functional derangements of different organs, when you find at the operation only bands and membranes causing adhesions. Also foreign bodies such as bullets or shell splinters may be responsible for violent persistent spasm (Klapp, Pribram).

Besides these mechanical agencies, I must finally mention the known fact of psychical factors conveying spastic reflexes to abdominal organs and vice versa, the fact that peripheral disease may highly affect psychical centres.

I have thus collected some clinical facts, which show the rather constant appearance of spasm effected by a viscero-visceral reflex. Yet we do not know why this spasm is not present in any case of a visible cause of it, why in one case it is transient, of short duration, in others persistent for a comparatively long time. Knowing the close relationship of the involuntary nervous system to the glands of internal secretion, we must think of endocrinal influences upon the whole organism in the sense of perhaps a greater readiness to respond to stimuli, or of the influence of certain glands upon the particular group of the parasympathetic. Also hormonic influence may be of a certain importance. In any case this side of the problem is still very obscure. Equally uncertain are the paths by which these reflexes are transferred. In cases where the reflex acts on the muscular coat of the same organ, the disease of which gives rise to the reflex, explanation seems easy, the reflex mechanism seeming to take place entirely in the myenteric plexus of the organ wall. The matter is more complicated if we are to suppose a remote reflex action.

It was assumed that there might be perhaps a reflex centre in the cœliac ganglion, or that the way of the reflex might lead over the higher centres in the medulla oblongata. Should this be so, we must conclude that reflex action can also be influenced by any disorders put on the way the reflex is converged upon. Very interesting observations on this point have been published.

In cases of violent spasm in the enteric system Exner and Jaeger described in the cœliac ganglion thickening of the connective tissue, Koennecke, inflammatory changes, Prader a metastasis of carcinoma. Steindl noticed in the post-mortem on a case of enterospasm, perivascular infiltration and forming of glioma tissue in the dorsal nucleus of the vagus and the substantia reticularis, Porges in a case of persistent pylorospasm a carcinoma in the cerebellum, spreading to the dorsal nucleus of the vagus, Schüle saw in duodenal spasm a glioma of the size of a pea on the fourth ventricle in the vicinity of the vagus nucleus. These observations must be considered very valuable in the further study of these reflexes.

I think I have given a plausible explanation of the frequent observation of spasm occurring in many parts of the alimentary canal, caused by reflex impulses originating from different sources of disease and disorders in the abdominal cavity.

If we now ask what is the direct effect of the spasm upon the organs concerned, we can distinguish two different effects—stasis and pain. As to stasis, we know its possible effect upon delayed emptying of stomach contents for many hours above the normal time, caused by mere pylorospasm or by organic narrowing associated with pylorospasm. This can lead to the formation of gastric ulcer if we rely upon the theory, stating that through spasm local ischæmia is produced on which stomach secretion acts. Spasm of the sphincter Oddi impairs the outflow of bile, causes jaundice and disposes to infection and stone formation. Spastic contraction of the cæcum or any part of the colon leads to abnormal retention of fæcal masses in the bowel, to inflammation of the mucous membrane of the colon, cæcum and appendix, and so to toxæmia (Lane).

These observations show the significance of spasm as an etiological moment of disease genesis, which, once developed, can form a vicious circle in maintaining spasm and can explain that so often after removal of the primary lesion, the troubles remain. Consider only the unsuccessful operations for appendicitis! The pain connected with spasm arises in various ways; according to Leriche it may result from the pressure of muscular fibres on sympathetic nerve tissue, the stimulus being transferred through assumed centripetal nerve fibres to the sensorium and there being felt as a cramp—or the pain, according to Propping and Leriche, starts under the influence of an exaggerated hypertonic action of the organ-wall, which exercises a mechanical pull on the involuntary nerve-plexus around the vessels leading to these organs. Such pain is generally of an indistinct violent colic character, shooting to different distant parts.

These remarks about functional disorders of abdominal organs may seem not to be connected with the subject before us, and yet what I have said is the essential basis and the key for understanding the occurrence of so many functional derangements of local and general nature and of real diseases met with in movable kidney. For the kidney is as much dependent upon the involuntary system as are the abdominal organs; therefore it would be difficult not to see why principal conclusions as regard reflex interaction between different organs through the medium of the involuntary nervous system, drawn from observations on abdominal organs, should not be applied also to the kidney. Hence, kidney disorders, and amongst them movable kidney, must be considered as well, as a possible source of originating a viscero-visceral reflex to the kidney itself and to remote organs. Let us consider from this point of view the various pathological conditions in movable kidney itself. There is first the dilatation of the pelvis of the kidney to degrees ranging from simple and hardly noticeable pyeloectasia to very pronounced hydronephrosis. The general

opinion is that the displacement of the kidney gives rise to a kinking of the ureter around its normal points of fixation or around some aberrant vessels, which impairs the normal drainage of urine. Or that a torsion of the kidney along its vertical axis effects a faulty, too highly situated outflow of the ureter, causing again retention. I cannot adopt this opinion in all cases of my observations and even not in all those published. I was seldom convinced, from the different ureterograms, that the apparent kink was so effective as to produce dilatation or hypernephrosis, the catheter being in such cases often easily introduced over the site of the kink or the pelvis being injected without difficulty. During operation I often was not able to find the point of the obstruction nor to reconstruct the picture of the kink by turning and moving the kidney in different directions. Again in a number of cases we cannot notice any obstacle nor any faulty direction in the entire course of the ureter, and yet an increased capacity of the pelvis is detected. Do not such observations lead us to the conclusion of a functional disturbance of the expelling power of the pelvis muscle or a functional obstruction in the lower urinary passages? That such a supposition is not without justification can be demonstrated by a useful method recommended some months ago by Perlmann and called "Dynamoscopy." The method is to fill the pelvis with a boric solution stained with indigo-carmin and to observe by a cystocope after the withdrawal of the ureteral catheter, the rhythm, energy and quickness of the efflux of the stained solution in the bladder. In using this method in several cases of movable kidney with hardly any enlargement of the pelvis and no signs of obstruction of any kind in the ureter, I could observe expulsion times prolonged five to seven times above the normal, which is 4 to 5 minutes. Such functional disorders of the pelvis can also be demonstrated by a series of pyelograms or by pyeloscopy as practised with such success by Legueu. However I must admit that, perhaps for different reasons, I have until now not been successful in obtaining undoubted and satisfactory screen observations of the pelvis of a movable kidney.

These considerations and observations impress me with the belief that functional disturbance plays in nephroptosis a greater part in the etiology of dilatation of the pelvis and hydronephrosis than is perhaps appreciated. I dare say also that impaired staining of the urine after injection of indigo-carmin, the delayed appearance of it, the diminution of the specific gravity and the diminished excretion of urea, hæmaturia, and even temporary anuria, observed by others and by myself, without any evidence of an organic kidney disease, may with all necessary restrictions be considered as a functional disorder in movable kidney.

From these functional disturbances to the disease is but one step. Retention leads to infection which very often complicates movable kidney. Pyelitis, pyonephrosis, pyelonephritis are the gradual sequelæ of bacterial invasion. Again, retention, alone or combined with infection, gives a predisposition to stone disease frequently met with in movable kidney.

So much for the significance of functional troubles in the kidney itself. Even more convincing to my mind are the observations of all those functional abdominal symptoms with or without a pathological basis, which, if my thesis is admitted, find their etiological explanation also in the abnormal mobility of the kidney. These symptoms and the clinical picture are the same, perhaps sometimes only more pronounced, as those met with in the various pathological conditions of the abdominal cavity before mentioned.

As regards such abdominal complications, we find in almost all former articles on movable kidney the opinion that it exerts a mechanical pressure on the abdominal organs, explaining thus the various symptoms and signs. In all those cases in which a wrong interpretation of pains and spasms induced me to open the abdomen I have not once gained the conviction that pressure of a movable kidney on pylorus, duodenum, gastro-hepatic ligament, colon or cæcum could have produced a

mechanical compression or kinking of these organs. This does not seem even theoretically probable, when we remember how easily a movable kidney, not fixed by secondary adhesions, slips from under our fingers on pressure in palpation. But the proof of the absolute impossibility of an actual compression seems to be given by observations, which often can be made, that the kidney is so displaced excluding any mechanical contact with the organ, to which present symptoms and signs had to be attributed. For instance, in case of a downward displacement of the kidney—combined with jaundice and pylorospasm, or displacement towards the middle line and symptoms of appendicitis—where is the mechanical pressure? Also these cases can be interpreted in no manner other than by the above-mentioned direct reflex action from the normal or diseased movable kidney to the abdominal organs.

The laparotomies of which I spoke, performed upon a wrong diagnosis, showed me, however, that the kidney can influence reflexly also in a more indirect way the abdomen, by producing a further secondary source of reflex issue in a mechanical way. I found in most of these cases and also in others, where I opened the peritoneum intentionally, various bands and membranes of adhesions between duodenum, under-surface of the liver, gall-bladder, transverse and ascending colon, or rather more frequently between these organs and that part of the posterior parietal peritoneum which was constantly irritated by the up-and-down or lateral or medial movements of the kidney. In these cases I was particularly struck by the comparatively small degree of mobility combined with a marked severity of the symptoms. Perhaps this secondary source of irritation influenced the susceptibility of this region to stimuli and, added to the primary source situated in the motility of the kidney, contributed to the aggravation of the reflex impulse.

These are also in my experiences the cases which give a rather worse prognosis, because removal of the primary lesion is not sufficient, as this special secondary lesion cannot be with any certainty excluded for ever, adhesions forming so easily again and again. No wonder that in such cases nephropexy is not followed by a success or at least by a full success. Anyhow these severe cases need particular attention as regards secondary changes in the abdomen, and nephropexy often ought to be combined with opening and revision of the abdominal cavity.

I have tried to show how great a part the involuntary nervous system plays in forming the clinical picture of movable kidney. Considering that in it many or few or even only one of the possible sequelæ may be predominant, we can now understand what great difficulties are occasionally experienced in elucidating the true cause of the primary disturbance. Considering further that psychical effects may be the overwhelming part of apparent symptoms, we cannot be surprised that many such cases are identified with the so-called idiopathic neurasthenia or hysteria. Such facts must be known to us to gain the right diagnosis and to think of the necessity of palpating the kidney. This must be done both in the recumbent as well as in the upright position. The different evidence the standing posture gives as compared with the reclining may be seen from pyelograms, to be expressed by the distance of one and a half vertebra.

I can add nothing to what is known as regards the etiology of movable kidney. It is mostly accepted as an acquired anomaly, due to many different factors. But it seems to me not without possibility that in some cases when a movable kidney is detected already in childhood, the supposition of a congenital condition is justified. This leads me also to think that some "nervous" children or some of the children who in early infancy have symptoms of severe and persistent pyelitis may be suffering from movable kidney.

The conclusions drawn from my study must be, that in spite of many opposite opinions on surgical treatment of movable kidney, nephropexy is a justifiable operation in any case in which there are any kind of symptom, and that nephropexy is the only treatment which can give lasting results. I do not believe in the efficiency

of any kind of bandages or belts. Of course we must be aware that in all cases complicated by a vicious circle of symptoms formed by secondary changes in the kidney itself, in the abdominal organs, or in psychical or nervous centres, the prognosis as to the end result may be at least very doubtful. However, also in these cases, nephropexy, combined with the exact revision of the kidney and the abdominal cavity, and, if necessary, with the repair or removal of the diseased organ, ought to be attempted. With regard to such a possibility an early diagnosis and early treatment of movable kidney is of great importance.

The technique of nephropexy must go along such general lines as to ensure a proper fixation of the kidney and a good drainage of the urine. Of all the methods recommended I perform most frequently through a lumbar incision suspension of the kidney around the last rib by a large flap cut out of the almost always thickened fascia propria. To this I add sutures, closing both renal fasciæ from below upwards, thus giving a kind of support to the kidney. This is a combination of Vogel's and Klapp's method. Needless to say, I always add an exact exploration of the kidney, of the ureter, and, whenever necessary, of the abdominal organs. My results, as far as I have been able to trace them—having twice been forced to change the place of my practice—have been very satisfactory, although I must admit that a few cases were little or not at all relieved.

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Discussion.—Mr. J. Swift Joly (President) said that Professor Jurasz had raised, in a new form, several controversial points long associated with the subject. One of the most important was as to whether the symptoms of movable kidney were caused purely by mechanical means, or whether there was at work a physiological factor parallel with pylorospasm or cardiospasm. Another was as to whether movable kidney should be treated by surgery, or by medical and other non-surgical procedures.

Sir John Thomson-Walker said it was somewhat difficult to discuss some of the obscure points in the paper, points which were, however, of extreme importance.

There was the question of the influence of the reflexes as a cause of the symptoms, as opposed to mechanical causes. He did not intend to follow Professor Jurasz into the question of visceral reflexes, but he would like to discuss a point not mentioned in the paper, namely, whether the symptoms associated with movable kidney ought to be considered alone, or whether other abdominal organs shared in the causation of these symptoms. The French school had always held strongly that movable kidney was only a part of a general visceroptosis. He (the speaker) did not go all the way with them in that belief, i.e., that general visceroptosis was present in every case of movable kidney, but he felt sure, as a result of his experience, that there were few movable kidneys in which the mobility of the kidney was the only abnormal condition present; in addition, there were in many cases, changes in the position of the ascending colon, of the cæcum, and often of other organs too. Thus, it was important not to confine attention to the movable kidney. He thought many of the symptoms were not due to the kidney at all, but were connected with the bowel, the kidney only participating in the general visceral derangement. The relation of the bowel condition to movable kidney had been fully discussed in this country. In this connexion he would mention the work of Sir Arbuthnot Lane, who, as early as twenty years ago, said that one of the factors in movable kidney was adhesion and dragging of the ascending colon and cæcum on the kidney itself. Another observer was Mr. George Waugh, who had done much work on the ascending colon and cæcum. It became a question whether, in dealing with movable kidney, one ought to deal with the kidney alone, or whether one ought not to include the intestine in any nephropexy operation.

Another point, which probably Professor Jurasz would find it difficult to explain, was why in some cases of movable kidney there were no symptoms at all. On the "reflex" idea, a kidney

which moved all over the abdomen ought to give definite symptoms, but such was not the case. Some gave rise to very severe symptoms, while others, equally movable, caused no symptoms at all.

Professor Jurasz invoked a reflex or functional effect, either causing functional spasm, or inability to contract owing to paresis of muscle. But he (Sir John) thought there were other points. In many cases of movable kidney there was a good deal of bowel trouble. In these there were adhesions at the back of the colon, and he had frequently noticed in cases of movable kidney that there were adhesions behind the bowel which could easily kink the ureter. Unless there was some such change in movable kidney he did not think there would be a kink. In those cases he thought it was largely due to adhesions, not gross in degree, but definite.

The next point to discuss was, what were the cases to choose for operation? There was no operation more disastrous to the reputation of the surgeon than nephropexy, and the reason was that sufficient care had not been exercised in choosing the cases. He understood Professor Jurasz to say that in all cases which showed symptoms, operation should be performed, but he (the speaker) did not agree with that. In cases of movable kidney one should not operate on the kidney itself unless there were symptoms definitely pointing to it. If operation were restricted to such cases, the results would be good. The best way to choose cases for operation was to put the patient to bed for a fortnight. If during that time the symptoms were in abeyance, but were renewed when the patient was allowed to get up, then stitching up the kidney resulted in a cure. In another type of movable kidney, by putting the patient to bed for a time the symptoms were made worse. These were chiefly bowel cases, and were not improved by the performance of nephropexy.

He further suggested that if one was dealing with movable kidneys, more should be done in the matter of X-ray examination of the bowel before operation.

With regard to the form the operation should take, in fixing movable kidneys many mistakes were made, such as :—

(1) Not seeing that the kidney was really *fixed* by the operation. If it was merely slung up one could not expect to limit its movement. Solid fixation to the posterior abdominal wall should be secured.

(2) Surgeons were apt to put the kidney into the position which they considered normal. This was all right in a normal person, but these individuals were not normal; numbers of them were of the class in which there was what was called the "virginal" form of movable kidney. There was a long, narrow chest, and most of the viscera were rather low down. There was a liability to fix a kidney in a position where downward pressure was likely from liver and diaphragm. It was more important to make sure, in operating, that the kidney was fixed and that the ureter was not twisted, than to strain a point to get the kidney high up under the rib.

(3) When stripping the capsule, the anterior surface was stripped as well as the posterior. This was not desirable, as the kidney should not be fixed to the bowel.

Would it not be wise, in cases in which the surgeon was somewhat uncertain as to the cause of the symptoms in a particular case, to consider colopexy at the same time as nephropexy? If the bowel were dealt with at the same time as the kidney, the usefulness of nephropexy as an operation would probably be thereby extended.

Mr. HUGH LETT said that with regard to the selection of cases of movable kidney suitable for operation, he had been conservative. He did not operate unless there was definite evidence either of renal pain or of distortion of the kidney pelvis. If one operated on patients because they had general abdominal symptoms, particularly nervous symptoms the last state would probably be worse than the first.

Mr. R. H. JOCELYN SWAN agreed with Sir John Thomson-Walker that one must not regard the symptoms of movable kidney too much on the functional side. He ranged himself with those who considered that the renal displacement was part of a general enteroptosis. When Professor Jurasz had spoken of abdominal cases in which adhesions were found on the under surface of the liver and about the duodenum and the ascending colon, and of the kinks which Sir Arbuthnot Lane had described, he (Mr. Swan) felt that this was simply an expression of a general enteroptosis, the kidney not being the only organ at fault. Often the symptoms were mixed and obscure, so that one was led to the question mentioned at the end of the paper, as to which cases ought to be subjected to nephropexy and which should be left alone, or have treatment for the general disturbance. He (the speaker) did not think that the

adhesions described by Sir Arbuthnot Lane were altogether due to chronic constipation or to intestinal stasis; they were rather due to a general enteroptosis, including a nephroptosis.

Was one to attack the kidney directly and fix it, or should the kidney trouble be treated by remedying the abdominal condition generally?

With regard to Sir John Thomson-Walker's remark that it did not much matter where the kidney was fixed, so long as it was fixed securely, his (the speaker's) view was that the kidney should be fixed as near as possible to its normal position. It had been his unfortunate experience to operate in cases which had already been surgically treated for movable kidney, and in which he had found the kidney lying obliquely, sewn along an oblique lumbar incision. In fixing movable kidney, if it could be slung round the last rib by a broad band of capsule taken from the posterior surface, that constituted a very efficient fixation operation; but the essential point in the operation for fixing movable kidney was thorough decapsulation of the posterior surface and removal of the fatty tissue. He himself fixed the lower pole of the kidney to the quadratus lumborum muscle.

Professor Jurasz had said that pyelitis was an important condition, and that the dilatation was secondary and due to reflex innervation of the renal pelvis. That was probably so in some cases, but it would not explain all cases of dilatation. In operating upon these cases, one frequently found points at which the ureter did not separate easily from the peri-urethral tissue, but was fixed there by adhesions. Those adhesions formed a potential source of back pressure, causing dilatation, and giving rise to many of the symptoms associated with movable kidney.

Mr. F. A. G. JEANS said, with regard to Sir Thomson-Walker's remark on the relationship of mobile kidney to the state of other abdominal organs, that whenever he (the speaker) had a case of movable kidney, he had an X-ray examination of stomach and colon also made, and if the motility of the kidney was part of a general visceroptosis, he did not operate upon the stomach, kidney, colon or any viscus. Of course the patient would have been previously examined by X-rays to ascertain whether stone was present. A certain number of cases of movable kidney were cured by fixing the kidney, but selection of appropriate cases for the operation was difficult. The cause of the condition was mechanical, and in almost all cases in which the procedure was efficacious, there was an aberrant renal artery, and possibly there were adventitious adhesions. Twenty years ago pyelitic kidneys were explored, and the kidney was brought out of the wound for the purpose of examination, a proceeding which probably tore the aberrant artery and so effected a cure in a certain number of cases, i.e., they were cured through sheer ignorance. Not only did Professor Jurasz operate on the right cases, but he took much trouble in arriving at his diagnosis.

Mr. ZACHARY COPE said he had recently had an opportunity of discussing the results of the operation for movable kidney with Mr. Billington, of Birmingham, who had a greater experience of the operation than that of most surgeons. Mr. Billington performed a thorough operation, fixing both kidneys, removing the appendix, and inspecting the gall-bladder; a review of 250 consecutive cases was undertaken by an impartial critic, and it was found that in 43 per cent. of the cases the patients were perfectly well from eighteen months to six years after the operation. These results were very good, even if the amount of relief due to the removal of the appendix could not be accurately appraised.

He would relate a personal reminiscence concerning his own right kidney, which had been mobile and had given rise to pain about twenty years ago. He wore a specially prescribed belt. After an acute attack of pain, a swelling was felt in the right iliac fossa and was presumed to be the misplaced kidney. An anæsthetic was administered with the intention of reducing the supposed misplaced kidney, but when he awoke it was to find that a large rubber tube had been inserted into a large appendicular abscess! Since then he had not been troubled by his mobile kidney.

Dr. F. G. EDHOLM said that by measures short of operation, movable kidneys could be tightened up into a more or less normal position. The average case of the kind was much benefited by suitable exercises, the kind of exercise being largely a matter of common sense. He had had cases in which the condition had been restored to normal without trouble.

Mr. G. E. NELIGAN said that one knew the kind of person who had duodenal spasm and underwent operation for duodenal ulcer, no ulcer, however, being found. One also knew of cases of spasm of the sigmoid, such as, for instance, in overworked surgeons, who thought, in consequence, that they were suffering from carcinoma and who, after returning from a holiday, had no more symptoms. There was no reason why the pelvis of the kidney should

not be the seat of a spasm causing pain. In a general hospital one saw many such cases, especially among visceroptotic women. The patients complained of right renal pain, but the pyelogram showed nothing wrong with the kidney or the ureter. Often the symptoms originated in some acute anxiety or mental trouble. Sometimes bands and adhesions were found which had been present without obvious inconvenience since birth. He would like to know how spasm of the pelvis of the kidney could be expected to be cured by fixing an otherwise normal kidney.

Mr. J. SWIFT JOLY (President) said that Mr. Neligan had very well put the point which had been occupying his (the President's) own mind. Professor Jurasz had, rightly he thought, attributed to spasm, dilatation or other physiological processes, the pain of movable kidney, yet in every case he advocated mechanical fixation of the kidney as a means of cure. He (the President) wondered whether by this fixation the surgeon was not doing more than he realized; in reducing the kinking was not one stripping the kidney of some of its important visceral nerves, and so altering reflex spasm?

Professor JURASZ (in reply) said that the subject was an old one, and he was aware that it was associated with a certain amount of disrepute. Too many kidneys had been subjected to the operation of fixing, and certainly some were badly fixed. In some cases, even if the diagnosis had been carefully arrived at, the operation ought not to have been performed. Spastic conditions, like those elsewhere, existed in the kidney. In some cases having a good outflow, an enlargement of the pelvis was discovered. During operation, adhesions might be found low down, but they did not show in the pyelogram. The spasm explained many occurrences which otherwise were inexplicable. When pain and jaundice had existed a long time, one naturally thought of the gall-bladder, but on operating one found nothing abnormal in the abdomen except adhesions, and these not sufficient to cause obstruction. Salpingitis sometimes caused jaundice, but there was only slight obstruction and no disease in the tubes. Many of the cases could only be explained by reflex action, such as was familiar in the abdomen to all operators. In a child with appendicitis the pain was often referred to the stomach. Therefore functional disorder could not be left out of account in connexion with this subject; even when kinks were present, some of the symptoms were due to spasm. Of course he did not contend that all the symptoms of movable kidney were due to functional disorder.

He agreed that it was difficult to select cases suitable for operation. During thirteen years' practice, in which many patients had passed through his hands, he had only operated on fifty-eight cases of mobile kidney. For example, a physician had asked him to operate on a patient for appendicitis. On opening the abdomen, there was no trace of appendicitis, but the speaker found a movable kidney, and told the physician that this was the cause of the symptoms. The kidney was fixed, and the patient was relieved of all symptoms.

He agreed that a movable colon had much to do with some cases of movable kidney, as the colon dragged on the kidney. In doing nephropexy it was important to free any constricting tissue, so as to make the kidney even more movable; then one could perform the slinging with greater certainty of success. He believed that many cases of spasm developed into organic trouble later, such as stasis, ectasia and stone formation. There were persons who responded readily to all stimuli, physical and psychical, and these individuals were very liable to spasm. He thought functional disorders ought to be taken more into consideration with regard to such conditions as movable kidney. The mere motility of the organ was a stimulus for various contractions. Perhaps with the wider view that he had suggested the performance of nephropexy might have its reputation restored.

[*November* 22, 1928.]

Routes of Absorption in Hydronephrosis: Experimentation with Dyes in the Totally Obstructed Ureter.[1]

By DUNCAN M. MORISON, F.R.C.S.Ed.

ABSTRACT.—In hydronephrosis the contents of the renal pelvis are constantly being changed. The exact mode of the interchange is still obscure. With the hope of obtaining some evidence of this absorption, a series of experiments has been carried out, introducing dyes at various periods in total hydronephrosis.

The findings of previous experimental work on the subject are conflicting. Two views are held; one, that absorption occurs solely through the tubular system, and the other, that there is a direct outflow from the renal pelvis into the venous system.

In the present work only such an amount of dye was introduced in each instance as to be well within the pelvic capacity of the type of kidney (rabbit). The factor of positive pressure forcing the dye into abnormal channels was thus reduced to a minimum.

Two groups of experiments were undertaken. In Group I, the dye was introduced at the outset of the hydronephrosis, and in Group II the dye was introduced at varying periods in the course of an established hydronephrosis.

The results obtained would indicate that in total hydronephrosis there are two routes of absorption from the renal pelvis, i.e., lymphatic and tubular. At the outset of complete ureteral obstruction there ensues for the first two or three days an active lymphatic absorption from the walls of the renal pelvis and ureter. After about the third day, tubular absorption commences and continues more actively than the lymphatic. If, however, dye is not introduced till the third day of a hydronephrosis, there is rapid tubular absorption but no lymphatic absorption. With longer periods of hydronephrosis the dye is drawn up the tubule system as far as the convoluted tubules. The further absorption of dye into the general system has not yet been obtained.

It is known that in hydronephrosis[2] the contents of the renal pelvis are constantly being changed. The exact mode of the interchange is as yet not apparent. To permit of a continuation of renal secretion, even though that secretion is admittedly progressively modified, there must be some process of reabsorption going on from the totally obstructed sac of the renal pelvis. With the hope of obtaining some evidence of this reabsorption, a series of experiments has been carried out, introducing dyes at various periods in hydronephrosis.

The findings of previous experimental work on the routes of reabsorption in total or closed hydronephrosis are conflicting. Burns and Swartz, working on dogs, were able "to demonstrate conclusively that a path of absorption is by way of the tubules and through the glomeruli into the blood-stream." Hinman and Lee Brown claim that the route is not through the tubules, but by fine venules directly into the venous system. This mechanism they term "pyelovenous backflow." Bird and Moise have more recently obtained results which do not, in their opinion, support the idea of "pyelovenous backflow."

In the work that has been done, the factor of "pelvic capacity" appears to have received little attention. Dye media have been injected into the renal pelvis in amounts far in excess of the total capacity of the respective type of kidney, and at pressures fully capable of producing trauma and forcing the dye into abnormal channels. Further, results drawn from autopsy material cannot correctly be compared with those derived from investigations on the living animal. To try and obviate these apparent discrepancies in the present work and to simulate more closely the natural process, only such an amount of dye was introduced in each instance as to be well

1 From the Department of Surgical Research, University of Edinburgh.

2 For convenience and simplicity, the term hydronephrosis is used to embrace the condition, from the induction of complete ureteral obstruction onwards.

within the pelvic capacity of that kidney. It was decided to observe the effects of dye reabsorption in two separate groups of experiments, the dye being introduced in Group I at the outset of a hydronephrosis and in Group II at varying periods in the course of an established hydronephrosis.

Experimental Procedure.

Animals employed.—Eighty-two rabbits, two dogs.

Dyes used.—Berlin blue, 2 per cent. in normal salt solution; collosal argentum, 1 per cent.; india ink (Pelican), 5 per cent. to 50 per cent. in normal salt solution.

Operation.—Throughout the series the lower end of the left ureter was selected as the site of total obstruction. Access was gained by a mid-line suprapubic incision.

In *Group I* dye was introduced into the renal pelvis through a fine ureteral catheter (No. 4 Ch.) passed up from the lower end of the ureter.

On withdrawal of the catheter the ureter was ligated, care being taken to prevent any spilling of the dye. The usual amount of dye introduced was 0·4 c.c. The rabbits averaged from 3½ to 4½ lb. in weight.

In *Group II* the lower end of the ureter had previously been ligated and left for a varying period. The contents of the renal pelvis were aspirated, the amount measured, then half that quantity in dye introduced, the ureter being again ligated with the usual precautions.

The procedure thus aimed at having the dye lying passively in the renal pelvis, and obviating, as far as possible, the factor of positive pressure forcing it into abnormal channels.

In *Group I* the process of dye absorption was observed after periods varying from thirty minutes to hours, days and weeks.

In *Group II* observations were made from a period of thirty minutes' hydronephrosis, followed by thirty minutes with dye, to equal periods of each, ranging from hours to days and weeks. Several series were also carried through of unequal periods of each, i.e., 1 day hydronephrosis, with 2, 3, 4, etc., days of dye; 1, 2, 3, etc., weeks of hydronephrosis, with 1, 2, 3, etc., days of dye.

At the conclusion of a prescribed time the animal was killed and the right kidney, spleen, and portion of the liver removed and fixed *in situ*. The left kidney and ureter were left *in situ* and subjected to Jore's fixing solution, usually for twenty-four hours, before being carefully resected and submitted for a further fixing. The ureter was then divided at the pelvo-ureteral junction, longitudinally incised, and pinned out for clearing. Thick, serial frozen sections were taken of the left kidney and cleared. Ordinary paraffin sections were made of the right kidney, liver and spleen. These were stained with eosin only. The superficial layer of the posterior abdominal wall was also removed and cleared for the purpose of studying the lymphatics.

Findings.

Group I.—Dye introduced simultaneously with the production of hydronephrosis.—In this group of experiments, the first evidence of dye absorption is by the lymphatics of the renal pelvis and ureter. It commences early (in one instance within thirty minutes) and continues as the sole route of absorption for about three to four days. In the renal pelvis the primary site of absorption, is at the angle where the walls of the calyx margin fuse with the expanded base of the pyramid. Later, the walls of the pelvis, as far as the pelvo-ureteral junction, show definite presence of dye. The walls of the ureter show patchy dye absorption at first in the lower portions and later in the upper third. There is a zone, roughly corresponding to the junction of the upper and middle thirds of the ureter, which rarely shows any trace of dye. From about the third day onwards, in addition to this lymphatic absorption, dye may be noted ascending the main collecting tubules at the papilla, and ultimately reaching

the convoluted tubules in the cortex corticis. This degree of tubular absorption has been observed before the seventh day. From the walls of the renal pelvis and upper portion of the ureter, the particles of dye are apparently conveyed by lymphatic channels, which pass posterior to the renal vein, and deposited in an elongated gland lying just below the union of the vein to the inferior vena cava.

In this group accordingly, the outstanding feature is the initial active rôle played by the lymphatics of the ureter and renal pelvis, followed later by the renal tubules. As the hydronephrosis progresses the contained dye would appear to be more actively absorbed by the tubules.

Group II.—Hydronephrosis established previous to the introduction of dye.—With a hydronephrosis of three days' duration and dye for one day, no evidence of any lymphatic absorption could be found. There was, however, a marked dye absorption up the collecting tubule system and into the secondary convoluted tubules situated in the cortex corticis. The tubule systems continue the absortion and after about the fourth day are accompanied by a slight degree of lymphatic absorption in the lower portions of the ureter. As periods lengthen the lymphatics appear to become as active as the tubules.

The findings in this group suggest that even a short period of pre-existing back pressure favours rapid tubular absorption, and that the lymphatics play a late and secondary rôle.

Both groups of experiments show that there are two main routes of absorption from the totally obstructed renal pelvis, i.e., lymphatic, and tubular.

Lymphatic Routes.

Renal Pelvis.—As evidenced by the presence of dye particles, the pelvic epithelium presents numerous intercellular spaces which connect by fine channels to a deeper system in the muscular coats and adventitia; this in turn becomes segregated into larger vessels which accompany the interlobar veins and, passing along behind the main renal vein, communicate with the lumbar glands. As previously stated, the most active lymphatic absorption occurs at the angle where the walls of the calyx margin fuse with the expanded base of the pyramid.

Ureter.—Since the lower end of the ureter was ligatured in all the experiments, the route of lymphatic flow in the lower third of the ureter could not be determined. There was usually a patchy area of dye absorption in the lower middle third zone. In this area the dye particles appeared to lie for the most part in fine channels running at right angles to the long axis of the ureter. Although no definite channels could be seen, it is possible that the lymphatic plexus draining this segment of the ureter, passes into channels which accompany the ureteric branches of the ovarian or spermatic vessels to reach the lumbar glands. In the upper third, the patches of dye absorption appeared to connect up with vessels running towards the renal pelvis.

Tubular Route.

It is to be noted that in all the experiments performed, no dye entered the tubule system till after a preliminary period of back pressure had been established. Once, however, this factor was present, the dye appeared to rapidly ascend the collecting tubules and reach the convoluted tubule systems situated in the cortex corticis. In the earlier phases of hydronephrosis it is interesting to note that the convoluted tubules which take up the dye, are those associated with the most peripheral layer of glomeruli in the cortex. As the condition advances, dye is observed in the tubule systems of the more deeply situated glomeruli. The extent to which dye reaches within the tubules is variable. The secondary convoluted portion is that which presents usually the most copious dye absorption. Occasionally the loops of Henlé are clearly defined. The primary convoluted tubules are difficult to differentiate, but may in some instances show absorption. No obvious dye particles have been

seen in Bowman's capsule. Paraffin sections show dye not only within the lumina, but also lying in and between the cells of the convoluted tubules.

The process of dye absorption is not evenly distributed throughout the kidney. Groups of tubule systems here and there appear to be picked out leaving others completely free of dye. Contrary to the findings of previous writers, no marked predilection for the poles of the kidney has been noted.

With the exception of two instances which will be referred to later, no definite evidence of a further absorption into the systemic system was observed, despite careful inspection of the sections taken from the liver, spleen, and right kidney of the animals used throughout Groups I and II. In many instances dark granules of a yellowish colour were seen in the liver, and spleen. These occurred even when india ink was used as the dye medium. It may be possible that they represent the dye in an altered form. It is difficult, however, to account for any change in the case of carbon particles.

The two exceptions above-mentioned occurred when the pelvic capacity had been inadvertently over-estimated, and a slight excess of dye introduced. In both instances, extravasation occurred from the renal pelvis at the angle where the walls of the calyx margin fuse with the expanded base of the pyramid, and are in intimate association with the interlobar vessels. From this site, the dye had invaded the venous system, and was disseminated. The liver, spleen, and opposite kidney in both cases showed obvious presence of dye. These two accidents apparently illustrate the condition described by Hinman and Lee Brown as "pyelovenous backflow."

Throughout the investigation, the sequence of the changes observed, in each group of experiments, remained practically constant. The rate and degree with which the changes occurred showed, however, marked variation in some instances. In setting down these observations, accordingly, an average has been deduced regarding the time factor. In several of the experiments conducted under Group I the findings tended to resemble those usually obtained in Group II. The explanation which offers itself is that in these animals a mild degree of back pressure may have already existed at the time of operation.

Discussion.

Throughout the present investigation care was taken to obviate, as far as possible, the factor of positive pressure forcing the dye into any abnormal channels. The action of the lymphatics can be readily appreciated, but the part taken by the tubular system is difficult of explanation. Why is there an initial period of delay before the dye begins to ascend the tubules? Is it due to some natural valve-like action at the tip of the renal papilla? Longitudinal sections of the normal rabbit's kidney show that the epithelium covering the sides of the pyramid is cuboidal in type, and arranged in a single layer. As the tip of the papilla is approached this layer becomes elaborated, till around the mouths of the papillary ducts it is fully 5 to 8 cells in depth, and papillary in type. With sudden raising of the intrapelvic pressure as produced by total ureteral obstruction, it may be that this redundant epithelial layer becomes sufficiently compressed so as to lie in apposition over the papillary duct channels, and thus prevent any regurgitation of the pelvic contents. With continued pelvic distention, there is a radial and outward pull on the papilla which would tend to stretch the epithelial layer and remove its obstructing influence from the mouths of the papillary ducts, thus allowing a patent communication with the pelvic contents. Should the initial delay be accounted for by some such mechanism, there yet remains an explanation regarding the ascent of the dye into the renal tubules. Despite the fact that a certain amount of urinary secretion continues to pass down the collecting tubules, yet the dye apparently passes up against stream. Considering the greatly modified process in hydronephrosis, it is obvious that the rate of secretion must be extremely slow. Further, to allow of fluid entering a closed sac which is already full, some of the existing contents

must be removed to make room. It is apparently during this gradual process of compensatory withdrawal that the suspended dye particles are carried up the lumen of the tubules. In the mechanism of renal secretion, the inference is gaining ground that the action of the convoluted tubules as well as of the loops of Henlé, is to reabsorb much of the filtrate issuing from the glomeruli. Should a given glomerulus be so embarrassed as to be unable to furnish its usual quantity of filtrate, then its own tubule system, still capable of good absorption activity, would be left with a margin of absorptive power. It is conceivable that this margin of absorptive power could draw fluids from associated portions of the tubule system, i.e. collecting tubules, especially if a state of fluid continuity existed in the lumen of the system.

In hydronephrosis, it has been shown that the peripheral layer of glomeruli is the first to suffer from pressure atrophy. In consequence the amount of filtrate coming from these glomeruli is progressively lessened, and if their associated tubules are still actively functioning they must be continuing to absorb. As in hydronephrosis the condition is one of fluid continuity throughout the lumen of the renal pelvis and the subdivisions of the tubular system, one would expect that if any absorption were occurring it would be by the tubules of the embarrassed glomeruli. By introducing fine granular dyes into the contents of the renal pelvis, suggestive evidence has been obtained that it is this acquired altered absorption exerted by the peripheral layer of tubules which steadily draws the dye from the pelvis, up the collecting tubules, and into the cells of the convoluted tubules. With advancing periods of hydronephrosis, and pressure atrophy proceeding from the periphery inwards, one finds that this action is taken up by the successive layers of convoluted tubules.

Conclusions.

From the foregoing study it would appear that in total hydronephrosis there are two routes of absorption from the renal pelvis, the lymphatic and the tubular. At the outset of complete ureteral obstruction, there ensues for the first two or three days, a purely lymphatic absorption from the walls of the renal pelvis and ureter. After about the third day, tubular absorption commences and continues more actively than the lymphatic. The convoluted tubules of the most peripheral glomeruli are the first to take on this function, and as pressure atrophy progressively supervenes, the more subjacent layers continue the process of reabsorption. In thus absorbing dye media, the cells of the convoluted tubules may be demonstrating a slightly altered normal function. If so, their action would tend to favour the "filtration and reabsorption" theory of renal secretion.

The routes from this absorption into the general system have not yet been identified.

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Discussion.—Mr. J. Swift Joly (President) said that in this interesting and important paper the author had gone far beyond the consideration of mere hydronephrosis; he had gone into fundamental principles, and had traversed what could be regarded as a great part of renal pathology.

Mr. F. G. Jeans asked Mr. Morison what was his explanation of the anuria, following sudden relief of bladder tension, in cases of enlarged prostate.

Mr. Winsbury White said that he had been greatly interested to see these convincing details of the experimental evidence that a re-absorption took place by way of the tubules. A good deal of clinical evidence suggested that some absorption went on. He had no recollection of a case of hydronephrosis, with a long history, in which there had not been some very definite signs of ill-health. That, he assumed, was due to re-absorption, and the

experimental evidence now adduced seemed to bear out his assumption. Again, if one examined microscopically the renal parenchyma of old-standing cases of hydronephrosis, there was always to be found this marked dilatation of the tubules.

Another point in which he was much interested was the site of junction of the base of the pyramid with the renal calyx. It would be remembered that there was a venous plexus there, and that was the site at which, according to Hinman, the pyelovenous backflow occurred. He was once carrying out experiments on autopsy kidneys in the post-mortem room, and what he had found drew his attention to this phenomenon in a striking way, though at the time he did not understand it. He was injecting liquefied gelatine up the ureter and into the pelvis, and he continued the pressure until there occurred a sudden rupture inside the kidney, and, to his surprise, he saw the renal vein full of liquid gelatine. He hardened the specimen and opened it and found that there was gelatine throughout the venous system of the kidney, and that rupture had occurred at the bases of several pyramids. He understood what had happened after he read Hinman's work. He took it that in what Mr. Morison called his accidental experiment, when rupture occurred, the same thing happened. He (Mr. White) repeated these experiments on several other occasions, and he found that by continuing the pressure he could always get a rupture in the same part of the kidney, and if the force was not relaxed the fluid would be found making its appearance at the hilum, thence spreading out on the surface of the kidney beneath its true capsule, and he thought this might possibly happen during pyelography if undue pressure were applied; in certain pyelograms one saw a diffuse opacity, which might be explained in that way.

Mr. JOCELYN SWAN said that he could discuss Mr. Morison's excellent piece of experimental work rather from the clinical than from the experimental aspect, particularly as to the relationship that the experimental evidence which Mr. Morison had now brought forward might have to the treatment in dealing with cases of renal infection. With regard to the common infection of the kidney by means of the *Bacillus coli* and other organisms, in which there was frequently some dilatation of the renal pelvis, this work suggested that the organisms could find their way into the renal tubules as easily as the particles of the dye which Mr. Morison had shown. If this were the case, it surely pointed to the superiority of medicinal treatment of these cases over-mechanical irrigation of the renal pelvis with disinfecting solutions, and perhaps it explained some of the failures following treatment by these means. The presence of organisms in the tubules of the kidney, and particularly in the cortical portion, would probably explain some of the more severe general symptoms associated with renal infections.

Again, in those cases in which a calculus was present in the renal pelvis, there was not only a certain amount of back pressure in the renal tissues, but, as was well known, many of these cases were infected. Was one, therefore, to be content to remove the calculus and think that by so doing the patient was going to be cured? or ought one, in the light of this evidence, to be more careful to treat the co-existing infection of the kidney after the removal of the calculus? In other words, should one suture the pelvis after a pyelolithotomy, or should one drain the kidney in all cases after the removal of a stone? Another question with regard to the clinical side was prompted by the two instances which Mr. Morison had shown and had described as "accidents," namely, whether solution under pressure had forced its way through the renal substance as far as the capsule of the kidney, a point of great importance in the technique of pyelography which should not be performed under an anæsthetic as no undue pressure should be used. Any anæsthetic necessary for carrying out pyelographic examination should be a local or parasacral anæsthetic.

Mr. KENNETH WALKER said he was himself particularly interested in the question of lymphatic spread; at one time he had done some work on that subject. In recent years, however, he had come to the conclusion that the lymphatics were not so important as he thought they were fifteen years ago. He would like to know whether Mr. Morison paid any attention to the capsule of the kidney in the cases showing an early lymphatic absorption of the dye. In his (the speaker's) own experiments with animals, he found that the capsule was of extreme importance. On placing dyes in the lower parts of the urinary tract, without any obstruction, he found that particles of the dyes wandered up in a surprising way, and distributed themselves throughout the whole of the urinary tract, and almost invariably he found dye in the capsule of the kidney. When one was using carbon and carmine particles, and when the particles were few and far between, it was easy to make mistakes: but in Mr. Morison's experiments, where more concentrated dyes were used it would not be difficult to see whether there was any

dye in the capsule of the kidney. It was known that there was a connexion between the lymphatics of the renal cortex and those of the capsule.

Mr. G. E. NELIGAN said that in all the experiments which Mr. Morison had described, the ureter was ligated. He (the speaker) used to be taught that if the ureter were completely ligated, in the majority of cases atrophy of the kidney would follow. Had Mr. Morison found that in every case in which he ligated the ureter, hydronephrosis followed, rather than atrophy of the kidney?

With regard to the spread of infection up through the pyramids to the cortex of the kidney, Mr. Morison's experiments seemed to confirm the experience of old cases of obstruction due to enlarged prostate. When he was a student he remembered the surgeons operating on these cases and that, if uræmic, the patient died within forty-eight hours after rigors; also that, post mortem, one found, throughout the substance of the kidney, some hydronephrosis, and from there upwards one could see, macroscopically, tracts of pus right up the cortex. In early days one washed out the kidneys with collargol, and if too much was used, one saw beautiful pictures of the collargol going through the whole substance of the kidney and beyond.

Mr. ZACHARY COPE said that in Mr. Morison's able paper there was little or no mention of the route whereby absorption took place into the general system.

The change in the general symptoms which took place when a hydronephrosis became infected was well known and must be caused by absorption, and he was sure Mr. Morison must have some observations and opinions on this question which would be of great interest.

Mr. J. SWIFT JOLY (President) said that he had been much impressed by the statement that the dyes were first collected at the junction between the calyces and the papule itself. According to Wildbolz, that was the first point at which tuberculous lesions were found in the kidney, the disease spreading thence directly up to the cortex.

A second point which had impressed him was that of absorption by the lymphatics in early cases, and by absorption in the tubules in the later cases. Mr. Morison had raised the question whether the thickened epithelium over the tip of the papillæ did not act as a valve in preventing absorption by the tubules in early cases. He was reminded by this of a paper which he had read before the Section four years ago and in which he had pointed out a similar mechanism preventing evacuation of the bladder in patients with an enlarged prostate. The pressure of the vesical contents tended to squeeze the intervesical projection of the prostate towards the urethra from all sides and so to prevent the evacuation of urine.

The question of the spread of infection from the renal pelvis to the kidney itself was a third point which Mr. Morison had, at least indirectly, touched upon. It had a very direct importance, because it showed the inefficiency of the renal lavage. One knew how inefficient urinary antiseptics were when given by the mouth, so that one was really, in this matter, "between the Devil and the deep sea" to know what was the best treatment in this type of infection.

In cases of hydronephrosis due to obstruction of the ureter, a rapid degeneration of the kidney ensued, and the question arose as to the length of time a kidney could remain obstructed, and yet regain its function. The results of experiments designed to settle this question differed a good deal, therefore he would like to know whether Mr. Morison had done work on the point and, if so, what were his conclusions.

Mr. MORISON, in reply to Mr. Jeans, said that in advanced back-pressure, such as that from an enlarged prostate, the arcuate arteries passing round at the base of the renal pyramids, were put on the stretch, thus reducing the calibre of these vessels. When back-pressure was removed suddenly, these arteries, like elastic tubes, tended to spring back and in so doing re-established their former lumen. This permitted an increased blood flow and congestion which resulted in tubular embarrassment and anuria, proceeding even to glomerular rupture with hæmaturia. That, he thought, might be the mechanism in advanced cases, but this was put forward as a tentative suggestion, requiring proof.

Mr. Winsbury White had spoken of the angle at the base of the pyramid, at the junction where the wall of the pyramid joined the base. That was the hinge on which all the movement of back-pressure occurred. [Drawing.] Employing autopsy material in which there was not so much elasticity as in living tissues, he found one was apt to get a tear at this site, and with increasing pressure, a definite rupture. If, however, a stage of back-pressure was already established, the hinge was not so likely to give. If slight overfilling had occurred in a pyelogram showing "clubbed" calices, one saw striæ radiating from the tips of the papillæ up into the pyramids, indicating a flow of the pyelographic medium up the main

collecting tubules, pre-existing back-pressure having rendered the mouths of the papillary ducts patent and in free communication with the cavity of the renal pelvis.

Mr. Jocelyn Swan had asked about chronic *B. coli* infections of the urinary tract. It was an important and interesting question. He (the speaker) thought a case of *B. coli* pyelitis was one of continuing re-infection in the pelvis, due to stasis and produced by faulty ureteral drainage. [Drawing.] People with chronic pyelitis were treated with instillations of silver nitrate, with the idea of desquamating off the lining epithelium, and thus getting rid of the organisms. The results of this mode of treatment were not too convincing. More recently he had been carrying out a series of ureteral dilatations in such cases and had obtained more gratifying results.

The President had referred to renal calculus. One of the etiological factors of calculus formation would appear to be stasis, therefore before removing a renal calculus, it was one's duty to remove this factor. A preliminary series of ureteral dilatations should be carried out. This procedure by improving drainage would lessen any infection present, and provide a better surgical field for the ultimate removal of the calculus.

With regard to the question as to how long after complete obstruction recovery could occur, experimental work on the dog had shown that even after three weeks of total obstruction, kidney function would come back sufficiently to support life, provided the opposite healthy kidney was removed. If, however, a kidney was obstructed for only two weeks, and then relieved, without removing the opposite already compensating kidney, the recovery of that kidney was partial and temporary, and the kidney ultimately atrophied. It would appear that the greatest recovery of functional activity only occurred when the stimulus for full work was present, as after removal of the other kidney. The subject had received special attention from Hinman, who referred to it as renal counterbalance.

Mr. Neligan had enquired as to the incidence of primary atrophy following total ligation of the ureter. Throughout the present series of experiments he (the speaker) had not encountered the condition. He had, however, on previous occasions seen two instances in which primary atrophy had supervened.

Mr. Kenneth Walker had asked about the physiology of absorption, and also regarding lymphatic spread within the kidney. With reference to absorption, he (the speaker) had had the opportunity, last year, of showing some of the present work to Professor A. N. Richards, of Philadelphia, who had expressed his opinion that this retrograde absorption of dye particles into the cells of the convoluted tubules, simulated closely his own findings in which he had introduced dyes directly into Bowman's capsule and found that in their passage down the lumen of the tubule, these dyes were absorbed by the cells of the convoluted tubules. The findings would tend to strengthen the "filtration and re-absorption theory" of renal secretion. With regard to lymphatic spread, the only kidney he had seen which suggested this route, was the one he projected on the screen, in which there was interstitial nephritis.

Mr. Zachary Cope had asked concerning the mode of absorption into the general system from these sites. One could appreciate from the symptomatology that such a re-absorption readily occurred. The difficulty remained, however, of demonstrating the exact mode of the transference. In the instance of interstitial nephritis cited, the dye particles were observed to have been absorbed into the cells of the convoluted tubules, and thence had apparently passed through the basement-membrane into what appeared to be lymphatic spaces. The spleen showed some scanty dye particles in the cells of the reticulo-endothelial system but none could be seen in the liver.

Section of Odontology.

[*November* 26, 1928.]

Dead Tracts in Dentine.[1]

(From the John Hampton Hale Research Laboratory of the Royal Dental Hospital of London.)

By E. W. Fish, M.D., Ch.B., L.D.S.

Abstract.—(1) When the dentinal tubules are opened or sufficiently irritated, their contents coagulate and die.

(2) Following this, the pulp lays down an impermeable barrier of lime salts (secondary dentine) to protect itself from contact with the dead tubules. Alternatively the pulp itself dies.

(3) The evidence that exposed dentine always dies is as follows: (*a*) Such dentine is insensitive right through to the secondary dentine. (*b*) The injured dentine is found experimentally to be shut off from the pulp in such a way that fluids cannot enter it. It thus lacks the necessary body fluids to support life. (*c*) Under an injury the primary dentine is seen to stop abruptly at the original pulp margin, and to be sealed off with a homogeneous barrier of lime salts before the tubules of the secondary dentine start. *The tubules of the secondary dentine take origin below this homogeneous layer in fine branches and obviously have no connexion with the injured primary tubules.* (*d*) The injured tubules although walled off from the pulp remain permeable from the mouth and have therefore not died by slow calcification.

Abrégé.—(1) Quand les canaux dentinaires sont ouverts ou suffisamment irrités, leur contenu coagule et meurt.

(2) Ensuite, la pulpe dépose une barrière imperméable de sels calciques (dentine secondaire) en protection contre tout contact avec les canaux morts. Alternativement, la pulpe elle-même meurt.

(3) Que la dentine exposée meurt toujours est prouvé par les faits suivants: (*a*) Une telle dentine est insensitive dans son entier jusqu'à la dentine secondaire. (*b*) Des expériences ont prouvé que la dentine lesionnée est isolée de la pulpe de façon que les fluides n'y peuvent pas pénétrer. En conséquence, les fluides du corps, nécessaires à la faire vivre, lui font manque. (*c*) On observe que la dentine primaire, sous une lésion, s'arrête nettement au bord de la pulpe originelle et qu'elle est scellée par une barrière homogène de sels calciques avant la naissance des canaux de la dentine secondaire. Les canaux de la seconde dentine naissent en fines ramifications au-dessous de cette couche homogène et n'ont, évidemment, aucun rapport avec les canaux primaires lésionnés. (*d*) Quoique murés vis-à-vis de la pulpe, les canaux lésionnés restent perméables par la bouche et, par conséquent, ne meurent pas d'une lente calcification.

Uebersicht.—(1) Wo die Dentin-Röhrchen offen oder genügend entzündet sind, koaguliert und stirbt der Inhalt.

(2) Gleich darauf legt die Pulpa, als Schutz gegen Berührung mit den todten Röhrchen, eine wasserdicte Schranke aus Kalksälzen (sekundäres Zahnbein) nieder. Abwechselnd stirbt die Pulpa selbst.

(3) Dass das blossgestellte Zahnbein unveränderlich stirbt ist wie folgt bewiesen: (*a*) Ein derartiges Zahnbein ist vollständig insensitiv bis auf das sekundäre Zahnbein. (*b*) Es ist experimentell bewiesen worden, dass das verletzte Zahnbein derartig von der Pulpa isoliert ist, dass gar keine Flüssigkeiten hineindringen können. Und so fehlen ihm die wesentlichen Körper-Flüssigkeiten, die ihn sonst am Leben erhalten. (*c*) Bei einer Verletzung sieht man, dass das ursprüngliche Zahnbein kurz vor dem primären Pulpa-Rande aufhört und vor dem Anfange der Dentin-Röhrchen des sekundären Zahnbeins durch eine homogene Schranke von Kalksälzen verkapselt ist. Die Röhrchen des sekundären Zahnbeins entstehen, in feinen Zweigen, unter dieser homogenen Schicht und haben augenscheinlich gar keinen Zusammenhang mit den verletzten primären Dentin-Röhrchen. (*d*) Die verletzten Dentin-Röhrchen, obwol von der Pulpa isoliert, bleiben jedoch permeabel durch den Mund und sterben deshalb nicht an einer langsamen Verkalkung.

Introduction.

The conception of dead areas in the dentine of a tooth with a vital and healthy pulp is at first somewhat startling, but a moment's reflection will bring to mind

1 This research was undertaken with the aid of a grant from the Medical Research Council, for which the author desires to express his gratitude.

certain clinical conditions which can only be explained by assuming that patches of dentine, if not dead, are at least profoundly modified. For instance, when attrition has occurred at the tip of a lower incisor, we know that the exposed dentine is absolutely insensitive and deeply stained, and it is inconceivable that irritating and infected fluids from the mouth could penetrate the tubules and stain them this dark brown colour without killing the delicate protoplasmic fibril and coagulating the lymph in the tubule. Before entering upon an explanation of such phenomena let us consider the irrigation of the tooth as a whole with body fluids, because it is only by means of these fluids that any tissue can maintain its vitality.

In referring to the movement of lymph in the dentinal tubules it will perhaps obviate any misconceptions which may have been caused by the use of the word "circulation"—and yet at the same time adequately describe the condition—if we speak merely of a mass movement of tissue fluid along the perifibrillar space of the dentinal tubule. There can be no doubt of the existence of this space or of the movement of body fluids along it in bulk, as distinct from a mere diffusion of these fluids.[1] In the crown of a tooth, under the enamel, we see some of the tubules enter the enamel and a slight irrigation of enamel prism sheaths with lymph sometimes occurs even in man. In the dog this irrigation of the prism sheaths is considerable, but the essential point is that in no animal yet examined has any indication been seen that fluids could pass through healthy well-formed enamel into the mouth or from the mouth into the dentine. When such penetration has been observed it has always been seen that the permeable enamel was either hypoplastic or that a large enamel lamella was situated at the point of entry of the fluid resulting in a lesion of the dentine, as we shall see later.

In the root of the tooth the dentinal tubules end as fine branches in a homogeneous translucent zone of highly calcified tissue immediately under the granular layer of Tomes. Some of the branches anastomose directly, and others do so by opening into the spaces of the granular layer, thus permitting the movement of lymph along them. At the same time a very large number of branches end simply in this homogeneous zone (fig. 1) in a way which is strictly comparable with the terminations of the tubules in secondary dentine (figs. 5 and 6).

Conception of the Dead Areas.

Bearing in mind this lymph irrigation of the dentine, we may consider the effect of interfering with its lymphatic system by creating a lesion at the periphery of the dentine. In order to visualize the sequence of events we will suppose that a monkey is anæsthetized and that a patch of enamel together with a definite macroscopic amount of dentine is ground off a tooth, resulting in the exposure of the cut ends of the tubules. These tubules at the moment contain healthy fluid lymph together with the fibril of Tomes. Diffusion immediately takes place between the saliva and the lymph, resulting in the coagulation of the latter. This coagulum does not, however, prevent the passage of more saliva by a continued process of diffusion into the deeper parts of the tubule, causing further coagulation until the whole tubule is filled with coagulated lymph and the fibril of Tomes is dead. Such is the theoretical consequence of opening a dentinal tubule to the mouth, and practical confirmation is obtained if the exposed tubules be covered with a cap containing a diffusible watery dye. The dye will enter the tubule and penetrate as far as the pulp showing that if the lesion had been left uncovered the saliva would have done the same.[2] This process of coagulation and death must inevitably progress further until the pulp itself is also dead if nothing occurs to prevent continued diffusion of irritating fluids from the mouth, and when a wide area of the dentine is exposed, as in a fractured incisor in a child, death of the pulp does

[1] Fish, E. W., "Circulation of Lymph in the Dentinal Tubules with Some Observations on the Metabolism of the Dentine," *Proc. Roy. Soc. Med.*, 1926, xix (Sect. Odont.), 59-72.

[2] Fish, E. W., "The Physiology of Dentine and Its Reaction to Injury and Disease," *British Dental Journal*, June 1, 1928.

commonly result. We must, however, recall that the diameter of a dentinal tubule is small and that diffusion is slow. Bacteria would be slower still to grow down these tubules from the mouth, so that the pulp has time to defend itself from contact with the dead tissue in the tubules if it has any capacity to do so. Upon subsequent examination of the monkey's tooth, if it has been left in the living animal for a few weeks after the operation, we find that the pulp does put up a very substantial barrier which has been known for many years as secondary dentine. The formation of this tissue will be considered in the next section. We may say then that the thesis which we are setting out to prove is that the primary dentine under a peripheral lesion of the dentine is dead not in the sense that it is hyper-calcified, but that the contents of the tubules have died suddenly from exposure to irritation and left the tubules permeable. In other words the dead tract of the dentine constitutes an infarct.

Evidence of the Existence of Dead Tracts in the Dentine.

(*a*) *Clinical Evidence.*—It is a matter of common experience, as was suggested at the beginning of this paper, that there are tracts of hard and apparently normal dentine—as distinguished from carious dentine—which prove insensitive to the dental bur. Notably these areas occur under the tips of the incisors where some degree of attrition has occurred, especially if the tooth has also a mesial and a distal cavity when almost the whole of the tip of the crown is generally insensitive. Another situation where insensitive dentine may be readily detected is under cervical caries, provided that the bur be confined to the dentine *anatomically* directly under the lesion. The sound dentine immediately adjoining this tract is hypersensitive.

It will be seen that these insensitive tracts are very circumscribed, and that they are easily and necessarily over-run in cavity preparation, hence they have not been described except generally, and perhaps inaccurately, by saying that the floor is less sensitive than the walls of a cavity. For this reason at a demonstration given at the Royal Dental Hospital in October, 1928, these insensitive tracts were explored with the dental bur, and were very readily demonstrated not only under attrition and caries, but also at the exposed neck of the tooth where there was no caries. They were found to go as far into the dentine as it was considered safe to follow them having regard to the risk of entering the pulp. They are always present under all lesions of the dentine, but a glance at fig. 2B will show that the clinical "floor" of the cavity is not always the place to seek them. Figs. 2A, B and C are diagrammatic representations of the insensitive tracts rendered visible by the method about to be described.

(*b*) *Diffusion Experiments in Vitro.*—If the insensitive tracts just described are indeed dead as one might suspect from the absence of reaction to painful stimuli, it will follow that they are devoid of body fluids, that is to say that they are so shut off from the pulp that fluids cannot pass into them from the pulp as they do into the live dentine. To demonstrate that these tracts are physiologically isolated from the pulp an extracted tooth is selected with a small carious cavity in it or with dentine exposed by attrition, and a hole is drilled into the pulp-chamber some distance from the lesion. A crumb of solid methyl-blue is placed in the pulp and moistened with water, the hole is sealed with white wax and the tooth dropped into formal salt for a month. On grinding a section of the tooth the whole of the dentine is found to be deep royal blue in colour except the tract of dentine immediately under the lesion, which stands out white against the blue ground and is seen to run from the surface lesion to the pulp-chamber where it ends in a patch of secondary dentine.

When first ground these tracts (fig. 3) appear quite as diagrammatic as the drawings 2A, B, and C. The tubules forming the tract which has received no dye from the pulp are just exactly those damaged at their peripheral end by the lesion whatever it may be, while at the pulp surface they, and they alone, are covered with

secondary dentine. In order to control this method dye was placed in the live pulps of teeth of the dog and monkey without extracting the teeth, and the only difference was that the dye was carried into the healthy live dentine in a few hours instead of taking several weeks to diffuse as it does post mortem, but it was still found that no dye reached these tracts of dentine which had been injured at the surface some weeks previously. It is therefore safe to conclude that such tracts are physiologically cut off from the pulp and are therefore dead. It is moreover obvious under the microscope that these tubules are not calcified up since their necrotic contents are plainly visible. It remains to discover how these damaged tubules are sealed off from the pulp, and in investigating this, further evidence of their necrotic condition is obtained.

(*c*) *The Formation and Function of Secondary Dentine.*—In discussing this subject the experimental production of secondary dentine will be described first and then the distribution and function of the secondary dentine found in human teeth. A monkey is anæsthetized and a large part of the enamel together with an appreciable layer of the subjacent dentine is ground off the labial surfaces of the incisors. At least four weeks must be allowed for the formation of secondary dentine and then these teeth are extracted. After extraction a hole is drilled into the root of each tooth to expose the pulp, and solid methyl-blue is introduced into the pulp-chamber as in the last experiment. It is next necessary to wait for several weeks—keeping the tooth moist and antiseptic—to allow the blue dye to penetrate all available tissue from the pulp-chamber. Finally a section is made by grinding. The plane of the section must include both the cut tubules at the surface and also the pulp.

On examination it will be observed that while the dye has penetrated all the uninjured dentine of the tooth the injured part is still white right through from the pulp to the surface and that at the pulp margin secondary dentine is laid down over the central ends of these injured tubules. Fig. 4 shows the distribution of the secondary dentine corresponding exactly to the extent of the enamel ground off. Fig. 5 is a higher magnification of the part indicated in fig. 4 and is a specimen of a very important type of photograph in establishing the thesis that the injured primary dentine is dead and entirely cut off physiologically from the body. It will be seen that there is shrinkage and degeneration in the contents of the tubules of the primary dentine. Next to these we have a perfectly homogeneous layer of fully calcified materjal which is quite structureless and which completely seals off the primary tubules. Once these are sealed off a much smaller number of new dentinal tubules start to form, and start moreover in the way in which dentinal tubules always do start—that is as fine branches running together to form one main tubule (compare fig. 1). It is particularly interesting to observe that these secondary tubules are in free communication with the pulp and are therefore freely irrigated with dye solution from the pulp, but that the homogeneous layer first formed completely prevents the dye from crossing into the dead primary tubules.

This homogeneous barrier is not always to be seen, and it would sometimes appear that the tubules in the secondary dentine are continuous with those of the primary dentine, but there is abundant evidence that this homogeneous barrier does always exist. Its apparent absence is due to a leash of normal, uninjured tubules being included in the section as well as the injured ones, and these are super-imposed upon each other; but the homogeneous layer can always be demonstrated if the section be by good fortune accurately ground.

Exactly the same phenomenon is observed in fig. 6, which shows a dead tract of dentine under an enamel fault in a temporary human tooth. Now if the injury at the surface of the dentine had merely stimulated the odontoblasts to greater activity so that they laid down more dentine we should not have had the occurrence of a homogeneous zone. This can be seen quite clearly in the uninjured tubules bordering the dead tract which are also shown in the photograph (fig. 6). The

odontoblasts associated with these living tubules N have, owing to some lesser irritation, been stimulated to form a fresh layer of dentine N_1 without the first formed dentine having died. We find therefore an obvious continuity between first formed and later formed dentine—the tubules are irrigated all the way from pulp to enamel with dye and are continuous. But where the primary dentine is actually killed by the severity of the injury the continuity is interrupted by the homogeneous barrier referred to and the tubules in the secondary dentine are obviously new tubules with terminal branches.

One more point must be considered; that is the reduction in the number of tubules in the secondary dentine. If the odontoblasts which originally formed the primary dentine remained intact after the injury they would presumably each form a new tubule in the patch of secondary dentine, but we find that in no circumstances does secondary dentine contain anything like the number of tubules which we find in primary dentine, and we are forced to the conclusion that the odontoblasts which have a process in the injured dentine die with the dentine and only *supernumerary* odontoblasts the existence of which is doubted by many observers, survive to produce the tubules of the secondary deposit. It is hoped to get direct evidence of this from some specimens now being prepared, but meanwhile strong supporting evidence is available in a few specimens where the secondary dentine itself has become infected and died and has been walled off by a tertiary deposit which is practically devoid of tubules and is merely a laminated calcific deposit or patch obviously laid down by the pulp to protect itself from irritation by the dead secondary dentine.

(*d*) *Permeability of the Dead Tract from the Mouth.*—If the above evidence be carefully weighed and if the experiments described be repeated to avoid drawing erroneous conclusions from isolated specimens, where sections are cut on the slant, there can be no doubt that injured dentine dies immediately upon occurrence of the injury; provided only that the injury is sufficient to produce coagulation in the tubules. That the death occurs immediately and not by slow calcification from the periphery inwards is shown by the fact that although the dead tract is cut off from the pulp by an impermeable barrier of lime salts, it still remains permeable to fluids from the mouth; which it would not do if it were fully calcified up by a process of chronic irritation. Its permeability is illustrated by the brown stains it receives from the saliva and experimentally by putting a dye into a carious cavity and observing its penetration in a section ground a week or so later. Moreover, if these dead primary tubules had become calcified before they died they would appear translucent in the sections and not black as they do (fig. 3).

Translucent Zones.

It would be unreasonable to conclude these observations, although they are necessarily lacking in detail, without some mention of translucent zones. A difficulty, however, arises because it has been found impossible to find true translucent zones in primary dentine under caries by the methods employed. When a section is being ground, before it has reached that degree of thinness requisite for examination microscopically a translucent zone is clearly seen by looking through the section at the light, but on further grinding, this zone proves to be the highly calcified secondary dentine. Fig. 7, however, shows a dead tract arising from a carious cavity with the uninjured tubules lying next to it almost completely calcified in their peripheral part, and it is conceivable that if this section had been ground out of the true anatomical plane of the tubules these areas would have met below the lesion and formed a translucent zone. This hypercalcification is, however, rare. It may be the result of irritation of a low grade passing laterally from the dead tubules to the live ones, but the more usual phenomenon is to find the live tubules bordering the dead tract more lavishly supplied with lymph than the rest of the dentine. This condition is suggestive of the area of hyperæmia which surrounds a centre of irritation elsewhere in the body.

LESIONS GIVING RISE TO DEAD AREAS.

The lesions under which these dead tracts of dentine have been regularly observed are as follows:—

(*a*) Attrition; (*b*) caries; (*c*) the exposed necks of teeth; (*d*) experimental injuries of some weeks' standing; (*e*) hypoplasia of the enamel—but only when the hypoplasia has been sufficient to permit fluids from the mouth to permeate through to the dentine; (*f*) enamel lamellæ. Similarly here the occurrence of a dead tract depends on the permeability of the lamella to fluids from the mouth.

To summarize these lesions one may say that:—

Whenever the contents of the dentinal tubule are exposed to either chemical or physical irritation which is sufficiently severe to cause coagulation in the peripheral end of the tubule, that tubule will die right through to the pulp and be cut off from physiological contact with the pulp by a layer of secondary dentine.

Discussion.—Mr. G. NORTHCROFT (President) inquired why the author stated that patches of secondary dentine were not laid down as a reaction to injury. His experience was that, clinically, excessive sensitiveness usually disappeared beneath cervical caries, but he did not consider that this occurred so rapidly as Dr. Fish suggested.

Mr. A. LIVINGSTON believed that the vigour of the experiment caused a more severe injury than occurred in Nature, so that natural healing could not occur as it might do under attrition. As a result of this the odontoblast might have to react violently, with deposits of new dentine, instead of calcifying within the tubules.

Mr. K. HOOPER observed that his own teeth had undergone marked attrition for six or seven years, but had retained a high degree of sensibility throughout.

Mr. A. T. PITTS considered that there must be a collateral circulation which was sufficient to prevent death of the dentine; in children's teeth the pulp might be almost exposed, and yet death of the pulp did not occur. He inquired how Dr. Fish explained the hardness of translucent zones, and how arrested caries and the translucent zones in the roots of pyorrhœal teeth were accounted for by the author.

Mr. J. G. TURNER stated that he had not frequently found translucent zones present under caries. He had observed cases where the dentine under unlined amalgam fillings had remained acutely sensitive.

Dr. FISH (in reply) said that the old view was that the odontoblasts being irritated laid down secondary dentine, whereas, in his opinion, secondary dentine was laid down as a reaction to dead tissue. He believed that when a tubule was invaded it immediately died. He did not consider that arrested caries was harder than normal dentine, but thought it was harder than carious dentine. He believed if the speakers had cut as many sections as he himself had done that they would be convinced of the correctness of his views.

LEGENDS FOR ILLUSTRATIONS.

E, Enamel. *N*, Normal healthy dentine. *Cm*, Cementum. *P*, Pulp. *A*, Attrition.
C, Caries. *R*, Exposed dentine at neck of tooth. *D T*, Dead tract of primary dentine.
S, Secondary dentine. *L*, Enamel lamella. *F*, Filling. *E L*, Experimental lesion.
H, Homogeneous layer of primary dentine. H_1, Homogeneous layer of secondary dentine.
T, Terminal branches of primary dentinal tubules. T_1, Terminal branches of secondary dentinal tubules. *T Z*, Translucent zone.

FIG. 1.—Normal anatomy of origin of dentinal tubules under the cementum. The tubules take origin as fine branches separated from the cementum by a homogeneous layer of calcified tissue which acts as a barrier between the lymph in the tubule and that in the cementum.

FIG. 2.—Diagrammatic representation of dead tracts in dentine. The dead tracts are separated from the pulp by secondary dentine in every case. Dead tract shaded. Normal dentine unshaded. Caries cross hatched. Secondary dentine black. (*a*) Dead tracts under attrition at *A*, and under the exposed area at the neck of the tooth at *R*; (*b*) under mesial caries at *C*; (*c*) compound dead areas under mesial and distal caries at *C*, and attrition at *A*. Although in such a tooth the dead areas are very extensive and almost the whole crown appears dead in this mesio-distal section, in labio-lingual section it would appear as in fig. 2 (*a*).

FIG. 3.—Section of carious human incisor. Methyl-blue dye was placed in the pulp and left there for four weeks after extraction of the tooth. The dye had diffused into all the uninjured dentine which was thereby shown to be in physiological communication with the pulp. The tubules which were damaged by the caries and photograph black were devoid of dye, being sealed off from the pulp by the secondary dentine. They were therefore dead, but had not been calcified up, since if they had they would have come out white in the photograph. The presumption is, therefore, that they died suddenly without any reaction having occurred in them.

FIG. 4.—Experimental lesion *E L*, in tooth of monkey at surface of dentine caused by grinding away the enamel four weeks before extracting the tooth. The deposit of secondary dentine exactly corresponds to the lesion. Methyl-blue was placed in the pulp after extraction and diffused into all uninjured tubules, but the injured tubules were devoid of dye.

FIG. 5.—Higher magnification of part of fig. 7 (indicated by dotted lines). The dead primary tubules show degeneration of their contents and are devoid of dye. The tubules of the secondary dentine are however full of dye which was prevented from reaching the primary tubules by the homogeneous layer of lime salts H_1. Further evidence of the complete lack of continuity between the tubules of the primary, and those of the secondary dentine, is the fact that the tubules of the secondary dentine obviously take origin under the homogeneous layer in fine branches T_1 (compare fig. 1).

FIG. 6.—Dead tract (*D T*) in the primary dentine of a human temporary tooth with homogeneous layer of secondary dentine H_1, and new secondary tubules *S*. Compare with this dead tract the neighbouring dentine *N* which is uninjured and N_1 which is a later continuation of growth of this primary dentine. There is here no dead tract, no homogeneous layer and no secondary dentine. This is the appearance we should have under any peripheral injury to dentine if the injured dentine did not die but was merely irritated.

Fig. 7.—Translucent zones *T Z*, or patches of tubules fully calcified at their peripheral ends as a result of irritation spreading *laterally* from a dead tract. An uncommon observation.

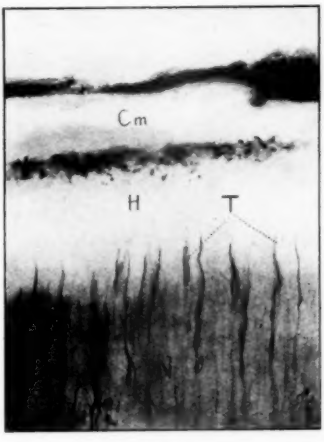

FIG. 1.

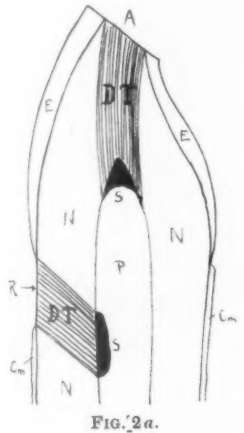

FIG. 2*a*.

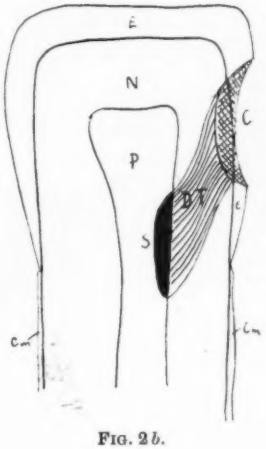

FIG. 2*b*.

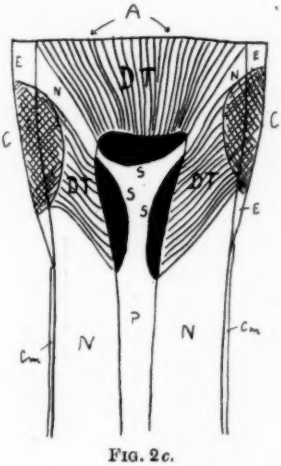

FIG. 2*c*.

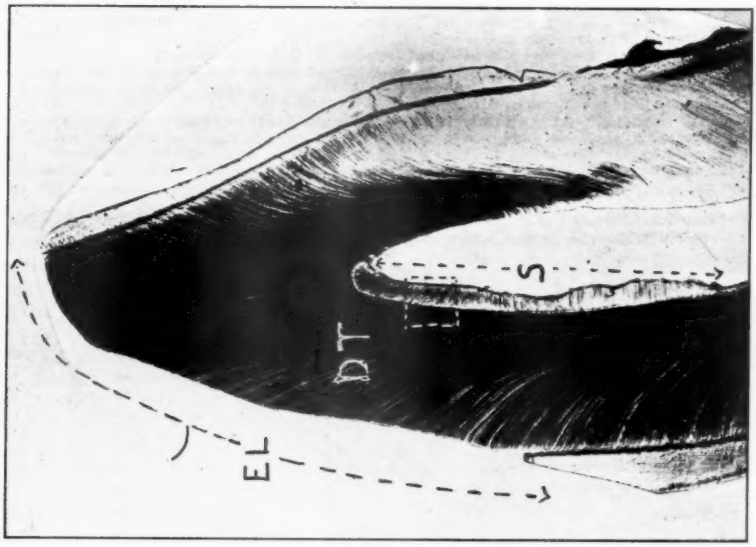

FIG. 4.

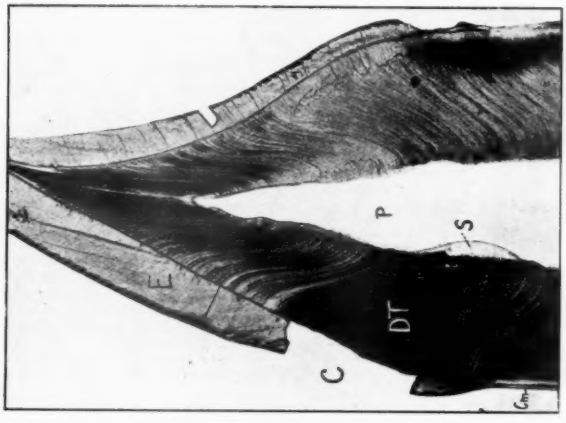

FIG. 3.

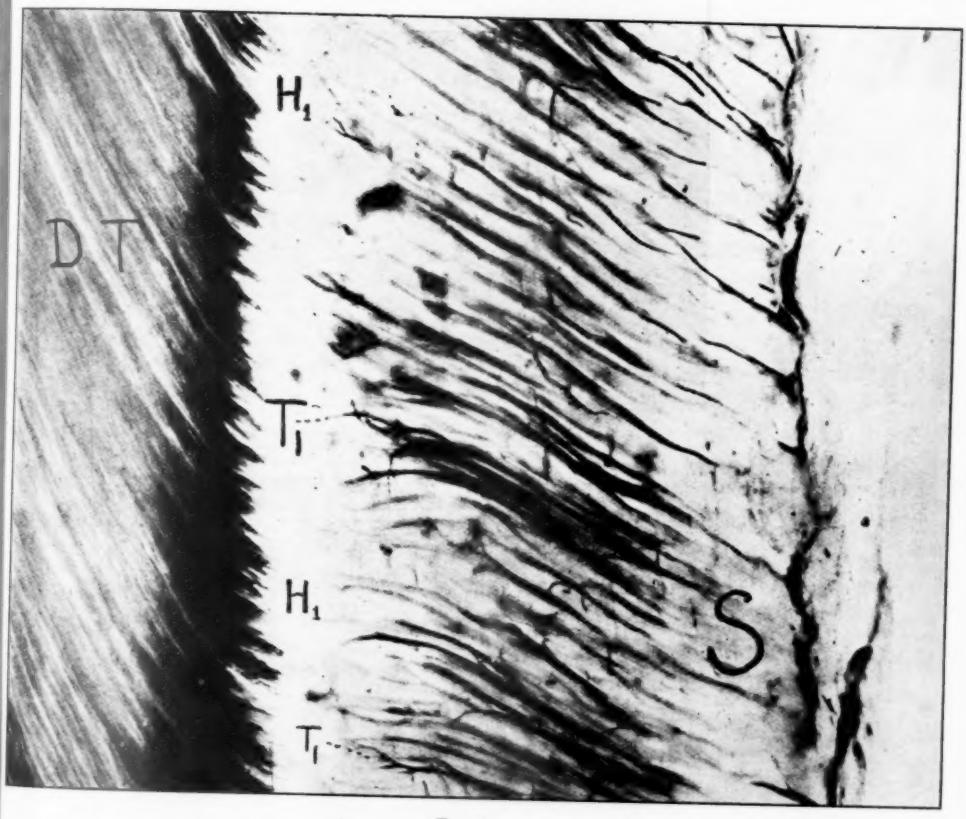

FIG. 5.

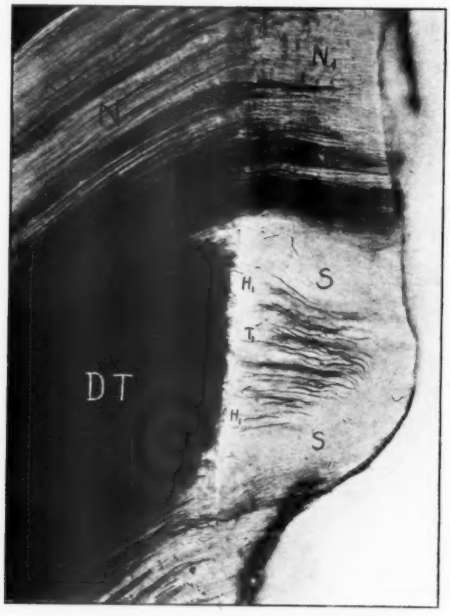

FIG. 6.

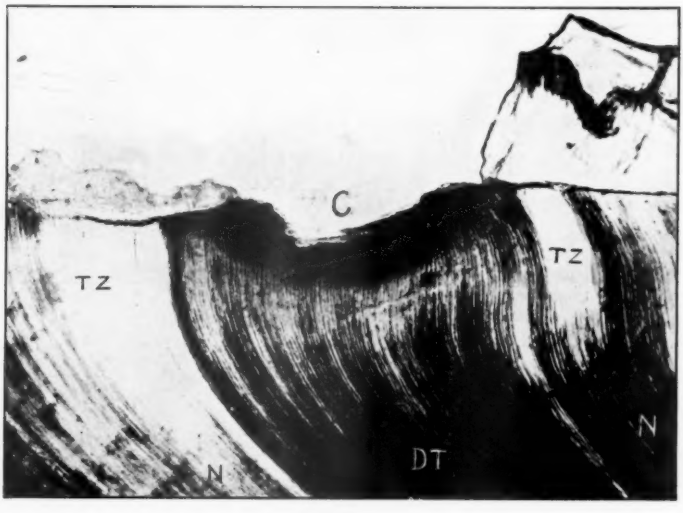

FIG. 7.

Variations and Abnormalities in the Position of the Teeth in Erythrocebus, the Patas Monkey.

By Sir FRANK COLYER, K.B.E., F.R.C.S.

THE Patas group of monkeys, formerly included in the genus Cercopithecus, is in the British Museum classified as a separate genus under the name Erythrocebus. In West Africa the genus is represented by the well-known Patas monkey (*Erythrocebus patas*), in the eastern side of the continent by a sub-species Pyrrhonotus or the Nisnas. A living example of the Patas monkey was exhibited for the first time in the gardens of the Zoological Society about the year 1834. It is a monkey which does fairly in captivity, and as a result is generally found in most Zoos. The Nisnas monkey (the eastern representative of the genus), according to the writer in the Royal Natural History, is frequently portrayed on Egyptian monuments and is undoubtedly the Cebus of the Ancients, which, on the authority of Pythagoras, was described by Ælian as inhabiting the Red Sea littoral. The genus is of interest to students of dental pathology on account of its tendency to vary and to develop marked irregularities in the position of the teeth.

The number of specimens examined consisted of:—From the wild state, 11; from the captive state, 38.

Variations of the Teeth.

(*A*) Absence of teeth. In one specimen the maxillary right second premolar was absent.

(*B*) Excess in the number of teeth. (*a*) A skull of a male Patas (B.M. 12.8.1.1.) has additional molars in the maxilla, the extra teeth being situated immediately behind the third molars. The right third molar is rotated and the left premolars occlude more forward than in the normal. (*b*) In another skull the left maxillary third molar was geminated with a supernumerary molar (fig. 1 *B*).

(*C*) Variations in the number of the roots. In two cases teeth, showing variations in the number of the roots, were present. (*a*) The maxillary premolars in monkeys have three roots, one internal and two external. In one specimen three of the premolars have a "web" of tissue uniting the three roots, and in the fourth tooth there is complete fusion of the roots (figs. 6 and 7).

(*b*) The second specimen shows numerous variations. The maxillary left first premolar has three roots. The maxillary left second premolar (fig. 2 *A*) shows an area of hypoplasia about one-third way up the labial aspect. The tip of the inner cusp (*a*) is well formed, but the distal aspect about one-third down is defective in formation and has a small root (*b*) springing from the area of hypoplasia. The three roots are fused together, the canals in the external roots are united, the canal of the internal root having a separate opening. The maxillary right first premolar has three roots, but they are, in the greater part of their length, united by a "web" of tissue. The maxillary right second premolar (fig. 2 *B*) has the roots more fused together than those of the corresponding tooth of the opposite side, to the distal aspect of the internal cusp (*a*) there is an extra cusp (*c*). The maxillary second molar (fig. 3 *A*) has an extra cusp at the antero-internal angle of the crown: the cusp has a root fused with the internal root. The postero-external and the internal roots are fused together. The maxillary right third molar (fig. 4 *A*) has only half of the roots formed; the pulp-canals of the three roots are fused together and form a "horse-shoe"-shaped cavity. The maxillary left first molar is normal in shape. The maxillary left second molar has a small projection on the crown at the position of the extra cusp in the right second molar. The postero-external and the internal roots are fused together (fig. 3 *B*). The maxillary left third molar, although not fully formed, has the three roots fused together (fig. 4 *B*).

The mandibular left second molar has the roots fused together, and the same condition is present in the right third molar (fig. 12).

Abnormalities in the Position of the Teeth.

The adult Patas monkey has a somewhat narrow palate and there is usually ample room posterior to the last molars. The incisors meet edge to edge, but in many of the specimens the maxillary incisors close a little in advance of the mandibular teeth. In three specimens the overlap was marked sufficiently to be regarded as an abnormality. Of the eleven specimens from animals in the wild state four exhibited deviations from the normal.

(*a*) *Erythrocebus pyrrhonotus whitei* (B.M. 11.1.13.1).—The incisors are irregular in position and there are spaces between the canines and the first premolars.

(*b*) *Erythrocebus patas* (B.M. 99.7.7.1).—The second premolars are rotated slightly and the first incisors are a little in advance of the second incisors.

(*c*) *Erythrocebus patas* (B.M. 11.6.10.2).—The maxillary second premolars are rotated and the mandibular second premolars are placed slightly oblique to the arch.

(*d*) *Erythrocebus pyrrhonotus whitei* (B.M. 13.8.18.1).—The left maxillary first premolar is rotated so that the internal cusp looks forwards.

The abnormalities are slight in degree and in marked contrast to those seen in captive animals. Of the thirty-eight specimens from captive animals, no less than twenty-five showed some degree of departure from the normal. The following cases will serve to illustrate the degree of irregularity that may be present. Fig. 5 shows the palate of a male animal. There is marked rotation of the maxillary second premolars and the teeth are in abnormal occlusion with the mandibular teeth. Considerable irregularity in the position of all the premolars is shown in the skull (fig. 6). The bones do not show any obvious pathological changes, with the exception of a slight roughening of the compact layer of the maxilla. On the right side (fig. 6) the maxillary first premolar is erupting in advance of its deciduous predecessor, the second premolar is in fairly correct position, the mandibular second premolar, almost fully formed, is low down in the bone and has erupted through the outer alveolar plate; on the left side (fig. 6) the maxillary first premolar has the crown directed downwards and backwards, the second premolar is rotated, the mandibular second premolar has appeared through the outer alveolar plate. The maxillary premolars, as mentioned previously, show variations in the form of the roots. There is a specimen in the Odontological Collection in which all four second premolars have pierced the outer alveolar plate in an almost horizontal direction (fig. 8).[1]

The skull (fig. 9) shows pathological changes in the bones. On the left side, as shown in the illustration, the following irregularities are present: In the maxilla the first premolar has erupted through the outer alveolar plate above the crown of the deciduous tooth, the second premolar, fully formed, is embedded in the bone and has the crown directed forwards, downwards and a little inwards, the third molar is high up in the tuberosity; in the mandible the second premolar has erupted through the bone below the roots of the deciduous molar, and the third molar is misplaced. A view of the right side of the skull (fig. 10) shows irregularity in the position of the maxillary premolars and the mandibular second premolar. In the mandible (fig. 11) each third molar is placed in an oblique direction and is some distance from the second molar; the bone of the mandible shows no obvious changes. The maxillary third molars in this specimen are high up in the tuberosity, the right tooth being geminated with an extra molar (fig. 1). Spaces are present between the mandibular right first and second incisors, the maxillary first incisors and the maxillary right first and second incisors. The maxillary second premolars are rotated, the mandibular right second premolar has erupted external to the line of the other teeth and is in malocclusion with the maxillary teeth, the mandibular left premolars have erupted external to the arch. Another specimen with the mandibular

1 Roy. Col. Surg. Museum, Odonto Col., G68.1.

third molars misplaced is shown in fig. 12. The left tooth is placed in a horizontal position with the roots embedded in the base of the coronoid process, the right tooth is tilted forwards. The maxillary premolars and molars in this specimen (fig. 13) are placed in a more oblique direction than in the normal, a condition seen in skulls showing protrusion of the maxillary teeth. There is a skull of a young female Patas monkey [1] in the Odontological Collection in which the third molars are malformed and the bones show evidence of imperfect formation. The third molars are misplaced, the mandibular second molars are tilted to a slight degree, and the maxillary second molars are rotated. In the thirty-eight captive specimens there were seven cases of misplaced mandibular third molars, and in five of these both teeth were irregular in position. It may be urged that the abnormalities to which reference has been made are attributable to captive conditions. Cercopitheques, Macaques, and other genera of monkeys are kept in captivity, and there are many skulls of these captive animals in our museums, but they do not, as a rule, show the "gross" irregularities in the same degree as Patas. The variations referred to in the early part of the paper are evidence that there is a tendency in Patas to vary, and the conditions of captivity appear to increase this tendency.

ILLUSTRATIONS.

FIG. 1.—*Erythrocebus Patas.* Maxillary third molars. *A*, right tooth, normal; *B*, left tooth geminated with a supernumerary molar.

FIG. 2.—*Erythrocebus Patas.* Maxillary second premolars, internal aspects. *A*, anterior; *P*, posterior; *a*, internal cusp; *b*, extra root; *c*, extra cusp.

FIG. 3.—*Erythrocebus Patas.* Maxillary second molars. *A*, right tooth, antero-internal aspect; *a*, antero-external cusp; *b*, antero-internal cusp; *c*, postero-external cusp; *d*, postero-internal cusp; *e*, extra cusp. *B*, left tooth, external aspect; *a*, antero-external root; *b*, postero-external root; *c*, internal root.

FIG. 4.—*Erythrocebus Patas.* Maxillary third molars. *A*, right tooth; *B*, left tooth.

FIG. 5.—*Erythrocebus Patas.* The maxillary second premolars are rotated.

FIG. 6.—*Erythrocebus Patas.* Right side showing irregularity of the premolars.

FIG. 7.—*Erythrocebus Patas.* Left side showing irregularity of the premolars.

FIG. 8.—*Erythrocebus Patas.* Roy. Col. Surg. Odonto. Col. G68.4. Misplaced second premolars.

FIG 9.—*Erythrocebus Patas.* Left side showing irregularity of the premolars and the mandibular third molar.

FIG. 10.—*Erythrocebus Patas.* Right side showing irregularity of the premolars.

FIG. 11.—*Erythrocebus Patas.* Mandible showing misplaced third molars.

FIG. 12.—*Erythrocebus Patas.* Mandible showing misplaced molars.

FIG. 13.—*Erythrocebus Patas.* Left side of skull showing irregularity in the position of the premolars and the molars.

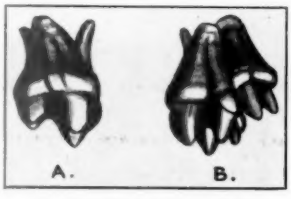

FIG. 1.

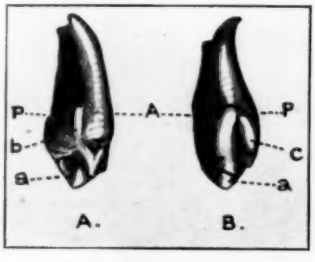

FIG. 2.

[1] Roy. Col. Surg. Museum, Odonto. Col., G68.15.

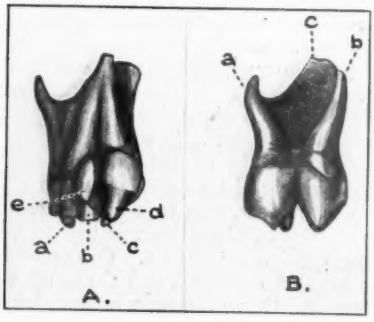

FIG. 3.

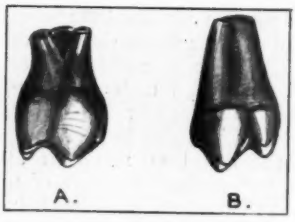

FIG. 4.

FIG. 5.

FIG. 6.

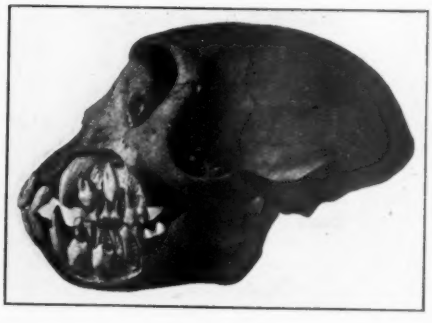

FIG. 7.

FIG. 8.

FIG. 9.

FIG. 10.

FIG. 11.

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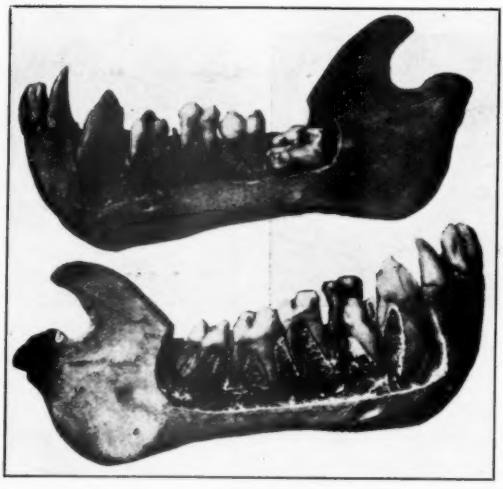


FIG. 12.

FIG. 13.

Section of Orthopædics.

President—Mr. NAUGHTON DUNN, M.B.

[*October* 2, 1928.]

PRESIDENT'S ADDRESS.

The Surgery of Muscle and Tendon in Relation to Infantile Paralysis.

By NAUGHTON DUNN, M.B.

ABSTRACT.—(1) The most important periods in the treatment of muscles weakened as a result of infantile paralysis are the acute illness, and if necessary a prolonged convalescence. (2) Division of tendons or muscles is seldom necessary for correction of deformity. (3) Successful transference of muscle power to a new insertion, given a good surgical technique, is dependent mainly on recognition of the group action of muscles. (4) Tendon transplantation should always be helpful, but will seldom by itself produce spectacular success in the restoration of function in infantile paralysis.

ABRÉGÉ.—(1) Dans le traitement des muscles affaiblis par la paralysie infantile, les périodes les plus importantes sont celles de l'affection aiguë et, au besoin, de la convalescence prolongée. (2) La division des muscles ou des tendons est rarement nécessaire pour la correction des difformités. (3) Étant donnée une bonne technique chirurgicale, le transfèrement du pouvoir musculaire à une nouvelle insertion dépend principalement de la récognition de l'action du groupe des muscles. (4) La transplantation du tendon devrait toujours être considérée utile, mais il est bien rare qu'elle produise *per se* le succès spectaculeux d'une restoration de fonction dans la paralysie infantile.

UEBERSICHT.—(1) Die wichtigsten Perioden in der Behandlung von Muskeln, durch Kinder-Paralyse geschwächt, sind die der akuten Krankheit selbst und—wo notwendig—der verlängerten Genesungsdauer. (2) Für die Mässigung einer Misgestalt ist eine Trennung der Sehnen oder der Muskeln selten notwendig. (3) Bei einer guten chirurgischen Technik ist eine erfolgreiche Uebertragung der Muskelkraft auf eine neue Insertion meistenteils von der Anerkennung der Gruppenarbeit der Muskeln abhängig. (4) Eine Verpflanzung der Sehne dürfte stets nützlich sein, jedoch wird sie allein selten den frappanten Erfolg einer Wiederherstellung der Funktion bei der Kinder-Paralyse darstellen.

IF we refer to a modern textbook on orthopædic surgery we find that all the principles on which I rely for the treatment of infantile paralysis are recorded, so that I have nothing new to offer. My only reason for selecting this subject for discussion is that so many alternative treatments are advised that a frank review of these and the results of our own practice and experience may be helpful.

It is probably his good fortune that a surgeon is rarely called on to treat a case of infantile paralysis from the acute onset throughout the convalescence to the final result. If he has this responsibility he would be well advised to tell the parents that, however careful the treatment during the acute illness and convalescence, incomplete recovery is probable. Unless the physician has the loyal coöperation of the parents in maintaining a line of treatment which we know to be surgically sound, he would be wise to leave the future in other hands.

We are dealing with an infection which primarily affects motor nerve centres, and it is with the part played by muscle in the causation of deformity and restoration of function that I propose to deal. The pathology of the disease is known, and the causes of deformity are understood, so that there is general agreement on the principles of treatment to be adopted during the acute illness and convalescence.

Treatment of Muscles during the Acute Illness.—During the acute illness, i.e., during the stage of tenderness, any other treatment of the muscles than that of rest, with prevention of postural deformities is generally recognized as harmful. The

spinal column and limbs should be immobilized in positions which prevent overstretching of the weakened muscles, and ensure that the position of the joints is that most useful to the patient, whatever the final degree of paralysis. The acute stage may last from one to six weeks.

Treatment of Muscles during Convalescence.— During convalescence control of treatment is more difficult ; the parents tend to form their opinion of the seriousness of the illness largely from the attitude of the child, and unless we can limit his activities, deformity is certain. The weakened muscles are like delicate plants in a greenhouse, which require all the care of a skilled gardener for their full development. The inflammation in the spinal cord has subsided, and the degree of ultimate recovery will largely depend on the treatment of muscles weakened or paralysed as the result of the pressure of the products of inflammation on the cells in the cord which control them. The positions of deformity must still be prevented by splintage, but the treatment of the muscles themselves is also of the utmost importance. Too often massage, electrical treatment and re-education are practised without a clear knowledge of the fact that the only muscles which require these are the paralysed or weakened muscles. It is a disservice to the patient to stimulate and encourage the use of the least affected muscles. We are trying to restore balanced muscular control of the joints; therefore the weak muscles must be constantly protected, nurtured and re-educated, strain and fatigue being avoided, while the healthy muscles are allowed to atrophy from disuse.

Assumption of the Erect Posture.—The tendency of the child may be to sit up or walk, and the question of when this should be allowed arises. The child must be kept recumbent or supported until we are satisfied that the spinal column can be maintained erect by muscular power; weakness of the gluteal muscles is also an indication for prolonged treatment in the recumbent position.

Not uncommonly, patients suffering from weakness of the spinal or gluteal muscles, require from six to twelve months' treatment before the upright position is allowed. Neither splintage nor surgery can mask the disabilities which follow weakness of these muscle groups, so their treatment with the patient recumbent should have special care.

The same principles of relaxation and treatment of the weakened muscles are more easily applied in the upper limb, knee, and foot. The abduction splint for the shoulder, the caliper splint for the knee, and simple splintage to support the foot, will usually prevent deformity and overstretching of the weak muscles, while moderate activity is allowed. I have found one exception to this—the gradual onset of calcaneo-cavus deformity of the foot if the calf muscle is seriously weakened; it is important to recognize this tendency before fixed deformity occurs. This compound deformity is insidious in onset, and I believe that in many cases, it can only be prevented by operation.

Splintage is therefore necessary in all cases of infantile paralysis, and the question of when splints may be dispensed with naturally arises. Splintage to prevent overstretching of the weaker muscles should be continued until there is evidence that further weakness of the affected muscles will not occur if freedom is allowed. They should not be suddenly discarded. The indication for the relaxation of splintage is the ability of the patient to maintain the limb in the position which splintage ensures. The range of movement may then be gradually increased so long as we are satisfied that the muscles are not weakening as a result of fatigue and overstretching. Splints to prevent deformity from overaction of stronger muscle groups, or to ensure stability, should be continued unless these tendencies can be relieved surgically.

The Correction of Established Deformity.—Our common experience is that we first see the patient long after the deformity is established. The child cannot walk, or walks badly, and increasing deformity is feared by doctor and parents. At no stage in the treatment of infantile paralysis are more patience and judgment necessary.

We cannot carry out all the text-book methods for correcting deformity. Surgically, we can quickly correct many of these, but we must realize that our final result may depend largely on the method we adopt. Two things are certain: (1) muscular control of the limb is already deficient, and (2) divided tendons and muscles quickly undergo further shortening and atrophy. I therefore submit as a principle that division or elongation of a tendon or muscle to correct deformities in infantile paralysis should be resisted unless we are satisfied from our experience:—

(1) That correction of deformity will improve function.
(2) That division or elongation of tendons is necessary to correct deformity.
(3) That the power of the shortened muscle or tendon could not be utilized by tendon transplantation at the time of operation.

I will refer to the common deformities following infantile paralysis, and my practice in their treatment.

Scoliosis.—We have emphasized the importance of prolonged treatment in the recumbent position during the acute illness and convalescence in paralysis affecting the spinal and gluteal muscles. Not uncommonly both these, with or without paralysis of the respiratory muscles, play a part in the rapid production of the more severe cases of spinal deformity. We can correct this deformity, or at least centralize body weight, by means of plaster of Paris jackets, etc., but we must realize that subsequent deformity will be more severe unless we provide for prolonged mechanical support and treatment of the affected muscles, or adopt one of the methods of surgical fusion of the vertebræ practised by Hibbs, Mackenzie Forbes, or Albee.

Theoretically, spinal fusion appeals to me for the treatment of many of these cases, but the mortality even in the hands of experts is high—Hibbs [1], 3·9 per cent.; Mackenzie Forbes [2], 10 per cent.

My personal practice is to depend on gradual correction of deformity and simple splintage to maintain centralization of body weight, while treatment to develop the weaker muscles is continued.

Lordosis.—This is usually secondary to weakness of the abdominal muscles, and/or flexion contracture of the hip-joints. In the former case a closely fitting corset will largely replace the function of the abdominal muscles.

Treatment of Flexion Contracture of the Hips.—This is the first point on which, I think, discussion may be useful. We all recognize that fixed flexion contracture of both hips is one which must be overcome before the upright position is possible. This deformity in bedridden patients is frequently associated with contracture of the knees and equinus deformity of the feet. What method shall we adopt to correct the deformity?

In the text-books (Whitman, Jones and Lovett, etc.) gradual correction of deformity of the hips is mentioned, but it certainly is not emphasized; in fact the only illustrations in Jones and Lovett are of cases in which deformity has been corrected by operation. The operations advised and illustrated are those recommended by Soutter [6] and Campbell [7]. By these operations we do at least two things—we divide and so weaken the tensor fascia femoris—and we detach a large portion of the origin of the gluteus medius. Soutter and Campbell both recognize that division of the psoas muscle and the anterior portion of the capsule of the hip-joint may be necessary in the severer cases.

Whichever operative procedure is adopted, complete immediate correction of the deformity is seldom possible or wise, so that we must be familiar with the methods of gradual correction of deformity in order to obtain full extension of these joints. The text-books which give prominence to these operations also tell us that no splintage can mask the limp resulting from paralysis of the gluteus medius muscle. Legg's operation of transplantation of the tensor fascia femoris muscle is the only one recommended to replace its function.

I agree that the operations advised may reduce the time necessary for the correction of flexion contracture of the hip-joint, but I submit that the contracture

resulting from infantile paralysis is the one in which they should seldom be employed.

The prominence given to operative methods in the text-books makes me feel that there is a real risk of our specialty becoming too surgical. We must realize that methods of correcting these deformities were successfully practised before these operations were surgically safe, and we can only assess the value of modern methods if we have experienced the possibilities of correction of deformity by gradual splintage.

Gradual Correction of Deformity.—It is my good fortune to have been trained in a school in which success in the application of the principles of gradual correction of deformity has been very striking. I have learned that Nature resents tension, and gradually allows relaxation of structures to which it is constantly applied. My experience is that operation is seldom necessary to correct flexion contracture of the hip in infantile paralysis. Tenotomy of the fascia I sometimes make a preliminary to gradual correction of deformity—but only in very severe old-standing cases would I consider a major operation.

During the past eighteen years I can only recollect once having anæsthetized a patient for correction of flexion contracture of the hip-joints resulting from infantile

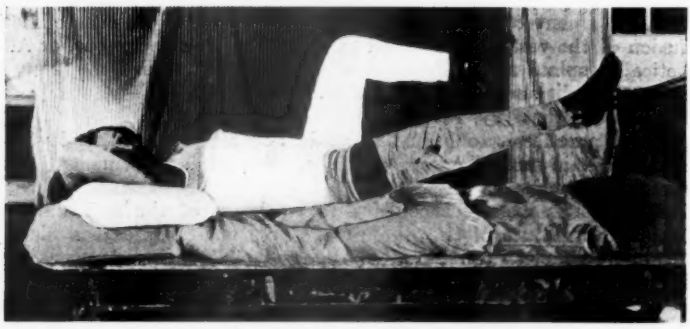


FIG. 1.—Dame Agnes Hunt's method of gradual correction of flexion contracture of the hip in infantile paralysis.

paralysis, and in this case the ilio-femoral ligament was found to be the main obstruction to correction.

When there is marked spinal lordosis, the method used by Dame Agnes Hunt at the Shropshire Orthopædic Hospital is valuable (fig. 1.)

The principles of this method are:—

(1) The correction of lordosis by flexion of both hips while the spine and one flexed hip are immobilized in plaster of Paris.

(2) The free limb is then fixed in a Thomas knee-splint which is gradually lowered as the tight structures yield to extension and gravity.

(3) The limb in which deformity has been corrected is then incorporated in plaster with the spine, while the other limb is gradually extended.

The illustrations (fig. 2, pp. 6, 7), show the result of the application of these principles in the correction of severe contracture of the hips, knees and feet. I personally took no part in the correction of deformity in this case. Had the result been one of surgical treatment I would have had reason to be proud. The case demonstrates the value of a staff fully trained in the application of the principles of gradual correction.

Deformity of the Knee.—Here again, good leverage is available, and any muscle-shortening is usually rapidly overcome by simple splintage. In severe old-standing cases of flexion contracture of the knee in adults, where arthrodesis has been decided on, division of tendons is not itself sufficient to allow correction of deformity. The most resistant structure is the posterior ligament of the joint.

Forcible Correction of Deformity.—This method is perhaps best illustrated in the treatment of cavus deformity of the foot. Here the Thomas wrench supplies the leverage. The value of this instrument in correcting severe degrees of established deformity of the foot must be witnessed to be appreciated, and this is necessary in order that we may assess the relative advantages of other methods.

My experience has been that after division of the plantar fascia and short muscles near their attachment to the os calcis, by a tenotome, complete over-correction of cavus deformity can usually be obtained by the use of the Thomas wrench in children and young adolescents.

I therefore seldom practise the stripping process devised by Steindler.

Tenotomy and Tendon Lengthening.—These operations are seldom necessary for the correction of deformity of the hip and knee; in the case of deformities of the foot, the temptation should still be resisted. A shortened tendon is usually an active tendon, and muscle power being already deficient, it is a safe general rule in infantile paralysis never to divide a tendon for correction of deformity alone. The only tendon I sometimes elongate is the tendo Achillis, but even this simple operation is not to be undertaken lightly. A slight degree of shortening of the tendo Achillis may have advantages in infantile paralysis, e.g., in cases of quadriceps insufficiency, when weight is borne on the limb, the strain on the gastrocnemius muscle locks the knee-joint and so increases its stability. In cases of simple cavus deformity of the foot, the shortened tendon acts as an anchor which allows true correction of this deformity at the mid-tarsal joint. In extreme equinus, elongation of the tendo Achillis may be necessary to allow relaxation and recovery of the weakened anterior tibial muscles, it should be elongated just enough to allow the right-angled position of the foot. Walking with the foot fixed in this position will ensure, when the plaster is removed, the slight dorsiflexion desirable for complete recovery of function. In many cases of deformity of the foot in infantile paralysis, fusion of the tarsus in a good weight-bearing position will be desirable to ensure stability of the foot. In these cases the tendo Achillis should always be elongated, if necessary, just enough to allow the normal weight-bearing relationship of the leg bones with the astragalus and os calcis [5].

Having corrected deformity, a suitable period of splintage and treatment of the weak muscles in relaxation is necessary before the final degree of recovery can be established. The question then becomes one of whether by surgical means we can improve function and reduce splintage.

The practical methods at our disposal are operations on bone to increase stability, tendon transplantation and tenodesis. The first lies outside the scope of this discussion, but we must remember that in most cases, especially in the case of the foot, operations on bone may be an essential preliminary to tendon transplantation, tenodesis, or all that is necessary to provide the best functional result.

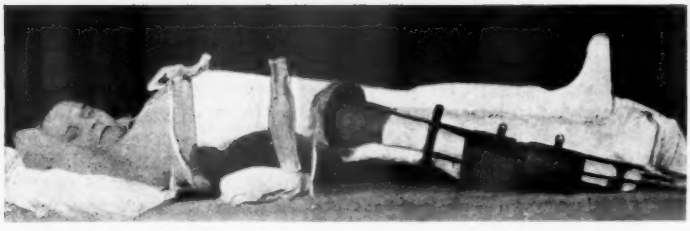
Tendon Transplantation.—The literature of tendon transplantation is so confused that it easily explains our failures and makes more worthy of publication the methods on which we depend for success.

Historical.—The literature on tendon transplantation, or tendon anastomosis, as it was called in the earlier days, contains much of interest. Although credit for the application of the principle to paralytic deformities undoubtedly belongs to Nicoladoni, who published in 1881 the operation of transference of active peroneal muscles to the tendo Achillis for a paralytic calcaneus deformity, we find that transference of power from one muscle to another by tendon anastomosis had been practised in traumatic surgery more than a century previous to this. Malgaigni, in his lectures in

I

I

II



III

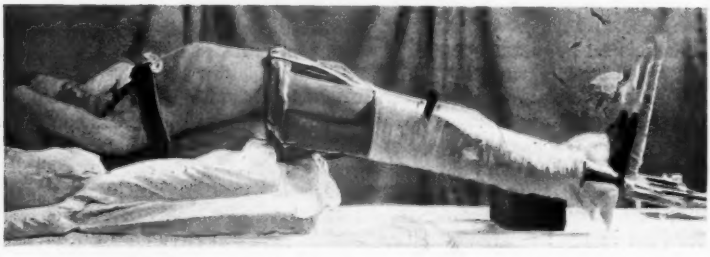


FIG. 2.—To illustrate result obtained by gradual correction of deformities following infantile paralysis. Duration of treatment, 1 year. No operations.

IV*a* IV*b* IV*c*

FIG. 2 (*continued*).

"Orthopædic Surgery," 1862, and Velpeau, in his "Operative Surgery," 1839, alike recommend that severed tendons be grafted upon intact adjacent tendons.

Probably the first surgeon to graft a tendon was Missa; the case published in the *Gazette Salutaire*, 1770, No. 21, was one in which the peripheral end of a divided extensor tendon of the middle finger was successfully grafted upon the tendon of the ring finger.

J. Hilton Waterman, in an address to the surgical section of the Buffalo Society of Medicine in 1902, reviewed the history of the evolution of tendon transplantation and its position at this time. He stated that at least six surgeons since 1881—the date of Nicoladoni's operation—appeared to have proposed tendon transplantation as a new and unheard-of phase of surgical intervention.

Tendon transplantation, therefore, appears not to differ from other surgical innovations in that it has been practised by an individual, forgotten, and rediscovered, perhaps many times, before it becomes a generally accepted surgical advance. The detail and application of the procedure then became a matter of controversy, and this seems to be the position of tendon transplantation to-day. It is nearly half a century since the operation was first utilized in a case of infantile paralysis. During this time successes and new methods have been reported, but the fact that its growth in popularity has been spasmodic rather than steady, makes one feel that neither the indications for the operation, nor the principles which should guide us in performing it, are yet generally accepted.

In reviewing the successes published up to the end of the nineteenth century, the most striking point is that in the lower limb they are all cases of transference of muscle power to an adjacent muscle, and in each case this has been a muscle normally in action with the one transplanted. The only success reported in

which a flexor had been substituted for an extensor, in infantile paralysis up to this time is transference of the flexor carpi ulnaris to the extensors of the fingers in a case of complete radial paralysis. It is also of interest that before the close of the nineteenth century tendon transplantation had been utilized by Franke in the treatment of spastic paralysis, and the operation of direct muscle transference had been established by Goldthwaite, who advised anastomosis of the sartorius muscle with the quadriceps extensor, in cases of paralysis of this muscle.

As a result of the publications of Vulpius, Lange and Codivilla, tendon transplantation became more generally known and practised at the beginning of the present century. Lange emphasized the importance of the direct periosteal implantation of the severed tendon, while Vulpius advocated tendon-to-tendon suture.

The operation became popular, surgical asepsis made it safe, and before the European war, as a result of the extensive experience of many surgeons, the following principles for success in the operation were established:—

(1) That previous to operation, correction of deformity should be complete.

(2) That the transplanted muscle should run in a direct line from its origin to its new insertion.

(3) That the muscle should be of sufficient power to meet the strain which would be imposed on it.

(4) A recognition of the fact that where (3) was not practicable, transference of muscle power could still be advantageously utilized as an adjuvant to other procedures such as tenodesis and arthrodesis.

Failures were, however, still common, and these were ascribed mainly to adhesions and faulty union at the point of insertion. The operation generally became less popular, and more limited in application, while the explanation of failure became the subject of investigation in many research laboratories and surgical units.

The literature of the present century illustrates the two phases through which the operation of tendon transplantation has passed. During the first it was popular, and subjected to intensive experiment; Vulpius devised at least 116 operative procedures of tendon transplantation for the treatment of paralysis in the foot alone. During the second phase, research into the anatomy and physiology of tendons and muscles has helped to explain failures and successes, while the extension of other methods, notably tenodesis and arthrodesis, has more clearly defined the limitations of the usefulness of tendon transplantation alone.

Anatomy and Physiology of Tendons.—Normal tendon is surrounded by loose, fatty, areolar tissue, and usually where it passes under ligamentous structures a special sheath is developed. This consists of fibrous tissue separated from the tendon by two layers of synovial membrane, one lining the sheath, the other covering the tendon, except at the posterior surface where the synovial membrane lining the sheath is reflected on to the tendon. This forms a loose, delicate membrane —the mesotendon—connecting the floor of the sheath with the tendon; great importance has been attached to it because it conveys the main vascular supply of the tendons. It is also to be noted that the synovial membrane is plicated at each end of the sheath, which is Nature's method of allowing free play of the tendon within the sheath, without rupture of the synovial membrane.

Biesalski, Mayer, Steindler and Bernstein are amongst the main contributors to the modern literature which deals largely with the problem of the prevention of adhesions and the maintenance of nutrition of transplanted tendons. Their views are based both on clinical experience and experimental research. Lange and his assistants also produce further evidence as to the value of silk as a means of the transference of muscle power, parchment being used to prevent adhesions at points where these are likely to form.

The advances claimed by these surgeons are mainly illustrated by operations on tendons controlling the foot, and we may profitably confine our discussion almost entirely to this problem.

We may summarize the result of modern investigations as follows:—

Tendon Sheath Exchange.—The method of tendon sheath exchange has been developed by Biesalski [6] and Mayer [7]. The principles are transference of the transplanted tendon to its new insertion through the sheath of the tendon it is replacing, and avoidance of adhesions by careful suture of fascia where the tendon crosses intramuscular planes. The operation necessitates rupture of the synovial membrane of the tendon sheath and the mesotendon of both tendons; it also sacrifices the tendon which supplies the sheath. Mayer noted later that it was not necessary to withdraw the paralysed tendon, as he found the sheath large enough to accommodate both tendons.

Steindler [8], while recognizing the advances of Biesalski and Mayer, notes that in two cases in which he had to re-operate (both were cases in which the peronei had been transposed to replace the tibialis anticus) the transplanted tendon, while it had not broken loose from its attachment, had degenerated into a fine, insignificant bundle of fibres, incapable of producing any mechanical action. He attributes these degenerative changes to faulty nutrition, and points out that tendons generally receive very little blood-supply from central vessels, that small vessels penetrate into the tendon at the point of insertion and from the muscle substance, but that for a large distance most tendons are dependent for their nutrition on small vessels passing to them through the mesotendon. He therefore advises that this structure be not disturbed at operation. Steindler recognizes that preservation of the mesotendon necessarily restricts the field of successful tendon transplantation.

Bernstein [9] reports his experimental results on three methods.

(1) The Lange method. Subcutaneous transposition of a tendon to a new periosteal insertion.

(2) The Biesalski-Mayer method. Transference of a tendon to its new insertion through the sheath of another tendon.

(3) His own method. Transference of the tendon together with its sheath and peritendinous structures.

As a result of histological findings, which are fully illustrated, he states that there is less nutritional disturbance of the tendon and less likelihood of adhesions when the sheath is transferred with the tendon. He illustrates transference of the peroneous longus, together with its sheath, to replace a paralysed tibialis anticus, and transplantation of the extensor longus hallucis and its sheath in a case of peroneal palsy.

These writers also draw attention to the importance of avoiding strangulation of the tendon by suture material.

Although the literature on tendon transplantation has dealt mainly with methods of restoration of functional control of the foot, certain advances have been claimed for muscle or tendon transplantation for the restoration of stability and function in the hip, knee and elbow, while the war provided us with an unusual experience of treating paralysis in the upper extremity by transference of muscle power to a new insertion.

Transplantation Methods at the Hip-joint.—The only published methods available for paralysis of the gluteal muscles are those advised by Lange [10] and Legg [11].

Methods of Lange.—Transference of power of an active vastus externus to replace or reinforce the gluteus medius and minimus. The operation consists of detaching the vastus externus from its attachment to the upper part of the femur and reattaching it by a row of plaited silk sutures to the crest of the ilium. Lange reported success when the vastus externus was sound, but found that it also frequently shared in the paralysis. This difficulty he has overcome by two methods:—

(1) Transference of power by means of silk ligaments from the erector spinæ of the same side to the great trochanter, and

(2) Transference of power from the latissimus dorsi of the sound side to the femur of the paralysed side.

Legg's Operation.—This operation is advised by Legg where the gluteus medius and minimus are weak and the gluteus maximus is intact. It consists of direct transference of the power present in the tensor fascia femoris to an insertion in bone just below and behind the great trochanter.

I have not had experience of these operations. My general surgical experience has been that living tissues, whether bones, joints, nerves or tendons, frequently demonstrate a dislike to the presence of foreign non-absorbable material. Lange claims that by special treatment of the silk this difficulty has been overcome.

I am sure Legg's operation would be effective in the cases in which he recommends it: partial paralysis of the gluteus medius with an active gluteus maximus, and tensor fascia femoris. Unfortunately these cases are rare. The possibility of this operation, however, emphasizes the importance of the preservation of the gluteus medius and tensor fascia femoris in correction of flexion deformity of the hip.

Tendon Transplantation at the Knee.—The operation commonly advised and practised for quadriceps insufficiency is transference of the biceps, and perhaps one of the inner hamstrings, to a new insertion in the patella. I have had long experience of this operation but I have never been satisfied that the return of any active voluntary extension of the knee-joint was due to the action of the transplanted muscle. I believe, however, that its tonic contraction gives increased stability to the knee when weight is borne on the limb.

The only operation which I practise for quadriceps insufficiency is transference of either the tensor fascia femoris or the sartorius muscle to the patella [15]. Either or both of these muscles are naturally in action in extension of the knee, and the transference of their insertion to the patella only renders their power more effective.

The case I demonstrate shows extreme hypertrophy of a tensor fascia femoris which has been transplanted. When the knee is voluntarily extended, the contraction of this muscle and the tight subcutaneous cord passing to the patella clearly demonstrate that this muscle is responsible for the action. The operation again illustrates the importance of preserving the tensor fascia femoris in correcting flexion deformity of the hip.

Tendon and Muscle Transplantation in the Upper Extremity.—Shoulder.—No transference of muscle-power has been established as an effective substitute for a paralysis of the deltoid muscle. We therefore usually depend on arthrodesis of the shoulder in abduction when the indications for this operation are present. These are, (1) good control of the scapula, and (2) useful function in the hand.

Elbow.—Steindler has reported successful transposition of the insertion of the triceps muscle to the front of the elbow in a case of flexor paralysis, and has introduced the method of transference of the common origin of the flexors of the wrist and hand to a higher level in the humerus, so that they might act more effectively as active flexors of the elbow-joint.

Tendon Transplantation of the Wrist and Fingers.—The war gave many of us extensive experience of tendon transplantation in the restoration of function. The main value of this experience was that we could rely on two constant factors: (1) The muscles, whose function we replaced, were definitely completely paralysed, and (2) the muscles transplanted were physiologically normal. Certain operations became standardized, and our results improved as we became more familiar with the surgical details. These cases were, however, frequently complicated by stiffness of the joints. Contracture may be present in infantile paralysis, but seldom the limitation of movement due to trophic and inflammatory changes in the joints and muscles. In both conditions free restoration of passive joint movement is an essential preliminary to success. Although many new operations were evolved, they

were all essentially the application of common principles. In musculo-spiral paralysis we found that loss of extension of the wrist, fingers and thumb, could be surely replaced by transference of the pronator radii teres, flexor carpi radialis, and flexor carpi ulnaris to the extensors of the wrist, fingers and thumb. I have also had success in two cases by transferring active radial and ulnar extensors to the common and deep flexor tendons when these muscles have been completely paralysed. Apart from these two group transplantations success was generally more certain if we depended on one of the same group for the transplantation. This is well illustrated in paralysis of the short extensors of the thumb. If we replace them with portions of the radial extensors of the wrist recovery of function is natural, abduction of the thumb being a movement closely and commonly associated with dorsiflexion of the wrist.

Paralysis of the intrinsic muscles of the hand results in loss of power of normal flexion of the metacarpo-phalangeal joints in closing the hand. Sir Harold Stiles overcame this by attaching slips of the flexor profundus tendons to the lateral border of the proximal phalanges.

Steindler [13] makes an ingenious application of a similar method in paralysis of the thenar muscles. The loss of the opposing action of the thumb seriously incapacitates the hand. Steindler replaces the action of the opponens pollicis by splitting the deep flexor tendon of the thumb and anchoring one half dorsally to the base of the first phalanx.

Tenodesis.—The operation of utilizing tendons as ligaments to limit joint movement has a definite value in infantile paralysis. The principle was recognized by Tilanus, Codivilla and Vulpius, and has been elaborated by Gallie [14]. Gallie in his first publication advocates embedding of the entire tendon in bone for varus, valgus, and calcaneous deformity. In a later [15] publication he indicates the advantage of implanting only a portion of the tendon in bone, so that any recovery in muscle power may still be available while it is protected from strain. He illustrates fixation of a portion of the tendo Achillis to the tibia in a case of calcaneus deformity. This is an operation which I have practised for many years, and it is one which I greatly value.

Personal Experience of the Surgery of Muscle in Infantile Paralysis.—I have emphasized the importance of complete rest and special treatment of the weak muscles for a prolonged period, because we know from experience that only by this means can we prevent deformities and give the weak muscles their best chance of recovery. With regard to correction of established deformity, I have shown that shortened muscles are usually active muscles, and that it is very seldom necessary to elongate or divide them in order to correct deformity. I made an exception in the case of the tendo Achillis, as I am satisfied that elongation of this tendon is sometimes necessary, but it is an operation which should be undertaken only after such consideration as would be given to one of the major operations of surgery.

General Principles.—Having corrected deformity, and treated muscles until we are able to estimate the likely degree of recovery, sound surgical judgment is necessary before we decide on how the power present may best be used in the restoration of muscle balance.

Unfortunately we cannot consider this as an isolated problem, we must consider alternative methods. For example, in the foot, arthrodesis of the tarsal joints may be utilized to maintain stability of the foot, and allow tendon transplantation or tenodesis to be more effective in their control of the ankle-joint. The degree of paralysis varies so much that it is difficult to make rules, but my experience is that in all but the milder cases tendon transplantation and/or tenodesis as applied to the foot are more usefully employed after stability of the tarsus in a good weight-bearing position has been assured by other means.

Having decided that tendon transplantation with or without other procedures is advisable to restore muscular balance, we must consider which tendons we shall

transplant and how we will transplant them. All are agreed that for success the operation calls for perfect asepsis, that the tendons should run a direct course to their new insertion, and that they should not be subjected to strain until their union to the receiving tendon or bone is established. Although adhesions would vitiate success, and every care should be taken to avoid them, I do not agree with many modern workers that this, or disturbance of the nutrition of the tendons, was the main cause of failure in the earlier operations.

On looking back on a fairly considerable experience of tendon transplantation I am clear that success or failure has depended mainly on two other factors: (1) the question of whether the transposed tendon would naturally or could easily be re-educated to perform its new function, and (2) the degree of tension under which the transposed tendon was sutured. The constancy of success in certain operations and the certainty of failure, in others suggest that an axiom in tendon transplantation in the lower extremity should be that a tendon, or part of a tendon, should only be used to replace one of its own group, i.e., one normally in action with it. I do not assert that an individual tendon cannot be trained to function in a group with which it is not normally in action, but for this, special attention to re-education and the active coöperation of an intelligent patient are necessary. These desiderata are not always possible, so that I prefer to confine myself to operations in which they are not essential.

The re-education of an individual tendon to act apart from its group appears to be more practicable in the upper extremity, where the action of individual muscles is more specialized. The movements employed in locomotion are, however, simple and controlled by groups of muscles, so that it is more difficult to re-educate an individual muscle to act apart from its own group. We therefore find that in the lower extremity the limitations of successful tendon transplantation are more clearly defined. I have already referred to my experience of failure of a transplanted biceps muscle to control active extension of the knee in cases of paralysis of the quadriceps.

When we come to paralysis of the foot the evidence in favour of this axiom is more striking. There are here, for practical purposes, only two groups of muscles—one group the anterior tibial, in action in dorsiflexion of the foot; the other the peroneal and posterior tibial muscles, associated in action with the tendo Achillis in plantar flexion. We find that any of the anterior tibial muscles may successfully replace one another, e.g., the tibialis anticus may be transplanted to the outer side of the dorsum of the foot, or the active power of the extensor longus hallucis may be transposed to the tibialis anticus. On the other hand, no tendon transplanted from the peroneal or posterior tibial groups will be effective in replacing the loss of active dorsiflexion. I emphasize this point because one of the tendon transplantations most commonly illustrated and advised is that of transposing an active peroneus longus to replace the tibialis anticus.

Just as in the case of the transference of the biceps at the knee—its tonic action and the fact that we have weakened a group which was stronger than its opponents may justify the operation as a means of increasing stability of the foot—the cases in which it functions as an active dorsiflexor of the foot in infantile paralysis must be very rare.

The posterior tibial and peroneal muscles form one group. All are in action in extension of the ankle, so that they may be used to replace one another with certainty of success. The successful use of active posterior tibial and peroneal muscles to replace a paralysed tendo Achillis is a striking illustration of the principle of the group action of muscles in the control of the foot.

Having decided which muscles to transplant, the question of technical detail arises. We wish to avoid adhesions, and to interfere as little as possible with the nutrition of the tendons. The degree of tension under which the tendons should be sutured, and whether their point of attachment should be tendon, periosteum or bone,

are still matters of argument, and will only be established to the satisfaction of the individual by his own experience. Personally I suture tendons under considerable tension, and do not consider it essential even in the foot that the point of insertion should be periosteum or bone. In utilizing a tendon, or portion of a tendon, as a ligament, I always pass it through a hole bored in the bone. This allows the tendon to be sutured to itself and gives greater security.

Cases of infantile paralysis vary so much, not only as to the muscles which are paralysed, but as to the degree of paralysis present, that no surgeon can legislate for the general case. I therefore confine myself to my own method of approach to one or two of the familiar problems.

We might refer to the common types of pes cavus.

(1) *Pes Cavus.*—Fixed deformity—no lateral instability of the foot—tibialis anticus active.

Treatment.—Division of plantar fascia, etc., over-correction by wrenching—fixation of the foot in plaster of Paris in the correct weight-bearing position for two months.

This type of case is not uncommon and is probably due originally to postural deformity.

(2) *Pes Cavus.*—Only slight power in the tibialis anticus muscle, and perhaps some shortening of the tendo Achillis—all other muscles good.

Treatment.—Complete over-correction by means of the wrench as in case (1), division of the toe extensors, and very slight elongation of the tendo Achillis if necessary—tendon fusion of the anterior tibial muscles, the foot being fixed in plaster at a right angle in inversion for two months.

(3) *Pes Cavus.*—Paralysis more severe, with laxity of the tarsal joints.

In this case I would consider arthrodesis of the tarsal joints in a normal weight-bearing position an essential preliminary to tendon fusion of the anterior tibial muscles.

Tendon Fusion of the Anterior Tibial Muscles.—This is an operation which I frequently use in preference to transplantation of an individual muscle in cases of weakness of the anterior tibial group. It consists of exposure of all the anterior tibial tendons above the ankle—the fascia covering them being cleanly dissected away; opposing tendinous surfaces are incised and the inner surface of the tendon exposed by dissection. The tendons are then united to one another by suture—the tension on individual tendons varying according to the previous tendency to inversion or eversion. This method obviates the necessity of the technically difficult operations designed to maintain integrity of the tendon sheaths and the nutrition of tendons. It is simple in performance and effective. No muscle power is sacrificed, and the nutrition of the tendons is not disturbed.

Calcaneo-cavus Deformity.—I have already remarked on the difficulty of preventing the insidious onset of calcaneo-cavus deformity by splintage. In these cases I frequently operate early—either fusing the peroneus longus and flexor longus hallucis to the tendo Achillis, or utilizing a portion of the tendo Achillis as a ligament to maintain a slightly equinus position of the foot in weight-bearing.

In established calcaneo-cavus deformity I depend on operations on bone to correct deformity and stabilize the foot, while all active posterior tibial and peroneal muscles are transferred to an insertion in the tendo Achillis. If power is deficient in these I utilize the additional method of tenodesis to maintain the position of slight equinus.

This illustrates the utilization of three methods in the treatment of one common deformity following infantile paralysis. I believe that it is by a correct assessment of the value of one, two, or all methods, that we will achieve the greatest success.

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Discussion.—Mr. T. H. OPENSHAW said that he had been pleased to hear the President say he did not expect the tibialis posticus to become re-educated so as to act as an extensor of the ankle-joint, or the peroneus longus to become an invertor. His own experience coincided with that view. But transplantation of the peronei corrected the valgus deformity, and therefore improved the condition of the ankle and of the foot.

He had also been pleased to hear the President say that the transference of the biceps into the quadriceps was not a procedure he believed in. Mr. Dunn was not apparently so emphatic about the sartorius muscle.

He himself was unable to say when the quadriceps ought to be operated upon and transplanted into, because the muscle sometimes recovered its power after a lapse of many years. He had at least twelve patients who had been wearing splints for a number of years —the longest time was twenty-three years, and the others eight years and longer—with subsequent recovery of power in the quadriceps.

Mr. R. C. ELMSLIE said that the subject was so difficult and diffuse that they must try to arrive at certain general principles, and he felt that Mr. Dunn *had* arrived at those principles.

One point, emphasized by the President, was that the various methods of tackling these problems must be suitably combined; in every case it was desirable to draw up a programme, utilizing the methods of arthrodesis, tenodesis, and tendon transplantation so as to achieve the best ultimate result. Such a programme could be classified under three heads: what one would do to the bone and joint, what one would do in giving fixation to tendons, and what one would do in the way of transplanting tendons. Those must be considered as a whole, not as separate entities.

A transplanted tendon must be capable of carrying out its new function, and should be properly lined. Too often tendons had been called upon to carry out functions which were impossible to them, e.g., to work round some angle which was an anatomically impossible one. The tendon should be capable of being re-educated in its new sphere. As the President had said, there was a difference between the upper and the lower limb. In the lower limb one was dealing with the functions of locomotion, purposes which were semi-automatic, i.e., without any obvious willing; here, therefore, any function transferred must be capable of such practically automatic action. In the upper limb, on the other hand, the function carried out was more voluntary, and therefore re-education of voluntary action was more feasible.

The President instanced the biceps transplanted to the quadriceps. A case of biceps transplantation had been shown before the Section a year or two previously, in which the patient was able to sit with the knee flexed and to extend the knee by the action of the transplanted biceps; yet when he walked he did so with a stiff knee, because the biceps automatically acted at the time when the knee should have been flexing.

Much had been said by Mr. Dunn on the experimental work carried out on the nutrition of tendons and on what happened when tendons were transplanted, but Mr. Dunn had not outlined his own methods of carrying out the principles arrived at as a result of those experiments. He (Mr. Elmslie) realized that the nutrition of the tendon in its natural state was probably dependent on the mesotendon but surgeons were accustomed to transplanting tendons very freely, and in so doing they destroyed the mesotendon and yet the function was successfully preserved. He believed that the tendon preserved its nutrition if it were transplanted through a fatty layer. He himself transplanted the tibialis anticus in subcutaneous tissue.

He felt that Members were greatly indebted to the President for what he had said on the gradual correction of deformities, as opposed to correction by operation. He was sure he was himself performing operations on flexed hips and flexed knees too freely; and he would go away from this discussion with the intention of making a more prolonged and definite trial of correction of flexed knees and flexed hips by gradual splintage.

Mr. HARRY PLATT referred to what was one of the most important parts of the paper, namely, the conservative attitude which ought to be adopted towards contracted muscles, especially in deformities of the hip and knee. During the past five years he had abandoned operations in the correction of flexed hips.

The subject of tendon or muscle transplantation was a fascinating one, and opinions, as in other branches of surgery, had oscillated from one extreme to another, principles having been lost in a maelstrom of technique. But there were technical principles which were equally fundamental, and he thought the work of Biesalski and Leo Mayer was worthy of much greater consideration than, so far, it had received in this country.

The peroneus longus transplanted through a long incision and carried underneath the annular ligament, through the sheath of the tibialis anticus, did not act as an invertor, but in common with the other dorsiflexors.

Although the surgeon must endeavour to reduce this whole problem to one of extreme simplicity, it was wise not to ignore some of the minutiæ in transplantation technique.

Mr. G. R. GIRDLESTONE said he thought that in the treatment of infantile paralysis there had been too great a tendency to put the foot at right angles. Most of these patients had a short leg and a weak or a paralysed quadriceps. A rather tight tendo Achillis was a source of no real harm if the person had a short leg, because the heel could be raised ultimately to correspond, and a rather tight tendo Achillis was a great advantage when the person had a weak quadriceps. By elongating the tendo Achillis, or by stretching it, one could greatly accentuate the evil of a weak quadriceps. A child could easily get over a weak dorsiflexor group by lifting the foot extra high in walking, but it could not get over a paralysed quadriceps, or a weak tendo Achillis, except by the use of a walking caliper.

The President had not said what was his own practice in regard to replacing the gluteus medius. Unless there was a strong tensor femoris which could be pushed back, he (the speaker) felt helpless in the face of a very bad gluteus medius limp, and would be glad to learn of any effective means of countering this.

Mr. H. A. T. FAIRBANK said he would like to ask the President what after-treatment he employed after correction of the flexed hip, and for how long this was carried on. There seemed to be a very strong tendency for relapse of the flexion in these cases, particularly when the extensors of the hip were completely paralysed. He (the speaker) used a double inclined plane bed with the pelvis lying over the highest point, since hyper-extension was essential. He admitted that he had probably operated in too large a number of cases with this deformity.

With regard to transplantation of tendons, he did not believe in re-education of the higher nerve centres, and he never tried to make a muscle act as an opponent of its own group, in the lower limb. In the upper limb the synergic action of muscles entirely altered the case. Having selected a suitable tendon for grafting, according to the accepted rules, he thought the most important point in the technique was the tension at which the grafted tendon was sewn; the tendon must be tight. The error was almost invariably in the direction of too low a tension rather than too high. He regarded the question of adhesions as of far less importance. He entirely agreed with the frequent use of a judicious combination of arthrodesis and tendon transplantation. He had come to this conclusion before the war, and after an interval of practically five years with little or no civil surgery he had had to tackle the problem afresh, and had arrived at a similar decision.

Mr. MITCHELL SMITH said that there was a great tendency to-day to correct deformities by operations, and he was pleased to hear this advocacy of a return to gradual mechanical correction.

Mr. W. ROWLEY BRISTOW asked whether the President had experience of tendon transplantation for paralysis of the deltoid and of transplantations about the hip to recover the power of abduction.

He would like to defend the principle of the quick correction of deformity as against gradual correction. Mr. Dunn, working at Baschurch with Miss Hunt and the team she had trained, had been in a position to carry out this treatment in a way which was not possible to those who were not favoured with such an expert staff.

Mr. B. WHITCHURCH HOWELL said he thought many of the failures in tendon transplantation were due not to lack of muscle balance or of anatomical alignment of the new tendon, so much as to a lack of splitting up of the muscle near its origin. Orthopædic surgeons had been brought up to stimulate these transplants rather late after the operation; his practice

now was to have them stimulated on the fourth day, and the results were better. The tibialis anticus was transplanted with a portion of the internal cuneiform and put into bone.

Mr. NAUGHTON DUNN (President), in reply, said that in dealing with a paralysed quadriceps he transplanted either the sartorius or the tensor fasciæ femoris. If one asked a child with a paralysed limb to extend its knee-joint, and in the effort the limb was turned outwards, it meant that the patient had a good sartorius muscle. But if it turned its knee-joint in, it had an active tensor fasciæ femoris. Therefore he would transplant either, according to the indication of the case. It was true that in early days stimulation of muscle activity was much delayed after transplantation. Later, it was found that treatment could be begun earlier, i.e., movement could be begun at the tenth day. He thought that the main feature in the later success of tendon transplantations in musculo-spiral paralyses of the wrist was that surgeons got to understand the tension under which the transplanted tendons should be sutured.

He had had no experience of tendon transplantation at the shoulder which had been effective; moreover, there were no reports of cases of this kind for which success had been claimed. With control of the scapula and with a good hand, the functional result was successful if arthrodesis of the shoulder had been carried out in the correct position. Mr. Bristow had raised the question of quick correction of deformity, and the skilled staff necessary for gradual methods. The reply to that was that the absence of a skilled staff was no reason why the best treatment should not be advocated.

In the after-treatment of flexion-contraction of the hip, he kept the hip hyper-extended for six weeks or so; there was then very little tendency to re-contracture if the contracture had been overcome by gradual splintage.

His experience did not include a successful case of transplantation or treatment by splintage, apart from fixation of the hip-joint, when the gluteal muscles were severely paralysed. That was why he emphasized the importance of the gradual correction of deformity in flexion-contraction of the hip-joint, rather than rapid correction involving the sacrifice of the gluteus medius.

He was satisfied that the peroneus longus would never act as an active dorsiflexor of the foot; it might stabilize the foot, but it was a muscle which he did not transplant.

Mr. Elmslie had pointed out that the flexors of the wrist were synergic in action when the wrist was dorsiflexed, and that was true, and it might help to account for the readiness with which they responded to action after transplantation. He (the speaker) had also had success with transplanting the extensors of the wrist to act as flexors of the hand. He thought that almost any muscle of the hand could be transplanted to function apart from its group. It depended on the fact that the actions of those muscles were highly specialized. The best results were obtained from group transplantations, or from one to another of the same group.

With regard to the physiology of tendons, he purposely spent a good deal of time in the paper in dealing with the modern literature in order to illustrate that it could not be so important as modern writers stated. He agreed with Mr. Elmslie that good results ensued from subcutaneous transplantation. The selection of the tendon and its suture under correct tension were the important factors.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President—Sir CHARLES GORDON-WATSON.

[*November* 14, 1928.]

Urinary Infections after Excision of the Rectum: Their Cause and Prevention.

By CUTHBERT DUKES, M.D., M.Sc., D.P.H.

Director of Department of Pathology, St. Mark's Hospital, London.

THE operation of excision of the rectum does not in itself expose the urinary organs to infection, but a risk of infection arises because of the difficulty which patients experience for a few days after operation, in passing urine naturally, and, in male patients, because of the practice of tying, in a catheter to guide the surgeon in stripping the rectum from the urethra. Catheterization is always necessary after operation, and whether the bladder be drained by a restrained or indwelling catheter, or by repeated passage of catheters, the hazard is incurred that a urethritis, cystitis, or even pyelo-nephritis will develop as a sequel to the surgical treatment. These infections often prove a troublesome, and sometimes a dangerous complication to an otherwise successful operation. The questions I shall try to answer in this paper are: How often do these infections occur? What is their cause and where is the loophole for infection? Are they preventible, and if so, what can be done to diminish the likelihood of their occurrence?

(1) *Frequency of Urinary Infections after Excision of the Rectum.*—I have not been able to find any written statement as to how often infections of the urinary tract follow the routine treatment in excision of the rectum, but I gather from conversations that some surgeons look on them as rare, whereas others recognize them to be common, regarding the risk as the price that must be paid for the benefit of riddance of a diseased portion of intestine. Actually a much higher proportion of patients than is generally realized develop a post-operative urinary infection. In the series of fifty cases referred to in this paper, urinary sepsis was, in women, an invariable sequel to excision of the rectum and either in a mild or severe form occurred without exception in all the men patients drained with a retained catheter sealed with a wooden peg. The unobtrusive character of these infections doubtless accounts for the fact that they are so often overlooked, for in many patients an infection of the urinary track was unsuspected until revealed by urine analysis. It is well known that in other inflammations of the bladder and kidneys, local symptoms are often inconspicuous. In the post-operative infections they are particularly likely to be overlooked, because lumbar pain is naturally attributed to the manipulation necessary for the previous removal of rectum and coccyx, and if the patient has trouble with micturition this also is put down to the more likely cause. Thus localizing symptoms, even if present, have little precision, and, if absent, still leave the question undecided, for the patient's emphatic denial of any "trouble with his water" must never be taken as proof that the urinary track has escaped infection. A warning must be entered also against opinions founded on the mere inspection of the urine for turbidity. In urine clear from turbidity due to any other cause, a noticeable cloudiness due to pus does not occur till more than 400 pus cells are present per cubic millimetre. There are so many other causes of turbidity in urine that mere inspection is an altogether inadequate method of judging the presence or absence of pus, about as reliable as the diagnosis of anæmia from a glance at a patient's complexion. The only way to decide the presence or absence of infection is to submit a specimen of urine to microscopic examination.

The following plan has been followed in the study of post-operative infections:—

The urine of each patient has been examined daily (Sundays excepted) before and after operation, using a quantitative test for the estimation of pus, the principle of which consists in counting the number of leucocytes or pus cells per cubic millimetre of urine.[1] When the necessary experience has been gained, this test for pus is not only the most accurate, but also the quickest and cleanest. The tests have always been made with the first urine passed on waking in the morning, thereby assuring a fairly constant concentration, and that an interval of four to five hours at least has elapsed since the bladder was last emptied. A wide experience with this test has shown that the urine of healthy people contains less than 10 leucocytes per cubic millimetre, whereas when more than 100 leucocytes per cubic millimetre are found, an infection or some source of irritation is present. Thus three zones can be defined: the normal, between 0 and 10; that of excess of leucocytes, between 10 and 100; and pus, above 100. (Dukes, Cuthbert, *Proc. Roy. Soc. Med.*, 1928, xxi [Sect. Path.], 13-17.)

The daily microscopic examination of urine by this quantitative method revealed the disconcerting fact that a cystitis developed within a few days of excision of the rectum in all the women patients. In the first fourteen of the men patients also, some form of urinary sepsis was an invariable sequel, but in the last twenty-two men special precautions have been taken to prevent infection, with most encouraging results.

Before turning to this brighter side of the picture we must consider the course the urinary infection followed in the earlier men patients, and which unhappily it still follows in all women patients. We are now concerned with the fourteen men in whom the bladder was drained by a gum-elastic catheter tied in position for three or four days and sealed with a wooden peg, and with the fourteen women drained either with a rubber self-retaining catheter or by repeated passage of a glass catheter.

The following sequence of events occurred with a predictable regularity in every case. The urine was sterile until the second or third day, when staphylococci appeared in cultures. During the first five days the urine cell count revealed either an excess of leucocytes or else a trace of pus, and then without warning, about the sixth to eighth day after operation, the stream of pus suddenly began to flow, the urine count rising from 100 to 1,000 leucocytes per c.mm. in about forty-eight hours. Pyuria usually continued for the remainder of the patient's stay in hospital (five to eight weeks), but in some cases the infection was of a milder character and subsided in about four weeks (fig. 1).[2]

The course of the more protracted infections could not be mapped out completely owing to the patient's discharge from hospital, but isolated observations on discharged patients showed that pyuria continued for many weeks or months, as illustrated by the case recorded in fig. 2. In this case the pus was charted for sixty days after operation until the patient was discharged from hospital. She returned after an absence of four and a half months and a continuation of the daily tests showed the degree of pyuria to be approximately the same as before (fig. 2). These two infections, the one healing spontaneously in about four weeks, the other continuing indefinitely in a chronic form, represent the two extremes between which can be included the course of all the infections which developed in the fourteen men and

1 Dukes, Cuthbert, *Brit. Med. Journ.*, 1928, i, 391.

2 In this and the following charts, the time in days dates from the patient's admission to hospital, usually a week before the operation for excision of the rectum, which is indicated by the vertical line. The thick black band below the day-line marks the period during which a catheter was tied in position and if subsequent catheterization was necessary this is indicated by vertical strokes. At the levels of the marks 10 and 100 on the vertical scale, horizontal lines are drawn to remind the reader that below 10 leucocytes per c.mm. is normal, above 100 pus and between 10 and 100 the zone of excess of leucocytes. For greater convenience the number of leucocytes per c.mm. is recorded logarithmatically, and thus double the height on the vertical scale indicates a tenfold increase, treble a thousandfold, and so on. This type of scale emphasizes the differences between smaller numbers and obscures differences when the numbers are large. It allows the plotting of such a wide range as 1 to 100,000 without the smaller figures losing their significance, a great advantage to these records because in urine counts, an increase from 10 to 100 is of much more significance than that from 900 to 1,000.

fourteen women catheterized by the methods described. The three chief points to be noted are the certainty of infection, its punctual onset and variable course.

(2) *Cause of Urinary Infection after Excision of the Rectum.*—The primary cause of these infections is the invasion of a portion of the urinary tract by pathogenic microbes. No doubt there are many secondary or predisposing causes, for example, previous urinary disease, such as urethritis or cystitis; or present abnormality, such as enlarged prostate and incomplete emptying of the bladder, and these may make

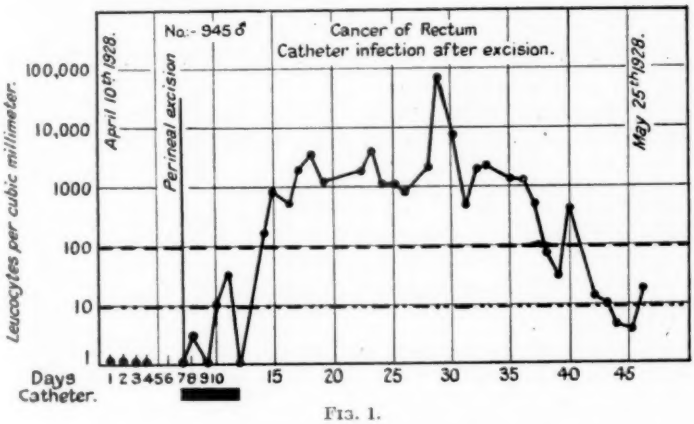

FIG. 1.

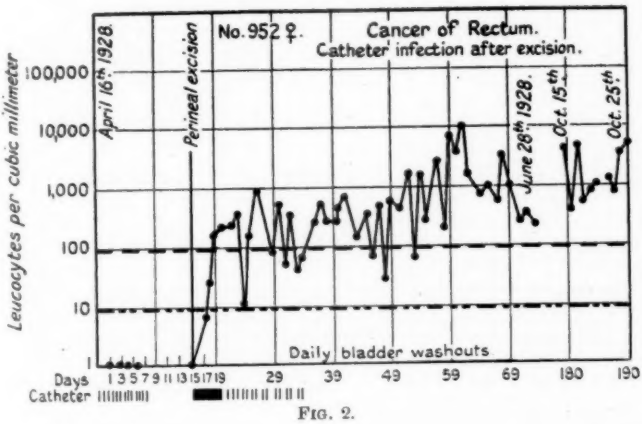

FIG. 2.

the task of prevention of sepsis more difficult in any particular case. But the primary cause is infection, and, unless some definite damage is done to the urinary organs at operation, the most likely path for the entrance of bacteria is the catheter.

The manner in which infection may be introduced by catheterization differs in the two sexes. To take the cases of the men first of all, there has never been any reason to suspect that insufficient care was observed in the sterilization of catheters, or in their introduction, but the question was put to the test by bacteriological

examination, which proved that the catheters and lubricants were completely sterilized by the routine methods in use. Suspicion quickly fell on the peg used for sealing the retained catheter. Even if the wooden peg (fig. 3) is resterilized after each release of urine, it is certain to become contaminated with bacteria immediately the bed clothes are replaced. Meanwhile the lumen of the catheter remains filled

FIG. 3.

with a column of urine and, however tightly the peg fits into the funnel-shaped end of the catheter, a thin film of fluid fills the capillary space between peg and catheter. Bacteria can grow along this thin film of fluid and reach the excellent culture medium filling the lumen of the catheter, and so get to the bladder. Confirmation of this was obtained by finding the bladder urine swarming with bacteria the day after insertion of the catheter and its closure with the peg.

In twenty-two men patients a better method than the wooden peg has been tried for sealing the retained catheter, and these experiments, though they have not fulfilled all expectations of prevention of sepsis, have in the main supported the view that the wooden peg is a serious source of infection. These twenty-two patients were not all treated alike, several different appliances having been tried for sealing the end of the catheter so as to prevent infection. In the first case the catheter was simply joined to a long piece of sterile rubber tubing closed with a spring clip. The tubing ended in a sterile collecting bottle on the floor by the bedside. Nothing was allowed to touch the end of the tubing for the four and a half days during which the catheter was tied in position. This proved an encouraging beginning, for the patient never had pus in his urine on a single day (fig. 4). However, subsequent cases catheterized in the same manner were not so successful, and a safer method than this is necessary for regular use. The different forms of apparatus which have been tried, ending in the evolution of the appliance described in the third section of this paper, have had the one common feature that they provided a cleaner seal than the wooden peg. Of the twenty-two patients so handled two had pus in the urine before operation, so must be omitted from the analysis. Of the remaining twenty, eight remained entirely free from any infection, in four a mild urethritis developed from which they recovered before leaving the hospital, and in eight there was a severe infection of the bladder urine, accompanied by continued pyuria for several weeks. In the earlier cases the catheter was sealed by methods which subsequent experience showed to be imperfect, and only the last six have been catheterized by the irrigation catheter (see fig. 8). Of these six cases, three remained entirely free from infection, in one a mild urethritis developed, and in two a severe infection. The two failures were in men with enlarged prostates who required daily catheterization for several days after the removal of the retained catheter.

In the eight cases in which these precautions against infection did not succeed, the pyuria ran a different course from that which has been noted as the expected sequence in peg-sealed catheter infections. In place of the brief incubation period of from six to eight days, there was now often a latent period twice or thrice as long, during which the urine was free from pus, though containing perhaps an excess of leucocytes. The danger of infection appeared to have passed, when suddenly, without warning, the flow of pus began (fig. 5). This disappointing onset of pyuria was usually accompanied by a few days' fever, but no symptoms suggested an infection of the bladder or kidneys, which would have passed unrecognized but for the urine tests. The only suggestion I can make in explanation of this puzzling phenomenon is that in some cases the infection may have spread up to the bladder from a latent

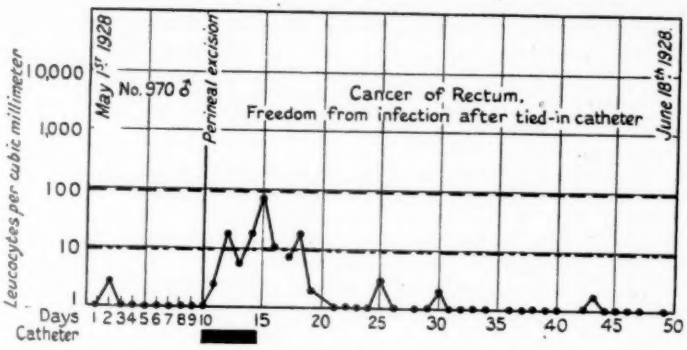

FIG. 4.

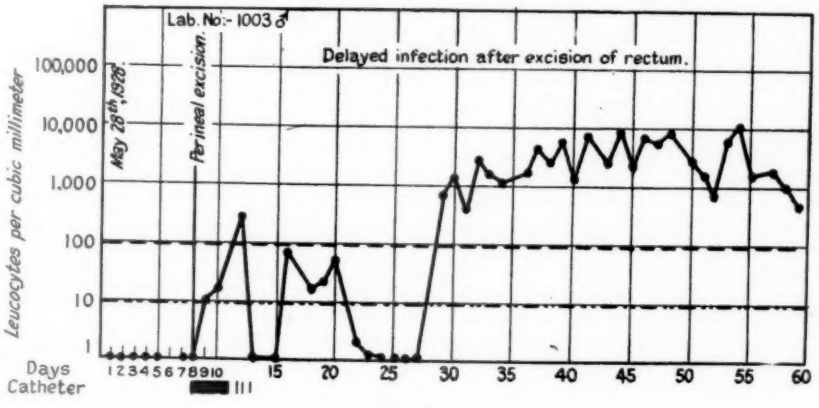

FIG. 5.

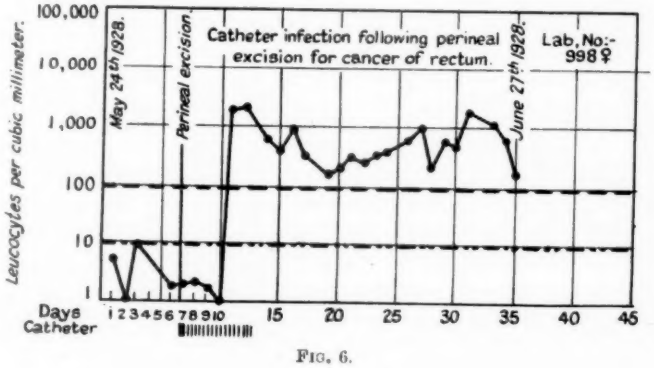

FIG. 6.

urethritis, due to the irritation of the retained catheter. Moreover, five of these cases required recatheterization after the removal of the retained catheter, and the passage of an instrument through the inflamed urethra may have carried the infection to the bladder. This hypothesis does not explain all the failures, because in some no re-instrumentation was necessary. Probably there are other causes of which we are still in ignorance for these delayed infections.

The cause of urinary infections in women also is dependent on catheterization, but the chances of infection for women are worse than for men, whether the bladder be emptied by repeated catheterization or by a self-retaining catheter. Cystitis followed rectal excision in each of the fourteen women patients. The infections developed within a week and ran a similar course to that prescribed for men patients in whom the catheter was sealed with a wooden peg (fig. 6).

The source of the infection that follows repeated catheterization is not a difficult conjecture when one considers the proximity of the dressings, perhaps soaked with discharges, and the inherent difficulty of relieving a helpless female patient. Under such circumstances the inevitability of the infection must be accepted without any suggestion of lack of skill. The other method of drainage (by a tied-in catheter) has also invariably resulted in cystitis, but up to the present the apparatus illustrated in fig. 8 has only been tried in one woman patient.

(3) *The Prevention of Post-operative Urinary Infections.*—The prevention of urinary sepsis after excision of the rectum is a task which requires the close co-operation of surgeon and pathologist.

The first principle of prevention is regular quantitative examinations of the urine for pus. The frequency with which such tests can be carried out depends on the laboratory facilities available. Daily charting was essential to the discovery of the origin and course of these infections, but would be impracticable as a routine. When the only question is: "Has the patient escaped infection?" this can be answered by six urine tests, the first before operation, the other five at weekly intervals after the excision. So many tests are needed because pyuria may either develop at once or not till a latent period of twenty or thirty days has passed. The directions for collecting the specimens are that the urine be passed into a clean but not necessarily sterile receptacle, from which the small sample bottle is to be filled.

A convenient way for the pathologist to report these tests with the minimum of explanation is by the use of a rubber stamp (fig. 7), printing a scale with numbers on one side and the significance of the figures on the other.

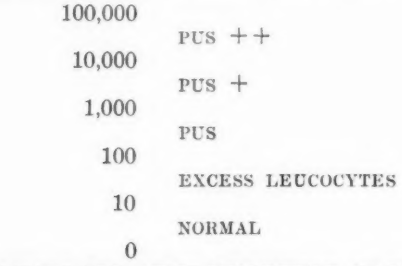

FIG. 7.

The report is completed by recording the number of leucocytes per cubic millimetre and underlining the definition of the group to which the number belongs. Thus a count of 500 is recorded by stating the figure and making a mark between a 100 and 1,000 indicating pus, and a count of 12,000 by its relative position in the

pus + + group. To the surgeon the advantage of this graphic method is that he can compare the weekly test at a glance and decide whether or not the urinary organs need attention.

Provided the urine leucocyte count never reaches the region of pus, no other test is necessary; the patient is free from infection. Even if pus is reported it need not be assumed that a cystitis or pyelitis exists, for the pus may be due to a transient urethritis. Often the localization of the inflammation can be easily settled by the examination of a two-glass test, the first sample containing the first two ounces of urine and the second the remainder passed. If pus is present only in the first specimen and absent in the second, the urethra alone is infected. If pus is present in both specimens, a cystitis or pyelitis or pyelonephritis has developed, and a catheter specimen should be obtained for the identification of the infecting microbe by culture. Until post-operative urinary infections are completely abolished in both sexes, regular urine tests for pus should be as indispensable as the records of the patient's temperature and pulse.

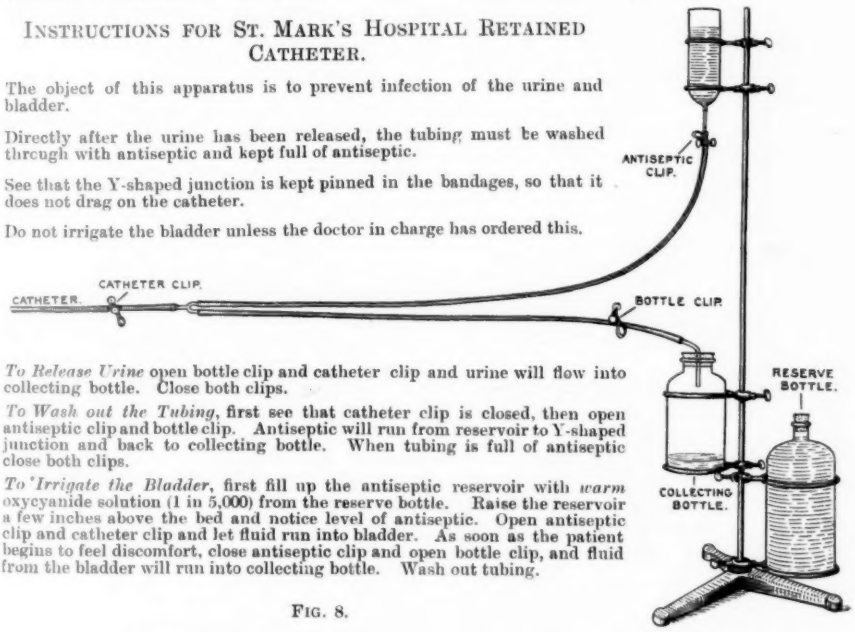
INSTRUCTIONS FOR ST. MARK'S HOSPITAL RETAINED CATHETER.
The object of this apparatus is to prevent infection of the urine and bladder.
Directly after the urine has been released, the tubing must be washed through with antiseptic and kept full of antiseptic.
See that the Y-shaped junction is kept pinned in the bandages, so that it does not drag on the catheter.
Do not irrigate the bladder unless the doctor in charge has ordered this.
ANTISEPTIC CLIP.
CATHETER CLIP.
CATHETER.
BOTTLE CLIP.
To Release Urine open bottle clip and catheter clip and urine will flow into collecting bottle. Close both clips.
To Wash out the Tubing, first see that catheter clip is closed, then open antiseptic clip and bottle clip. Antiseptic will run from reservoir to Y-shaped junction and back to collecting bottle. When tubing is full of antiseptic close both clips.
To Irrigate the Bladder, first fill up the antiseptic reservoir with warm oxycyanide solution (1 in 5,000) from the reserve bottle. Raise the reservoir a few inches above the bed and notice level of antiseptic. Open antiseptic clip and catheter clip and let fluid run into bladder. As soon as the patient begins to feel discomfort, close antiseptic clip and open bottle clip, and fluid from the bladder will run into collecting bottle. Wash out tubing.
RESERVE BOTTLE.
COLLECTING BOTTLE.

FIG. 8.

The second principle of prevention is the use of a cleaner method than the wooden peg for the sealing of a retained catheter. After experimenting with several different appliances I designed the apparatus, named for convenience the St. Mark's Hospital Retained Catheter, which provides an antiseptic seal and allows the catheter or bladder to be irrigated without disturbance to the patient. A reproduction of the printed instructions and illustration is shown in fig. 8. This is sufficiently self-explanatory of the way to use the irrigation apparatus, but some details must be given of the methods of sterilization and fitting together:—

The catheter is prepared immediately before the operation of excision by attaching about six inches of rubber tubing, closed with a metal clip. This is sterilized by boiling for a few minutes, then plunged into cold oxycyanide of mercury (1 in 5,000). After lubrication and

cleaning of the meatus, the catheter is passed and fixed in position with adhesive plaster, a dressing of gauze soaked in glycerine surrounding the catheter and separating the glans penis from the strapping. If the dressing be kept moistened with glycerine for the whole time the catheter is *in situ* the risk of urethritis or balanitis is considerably diminished. The bladder is emptied, the spring clip closed and the end of the rubber tubing wrapped round with a piece of gauze soaked in antiseptic and tucked into the abdominal bandages. The patient is now ready for the operating theatre. Meanwhile the irrigation apparatus is made ready by filling it up with a fresh supply of 1 in 5,000 oxycyanide solution. When the patient returns from the theatre, the rubber tubing at the end of the catheter is joined up to the Y-shaped junction and pinned to the bandages. The two long pieces of rubber tubing are brought out between the sheets to the side of the bed where the irrigation apparatus stands.

The same apparatus may be used for women patients, the only modification being that a rubber self-retaining catheter is used instead of a gum elastic, and a disc-shaped rubber sponge about two inches in diameter is threaded on to the catheter, soaked in glycerine and pressed hard against the external meatus by means of tapes that pass round the thighs and abdomen. The object of the sponge, on to which fresh glycerine must be poured daily, is to guard against infection from the vagina and from the perineal wound. Whether or not this method of draining the bladder by retained catheter and irrigation apparatus will prove to be so successful in women as in men, it certainly possesses the recommendation that it adds to the comfort of the patient and is convenient to the nurse, because it obviates the exposure and movements incidental to other methods of catheterization. The urine may be released without the patient being wakened from sleep.

Before leaving the subject of catheterization, mention must be made of the danger of passing any instrument for a few days after a retained catheter has been removed. Some degree of urethritis is almost invariably present at this time and there is a special risk that bacteria will be carried from the inflamed urethra into the bladder. If, after removal of the retained catheter, recatheterization is necessary, the anterior urethra should first be irrigated with an antiseptic.

The third principle of prevention is the protective inoculation with a vaccine prepared from the bacteria which cause post-operative urinary infections.

Whatever views we hold about the value of vaccines in the *treatment* of disease, we should be blind to the facts if we denied the value of vaccines in the *prevention* of disease. Judging from experience with typhoid, paratyphoid and cholera, the best time for inoculation is when the patient is in good health previous to exposure to infection.

The situation which presents itself when an operable cancer of the rectum is first diagnosed is that an interval of at least a week, and possibly three or four weeks, must elapse before the urinary organs are exposed to infection. The bacteria which cause post-operative urinary infections are primarily the *Staphylococcus aureus* and *albus*, and secondarily coliform bacilli, types of infection for which vaccine treatment has a good reputation.

At the beginning of this investigation protective vaccination was tried in a few cases and the subsequent infection appeared to run a milder course. When attention became focused on improvements in the catheter, vaccine treatment was dropped to see if infections could be prevented by a better catheter alone. As already mentioned, the improved catheter has been successful in about two-thirds of the men, and it looks as though the proportion of successes will rise with further experience, but there remains the unsuccessful minority of men patients and all the women. This being the situation at present I suggest that some further protection is necessary, and propose two doses of vaccine, prepared if possible from strains of *S. aureus* and *albus* and *Bacillus coli*, which have been recovered from post-operative urinary infections. The vaccine in use at St. Mark's Hospital contains as a first dose 100 million each of *S. aureus* and *albus* and *B. coli*, and as a second dose 200 million each of these bacteria. The two doses are injected subcutaneously at intervals of a week, as early as possible before the operation of excision of the rectum.

These three principles of prevention—systematic investigation of the urine, improved retained catheters and protective inoculation—will prevent urinary infections after excision of the rectum in the great majority of men patients and promise an appreciable reduction in the number of post-operative infections in women.

In conclusion, I must express my deep obligation to all my colleagues at St. Mark's Hospital for the facilities given to me for the examination of every patient in the hospital, and to Mr. J. Alban Andrews, F.R.C.S., for his co-operation and advice during the early stages of this work.

Summary.

In a series of fifty cases of excision of the rectum, fourteen in women and thirty-six in men, the frequency, cause and prevention of urinary infections have been studied by daily quantitative tests for pyuria and repeated bacteriological examinations of the urine. Pyuria appeared punctually on the sixth to eighth day after operation in all the women patients, and in the fourteen men patients in whom the retained catheter was sealed by a wooden peg. In some cases the pus disappeared in four to five weeks, but in the majority the pyuria continued for several weeks. For two or three days before the flow of pus began staphylococci were recovered in pure culture from the urine, but later cultures usually showed a mixed growth of coliform bacilli and cocci. The lesion in the women patients and majority of the men was a cystitis, but in a few of the men patients it is possible that the pus may have been due to a severe urethritis. During convalescence from excision of the rectum urinary infections rarely produce obtrusive symptoms, and the presence or absence of an infection can only be determined by regular microscopic examinations for pus.

Post-operative urinary infections are due not to the operation itself but to the means used for draining the bladder. In men patients the commonest source of infection was the wooden peg used for closing the retained catheter and the substitution of a better seal in the last twenty-two cases resulted in the prevention of infection of the bladder urine in two-thirds of the cases. When these methods of protection failed the subsequent infection ran a different course from that previously described. The primary focus and manner of spread of these delayed infections has not yet been determined. In women the prevention of infection is a more difficult problem because of the danger of contact between the catheter and local sepsis.

The prevention of urinary infections after excision of the rectum is a task which demands the close co-operation of the surgeon and pathologist. Regular microscopic examinations of the urine for pus are necessary, six tests at weekly intervals being the minimum for settling whether or not infection has occurred and revealing it at an early stage. To prevent infection of the bladder-urine the end of the retained catheter must be kept sealed with antiseptic. The irrigation apparatus described in this paper as the St. Mark's Hospital retained catheter is the most convenient appliance for draining the bladder and has been the most successful in the prevention of sepsis.

Although the risk of infection can be very considerably reduced by improved catheterization this alone has been insufficient to prevent urinary sepsis in a small minority of men patients. In women a modification of the same apparatus may reduce the frequency of infection but is unlikely to abolish it altogether. In view of the desirability of some additional protection it is recommended that patients who are to have the operation of excision of the rectum be given two doses of vaccine, prepared from the bacteria which cause post-operative urinary infections, the first dose to be given as soon as possible after diagnosis and the second after an interval of seven to ten days.

Discussion.—Mr. KENNETH WALKER said that it was only by such team work between the pathologist and the surgeon, as that exemplified in Dr. Duke's paper, that good results could be obtained. The in-dwelling catheter and the complications that arose from it were problems that interested the genito-urinary surgeon to an even greater extent than the proctologist. Dr. Dukes had laid emphasis on the spread of infection from a dirty plug along the interior of the catheter. There was also the possibility of infection along the outside of the catheter. No catheter could be retained within the ureter without inflicting trauma on the delicate epithelial surface, and such trauma prepared the way for infection. It was difficult to know how this could be prevented. Had Dr. Dukes noted amongst those cases in which his aseptic methods of dealing with the interior had failed, that the catheter had been retained for a longer period, and were the successful cases those in which it had been in position for a short time? He (the speaker) would be interested to hear whether Dr. Dukes could suggest any measures calculated to diminish infection along the outside of the catheter.

Mr. J. P. LOCKHART-MUMMERY said that these investigations of Dr. Cuthbert Dukes proved conclusively that a well-marked degree of sepsis of the urinary tract was an almost constant result of catherization after rectal excision, even though no symptoms suggested such infections, which were undetected in the majority of cases unless a routine examination of the urine was made.

An important factor in these cases was retained and residual urine He believed that if it was possible to avoid the presence of residual urine infection would seldom occur.

Unfortunately after the removal of the rectum there were two factors which tended both to residual urine and retention, namely, the falling back of the bladder fundus, due to the removal of its posterior supports, and some weakness of the bladder muscle from the operative trauma.

It was clear that much greater care must in future be taken to avoid these infections, which tended to hamper the patient's recovery, and he hoped Dr. Dukes would continue his investigations until such time as he was able to devise a means of entirely preventing urinary sepsis after this operation.

Mr. LIONEL E. C. NORBURY said that he would like to know whether Dr. Dukes had any information with regard to the present condition of the urinary tract of patients treated in the hospital under the old régime. It was terrible to contemplate that patients who had undergone a severe operation such as excision of the rectum should run the risk of a serious urinary complication.

With regard to Mr. Lockhart-Mummery's remarks, Mr. Norbury suggested that spinal anæsthesia might be a factor in producing retention of urine.

Mr. ASLETT BALDWIN said that no doubt all the usual urinary disinfectants had been tried. He would like to recommend about 40 gr. of extract of garlic daily, given in the form of 4 gr. pills about 20 minutes before food. This was also the best drug he had found to combat intestinal flatulence in those confined to bed. It was then best given combined with charcoal in the form of "Allisatin" tablets. As it was excreted by the mucous membranes it was most useful in any infection of the respiratory tract.

Since urinary infection still took place in spite of all known precautions, it would be well to increase the patients' resistance by giving cod-liver oil for some time before operation, vitamin A having lately been proved at Leeds University to be a powerful "anti-infection vitamin."

Mr. TURNER WARWICK said that when urinary infection occurred in these cases there were three traditional lines of treatment: (*a*) bladder irrigations; (*b*) vaccine treatment; (*c*) urinary antiseptics by mouth. It was true that Dr. Dukes' paper only referred to his results along the first method of treatment, but he wondered if Dr. Dukes' work had thrown any light on the value of the other two methods.

The possible routes of the infection to the urinary tract after excision of the rectum were: (1) along the lumen of the catheter; (2) along the outside of the catheter along a slightly damaged urethra: (3) by the blood-stream, with infection of the kidney and descending infection of the urinary tract; (4) direct spread by tissue spaces and lymphatics.

Dr. Dukes had shown that the usual path of infection was along the lumen of the catheter. Mr. Kenneth Walker had stressed the possibility of the second route and another speaker that of the third route. The fourth route had to be considered in view of the proximity of a mildly infected wound to various portions of the urinary tract. He asked whether Dr. Dukes' investigations had yielded any information indicating the frequency of the last three routes.

Dr. DUKES (in reply) said that the answer to some of the questions raised could only be provided by further experience. Three questions, however, could be definitely answered. No relationship existed between the time the catheter was left in position and the frequency or degree of infection. With peg-sealed catheters infection had followed retention of the catheter for only a few hours, whereas, with the St. Mark's retained catheter, in several cases the catheter had been left in position for from five to seven days without a trace of pus. The duration of post-operative urinary infections was very variable. A few patients recovered spontaneously in four or five weeks, but in other cases pus was still present in the urine after several months. No urinary antiseptic so far tested had been of any use in preventing the infections due to peg-sealed catheters, or in shortening the course of infections.

Specimen of Vesico-Colic Fistula.—LIONEL E. C. NORBURY, F.R.C.S.—Mrs. W., aged 45. History of flooding five months previously. Fibroids of uterus diagnosed. Abdominal exploration by gynæcologist. Large mass found in left iliac fossa, adherent to uterus, bladder, and pelvic colon. Diagnosed as either carcinoma of colon or diverticulitis. Abdominal wound closed. Passage of fæces and gas through bladder soon afterwards, with definite cystitis. *On Examination:* Large mass felt through abdomen and *per rectum* in left iliac region. Thought to be a case of diverticulitis of the colon.

May 18, 1928.—A transverse colostomy performed, with subsequent irrigation of bladder. A few days later large abscess discharged through old abdominal incision, with disappearance of mass. Great improvement in general condition and condition of bladder. Although no fæces passed *per urethram*, yet communication between bowel and bladder remained, as was proved by the fact that irrigation of the rectum or lower colostomy was accompanied by the return of fluid through the bladder. Radiograph after a barium enema showed a large communication between the colon and bladder.

Since the wound did not heal and the patient was much distressed by the constant flow of pus from the bladder, a further operation was decided upon.

October 13.—Examination under anæsthetic showed a hard mass, presumably growth in the pelvis, with adhesions to the abdominal wall in the region of the old scar.

October 15.—Resection of portion of pelvic colon, rectum and part of the bladder, together with a portion of the abdominal wall. Tube drainage of bladder and of pelvic cavity. Microscopy showed the growth to be a colloid carcinoma.

At the present time the bladder is closed, the patient is able to retain urine for at least from three to five hours, and she sleeps most of the night. General condition, excellent.

Specimen: Leiomyoma of Rectum.—W. S. PERRIN, M.Ch.—Removed from anterior wall of rectum of a man aged 39. The symptoms were hæmorrhage from rectum on defæcation, which had extended over a period of three years. The growth was just above the anal canal and had a broad pedicle.

Two Specimens of Villous Tumour of the Rectum.—W. B. GABRIEL, M.S.—(1) Specimen of a large villous tumour in the upper-third of the rectum—an unusual position. Obtained post-mortem from a female patient aged 72, who had died fourteen days after colostomy was performed.

(2) Transverse sections of an extensive recurrent villous tumour of the rectum.

History.—The patient is a man now aged 65. Operations were performed in 1916 and again in 1917 for local removal of a villous tumour in the lower third of the rectum. The tumour recurred, and in 1923, posterior proctotomy and extensive excision were performed. The growth recurred in less than a year, but the patient carried on until 1928 when he was found to present an extensive friable growth reaching up as far as the finger could reach and involving the entire circumference

of the bowel. In parts it appeared firmer and nodular, and clinically it seemed suspiciously like malignant degeneration.

Treatment, June, 1928.—Colostomy, followed two weeks later by perineal excision, the bowel being divided well above the upper limit of the growth. The patient has made a good recovery and the wound has healed well.

Microscopical Examination.—This shows the tumour to be a simple villous tumour with no evidence of malignancy. Dr. Cuthbert Dukes has investigated this case by means of whole sections which show clearly the superficial nature of the tumour.

Specimen of Carcinoma of Ascending Colon with great Dilatation of Cæcum. Removed by Resection of the Distal Half of the Colon after Preliminary Short Circuit.—Sir CHARLES GORDON-WATSON, K.B.E., F.R.C.S.—The specimen was removed from a man aged 67, who was suffering from partial obstruction when he first came under observation. At operation, after the diagnosis was established, a lateral anastomosis was performed between the terminal ileum and the mid-portion of the transverse colon and a catheter was tied into the dilated cæcum. Resection was delayed owing to a temporary fæcal fistula, but was carried out about seven weeks after the short circuit. The cæcostomy was allowed to close three weeks after the first operation.

In spite of the short circuit and temporary colostomy, the cæcum had not recovered its tone but was almost as large at the second operation as at the first. This was due to the fact that some intestinal contents passed on through the ileo-cæcal valve and did not easily pass the malignant stricture in the ascending colon. The ileo-cæcal valve, functioning well, prevented regurgitation. The moral of the case is that a lateral ileo-colostomy without division of the ileum is not in itself a safeguard against perforation of the cæcum in acute obstruction of the ascending colon, although this particular case fell short of this extremity. A second point is that a cæcostomy acting as a safety valve, should not be allowed to close unless the second operation of resection is contemplated at an early date. Some years ago at St. Bartholomew's I lost a case through perforation of the cæcum occurring after a similar short circuit, by acute obstruction arising in the same situation.

[Sir Charles Gordon-Watson demonstrated the specimen on the screen and called attention to the marked stenosis produced by the growth and the extreme dilatation of the cæcum.

Section for the Study of Disease in Children.

[*November* 5, 1928.]

DISCUSSION ON FIBROSIS OF THE LUNG IN CHILDREN.

Dr. L. S. T. Burrell.—So many chronic pulmonary diseases begin in childhood that it is important to recognize and treat the various catarrhal conditions so common in the child. But before discussing these I should like to say a few words about tuberculosis in children.

Tuberculosis of the lungs, except as part of a general tuberculosis, is rare in children. During the first five years of life the mortality from tuberculosis is high, but after that age it rapidly falls and in children between the ages of 5 and 16 there is a very low mortality indeed. After 16 the mortality rises until it reaches its maximum in middle age. There are three age-groups as regards tuberculosis, the first consisting of the first five years of life when general and surgical tuberculosis is common but pulmonary tuberculosis is rare. The second between the ages of 5 and 16 when all tuberculosis is rare. And the third, after 16, when surgical tuberculosis is rare but tuberculosis of the lung is common.

Tuberculosis of the glands around the root of the lung is frequently present in children, but the lung tissue does not subsequently become infected from the glands. Dr. Wingfield examined 160 hardened specimens of the thoracic contents of children and found naked-eye tuberculosis of the root of the glands in 17·5 per cent. of the cases, but he did not find any spread of the disease into the lung.[1]

In these cases of tuberculous bronchial glands, the disease normally becomes arrested, leaving behind some calcareous glands but no active disease. In some cases the children are listless and poisoned by the tuberculous toxin, but in many cases there is little, if any, impairment of the general health. Of course tuberculosis of the roots of the lungs does occur, but I do not think it is secondary to the so-called hilum or bronchial gland tuberculosis. When the lung is affected, the spread is often rapid. I saw a child aged 13 who was at school and apparently well when she suddenly became ill with what was thought to be influenza. A fortnight later harsh breath sounds but no adventitious sounds were heard over the root of the left lung. A little sputum was obtained but no tubercle bacilli were found. The temperature was 98·6° F. in the mornings and 102° F. at night. A skiagram showed an opacity at the root of the left lung, and a second skiagram, taken three weeks later, showed considerable spread of the disease; there was bronchial breathing and bubbling râles were heard over the middle portion of the left lung and the sputum was loaded with tubercle bacilli. During the five weeks of illness the child's weight had fallen from 6 st. 11 lb. to 5 st. 3 lb. A left artificial pneumothorax was at once induced and a year later the patient was free from symptoms and weighed 7 st. 7½ lb., but there was a large area of fibrosis. This case illustrates the extreme rapidity with which tuberculosis of the lung spreads in children. It should also be noted that tubercle bacilli were not found in the sputum in the early stages. Whereas absence of tubercle bacilli from the sputum in the later stages is very strong evidence against the disease being tuberculosis, it is by no means uncommon to fail to find them in the early stages.

In the adult, disease at the apex of the lung is almost invariably tuberculous, but in children the great majority of such cases are non-tuberculous, and resolve like ordinary pneumonia after a time, although some fibrosis occasionally remains.

It is the catarrhal conditions which are important, for they are so common in children, and are apt to lead to bronchiectasis, fibrosis, or chronic bronchitis. The principal causes of catarrh in children are measles, influenza, and, to a less extent, whooping-cough. In 1925 there were 94,769 deaths in England and Wales in

1 R. C. Wingfield, "Modern Methods in the Diagnosis and Treatment of Pulmonary Tuberculosis," 116.

children under 15 years of age. Of these, 20,637 were due to non-tuberculous respiratory disease, 1,539 to pulmonary tuberculosis and 4,490 to other forms of tuberculosis.

In lobar pneumonia, due to the pneumococcus, there is much congestion and exudation, which is completely absorbed when the patient recovers. There is no actual destruction of lung tissue as there is in broncho-pneumonia. The cases of so-called unresolved pneumonia are usually those in which the pneumonia takes longer to resolve than usual. It is only when there is actual destruction of lung or bronchial tissue that healing takes place by scar tissue and fibrosis results. It is, therefore, the bronchopneumonic type of disease, following measles or influenza, which is most likely to lead to fibrosis.

There is undoubtedly a great amount of catarrhal infection of the lungs in early life. Many children are known to be liable to colds or attacks of bronchitis; often it is a general bronchitis, but frequently each attack is localized in a certain area of the lung, and in each attack it is the same part of the lung that is affected. As time goes on fibrosis takes place and often bronchiectasis follows. Moreover, often several children in a family are affected and suffer from bronchitis and the mother will state that her children have weak chests. This suggests an infection spreading through the different members of the family. Treatment, therefore, should be begun early and should be thorough for, by curing the first child and removing the infection, the other children are protected. When once fibrosis or bronchiectasis is well established there will always remain some damage in the lung. In the case of a child who has frequent attacks of bronchitis, before any definite fibrosis has developed much more can be done. All sources of infection, such as the tonsils, teeth, gastro-intestinal tract should be sought for and treated where necessary. A vaccine is more often successful in children than in adults, and, if it is impossible to obtain an autogenous vaccine, a stock one may be given and will often produce good results, if used as a prophylactic. Parke Davis' anti-catarrhal vaccine has prevented the liability to colds and bronchitis in several of my patients. I usually start with a small dose, such as 0·1 c.c., and increase every three or four days by 0·1 until 1 c.c. is reached, or until a reaction is produced, in which case the dose is not increased.

A long holiday will often effect a cure, and I have seen excellent results from a visit to Switzerland or to the seaside. Coddling is a mistake. Light clothing, sunlight and fresh air are as good as hot rooms and heavy clothes are bad. Of course, during the acute attacks the child should be kept in bed, but afterwards, the real treatment, aiming at the prevention of recurrence—with possibly a subsequent fibrosis and infection of other children—should begin, and should consist of getting rid of all foci of infection and training the child into the best possible condition by healthy outdoor games, good food and as much sunlight as possible.

Dr. A. G. Maitland-Jones said that one important point in Dr. Burrell's paper on which he thought all Members would be agreed, was that tuberculosis was a somewhat rare cause of fibrosis of the lung in children. He would like to learn more about the condition termed "epi-tuberculosis." Perhaps that condition was illustrated by a case he had had in an infant, seven months old, brought to hospital because of pyrexia. There were signs of consolidation in the right upper lobe. The condition lasted, with fever, for three months, and then dried up. X-rays showed shadows in the right upper lobe, and the von Pirquet reaction was positive. No tubercle bacilli were found. It might be epi-tuberculosis, but he had not heard a definition of the term. He regarded the case as one of a sub-acute or chronic pneumonia.

One saw children who came into hospital with pneumonia, which lasted one or two months, with a temperature, etc.; than the temperature fell, and the patient was left with physical signs, such as dullness over one or other lobe, with weak breath sounds and perhaps some crepitations. It was possibly fibrosis, but the signs

persisted for two, three, even five months, but afterwards cleared up completely. Should one diagnose a definite fibrosis in such a case? He hesitated to call the condition unresolved pneumonia. He saw a fair number of children with these physical signs in one lobe, and yet in the adult one found only a few cases of frank bronchiectasis, and he wondered whether these children recovered, as he thought probable, or died before arriving at adult life.

Dr. Hilda Stoessiger said she had been following up consecutive cases of lobar and broncho-pneumonia which had recently been admitted under Dr. Chodak Gregory, at the Royal Free Hospital, in order to ascertain the early stages in the development of chronic pulmonary catarrh and pulmonary fibrosis. Though the number of cases was not large enough to base any firm conclusions upon they had shown one or two facts. Some children were appearing every few weeks with a localized pulmonary catarrh in the old pneumonic area. She (Dr. Stoessiger) had been surprised that a number of cases in which such signs had appeared, had been diagnosed and treated as cases of lobar pneumonia. These instances illustrated the frequent difficulty in differentiating between lobar pneumonia and broncho-pneumonia in very young children.

Among the cases of both lobar and bronchopneumonia a large number gave a family history of contact with respiratory disease. In thirty-two consecutive cases of lobar pneumonia there were twenty-two with a past history of recurring bronchitis and pneumonia, and sixteen with a family history. In thirty-four cases of bronchopneumonia there were nineteen with a past history (that of three not being stated) and seventeen with a family history. This argued for a careful supervision of all cases of pneumonia, whether there was a clinical resemblance to lobar pneumonia or not.

With regard to the value of X-rays in the diagnosis of these conditions, she had seen many cases having signs of repeated local catarrh and early fibrosis in which the skiagrams had shown no difference between the two sides of the chest. On the other hand, skiagrams occasionally showed up patches of fibrosis, in the absence of localizing clinical signs, in generalized bronchitis with emphysema and in obscure cases in young infants. She had in mind the case of a marasmic baby admitted with marked clubbing of fingers and toes, with no history of cough and no signs in the chest, in which a skiagram had shown a dense fibrosis in the middle zone of the right lung.

Was there not a possibility that syphilitic induration of the lung might be a cause of some of the rarer types of pulmonary fibrosis in young infants?

Dr. C. D. S. Agassiz said that in the hospital to which he was attached there were 350 beds for tuberculous children, and his experience was that pulmonary tuberculosis in children was not uncommon, if one took as the criterion the presence of tubercle bacilli in the sputum; this occurred in quite one-third of the children.

In his experience children with fibrosis always had the signs most marked at the base, sometimes at both bases; they cleared up quickly, but then the indications recurred. One week such a child would have marked fibroid signs on one side, and the next week would have very few symptoms. This might explain some of the results which Dr. Burrell obtained by the use of vaccines.

Sometimes he (Dr. Agassiz) had found tubercle bacilli in the fæces, though they had not appeared in the sputum.

Dr. H. E. A. Boldero said that the President had rightly stressed the importance of these catarrhal pulmonary conditions. The causes of ill-health of children under five years of age fell into two chief groups: nutritional or gastro-intestinal and these so-called catarrhal cases. With regard to the first, infant welfare activities had done a great deal, but, for the second group, nothing had been

effected. Could not more be done for these children, as the present treatment was far from satisfactory?

With regard to preventive measures, it was easy to suggest that these children should be kept out of doors and away from subjects of coryza; but he (the speaker) had it in his mind to attack the subject in a wholesale way, by trying to persuade mothers to agree to inoculation of their children with the Parke Davis or other vaccine.

Dr. James Watt said he was puzzled to know what happened to children the subjects of non-tuberculous fibrosis. There seemed to be a large incidence of fibrosis among children, but in adults there were only few cases of the non-tuberculous form. What, therefore, happened to those children? Did they succumb in early adult life, did bronchiectasis develop, or did they suffer from later engrafted tubercle?

Dr. A. G. Phear said that Dr. Watt's problem was an important one. Most probably these children died before reaching adult age. But he had known cases in which children had been under observation with definite signs, and then a few years later those signs had disappeared. One such case was that of a young girl, first seen seven years ago when 8 years old, with definite signs of fibrosis and bronchiectasis. She had then passed from his observation, but two years later he saw her again, with an almost exact reproduction of the signs. Again he saw her, five years later, and if this had been the first occasion on which he had seen her he would have declared her chest to be normal. That case, and others, suggested that the condition might clear up to such an extent as to cause the physical signs to disappear. Without a knowledge of the history it was easy to fall into the error of diagnosing cases of slowly resolving pneumonia as fibrosis and bronchiectasis, i.e., to mistake a changing condition for a permanent one. Had Dr. Burrell any explanation for that problem?

Dr. G. A. Sutherland said the profession knew something of the conditions under which fibroid lung arose, and also about the progress of the disease during a large number of years. It was usual to regard bronchiectasis as a common end of fibrosis of the lung, but in his view it was an exceptional result of that condition. It was generally accepted that bronchopneumonia was the chief cause of fibroid lung in children, not the ordinary simple bronchopneumonia—regarding which he was somewhat sceptical—but that associated with, or following, measles, whooping-cough, or perhaps influenza.

Probably lobar pneumonia never led to fibroid lung; at the hospital to which he was attached there had not been a typical case of lobar pneumonia which subsequently developed into one of fibroid lung. The latter condition he looked upon as a local disease, one arising in the lung; it was not likely that a general infection would produce a disease confined to one part of the lung. Dr. Burrell had referred to the left lower lobe as being the common site, and he (the speaker) agreed with him; it was so in two of every three cases. A reason which suggested itself to him was that in association with acute bronchopneumonia there was usually a dilatation of the heart, because of the strain, especially upon the right heart, with pressure upon the left lower lobe, and that lobe probably collapsed, and did not clear up like the rest of the lungs.

If there was one disease which needed to be clearly differentiated from another, it was pulmonary tuberculosis from chronic fibroid lung. Acute pulmonary tuberculosis in children was rare—he was not alluding to miliary tuberculosis—and there seemed no special reason why the chronic type should be at the left lower lobe. Dr. Burrell seemed to think that in cases with signs of fibrosis, if tubercle bacilli were found in the sputum, that clinched the diagnosis; but he (the speaker) was not sure about that. There was a very mixed infection in cases of fibroid lung, and the pathologist at his (Dr. Sutherland's) hospital nearly always found in them pneumococci, streptococci, *Micrococcus catarrhalis*, and the influenza bacillus. No one had yet proved which of those organisms was primarily responsible for the condition. Moreover, in the circumstances in which so many London children lived, it was

not at all remarkable that among the organisms found in the sputum, there should be found the tubercle bacillus, and it did not follow that the child had tuberculosis of the lung. It was common knowledge that tuberculosis was very frequently diagnosed, especially by School Board medical officers and in tuberculosis dispensaries, in cases of simple fibrosis of the lung. Sometimes the good results shown at sanatoria were due to the fact that some of the cases being treated in them were not tuberculous at all. Dr. Burrell had referred to bronchial catarrh as leading up to this condition of pulmonary fibrosis, but he (the speaker) doubted whether bronchial catarrh was a common cause of fibroid lung. One reason for this doubt was that good results were stated to follow the use of a vaccine, whereas if there had been actual fibrosis he did not think such results could have ensued. He considered bronchial catarrh and pulmonary fibrosis to be the results of the initial illness. He took rather a hopeful view of the ultimate fate of children with fibrosis of the lung; in many the acute exacerbations subsided. He never objected to these cases being sent to a sanatorium, for the conditions and treatment there were ideal.

Dr. F. Parkes Weber said that one class of case which interested those whose patients were chiefly adults as much as those whose patients were chiefly children, was that in which there occurred local recurrent attacks of what appeared clinically to be slight pneumonia, and he asked whether any Member knew of evidence that typical cases of the kind ever led to fibrosis of the lung. He himself was sceptical on the point. Furthermore, was any reason advanced why the recurrences each time should affect the same part of the lung? And was there any real evidence that these cases represented attacks of pneumonia around an old tuberculous focus?

Post-mortem examinations were very rare in such cases, because perhaps they usually recovered; at least, adults apparently did.[1] Such cases had, he believed, been called "epi-tuberculosis," on the supposition that they occurred on top of, or were associated with, tuberculosis, but the explication which had been advanced that each attack was analogous to a huge Pirquet's reaction around a chronic tuberculous focus, though it sounded plausible, was not, he believed, proved to be correct.

Dr. F. J. Poynton (President) said that Dr. Phear had emphasized one point which had been in his own mind, namely the remarkable recoveries occurring in children supposed to have had extensive fibrosis of the lung, with displacement of the heart. Sometimes, in consequence of such a recovery, he had thought his diagnosis must have been wrong. There must be some explanation, and perhaps Dr. Burrell would be able to throw some light on it. He (the President) often thought the observer had been led astray by the amount of signs that were heard into thinking that the lesion of the lung was greater than was actually the case. He thought that the explanation of the fact that bronchiectatic cases which had been supposed to be doing badly improved when placed in better surroundings was that the lung was compensating, and that the amount of noise heard in the chest gave an exaggerated idea of the extent of the mischief. He was glad that infant welfare work had been mentioned; what those centres did for children in the way of prevention in the type of case under discussion was really a valuable contribution to medicine. Various speakers had alluded to the way in which the same part of the lung "lit up" time after time; that had also been his own experience. He regretted that, so far, there had been no surgical contributions to this discussion, as he was much interested in a number of surgical points. All knew the kind of case in which a child had a very extensive fibrosis in one lung, and a less extensive condition in the other, and in which multiple bronchiectases gradually developed, and finally the sputum became offensive. He wondered whether, by surgical measures, it was possible to save such a child from what otherwise seemed to be inevitable destruction. He was much interested personally in a question

[1] Dr. Weber reported a striking case of the kind in an elderly woman under his care, in an article on "Arrested Pulmonary Tuberculosis," *Zeitschrift für Tuberkulose*, Leipzig, 1904, v, 216, 217.

in regard to empyema, as, for a number of years, he had been working at the possibility of doing away, in many cases, with the open operation for children, and substituting some form of closed operation. When some form of aspiration was used one was left with a lung which sometimes did not expand. It was interesting to note how some of these lungs seemed to be stunned—he could find no better word—and would then suddenly expand when the child was placed in better surroundings. It was an interesting question whether one encouraged fibrosis by aspiration. His (the President's) gradually increasing impression was that this was not the case, and that eventually the lung expanded so that there was no fibrosis as a result of that treatment.

He had been interested in what was said about vaccine treatment. He himself had not felt disposed to use vaccines in the case of children, but, if vaccines did good in these cases, it was a great advantage, because the continual recurrence of these catarrhs in children was a matter of great importance.

They had all seen cases in which the condition had been—and rightly—diagnosed as fibrosis of lung with bronchiectasis, and in which post mortem there had been found both tuberculosis and a condition which one would have thought was a non-tuberculous fibrosis. He had seen remarkable examples of dilated bronchi, with tuberculous cavities, in cases which were not unilateral, but which were of the well-known type, a large amount of one lung being involved, a lesser amount of the other.

Dr. H. Joules asked whether there was any method of differentiating between collapsed lung and fibrosis of lung. He had seen conditions—the result of a collapsed lower lobe—which, in many cases could not be differentiated by clinical examination from advanced fibrosis, yet which two or three months later had cleared up entirely.

Dr. Burrell (in reply) said one need not be surprised that tubercle bacilli were found in the sputum and in the fæces of children who clinically were not tuberculous. If one accepted the current view with regard to tuberculosis. one must believe that practically every child was infected and so had tubercle bacilli at some time in the body. If a child had what was considered to be fibrosis, and tubercle bacilli were definitely present in the sputum, the prognosis was much worse than in a similar case but in which no tubercle bacilli were found in the sputum. That applied to both the child and the adult. The mortality among the "T.B. plus" cases was always enormously higher than in the "T.B. negative" cases in any institution. Collapsed lung could usually be distinguished from fibrosis by early displacement of the thoracic viscera. A case of carcinoma of the bronchus would sometimes start as an apparent pneumonia, but the heart and mediastinum would be drawn to the affected side.

In reply to Dr. Phear, it was certainly curious that one did not find, post mortem, these cases of fibrosis in adults. He (Dr. Burrell) thought that many of the children died. As shown by the figures which he had quoted, 20,000 out of 90,000 who died in childhood, had these catarrhal conditions. In the case of many children, fibroid lung was diagnosed when it was not present. Judging from post-mortem findings, fibrosis of the lung was not common. Many of the cases of recurrent bronchitis or pneumonic attacks were not cases of fibrosis, and he agreed with Dr. Phear that sometimes the intensity of the signs led one to mistake a temporary condition for a permanent one. These conditions took a long time to become permanent.

He did not think that any real advantages were traceable to vaccines given when once fibrosis was established. What he set out to emphasize was that treatment must be begun before a fibrotic or a bronchiectatic condition occurred; whether that consisted of vaccine, sending the child to play on the sands at the seaside, or dealing with septic teeth and tonsils. There were two outstanding reasons for the comparative infrequency of fibrosis of lung in the adult: (1) a large number of children suffering from fibrosis did not reach adult life; (2) in a considerable proportion of children with catarrhal conditions fibrosis did not develop.

Section of Comparative Medicine.

President—Major-General Sir JOHN MOORE, K.C.M.G., F.R.C.V.S.

[*November* 28, 1928.]

Unsolved Problems in Salmonella Food Poisoning.

By WILLIAM G. SAVAGE, M.D.

ABSTRACT.—Salmonella groups of organisms are recognized as predominant in food poisoning outbreaks, but knowledge of the primary source of Salmonella bacilli and of paths of infection to implicated food is incomplete.—Unsolved problems discussed.—Food animals suffering from Salmonella infection are common in Germany but comparatively rare in this country and in most outbreaks the original food is shown to have been sound.—Infection of food from a human carrier is extremely rare. Detailed study of individual outbreaks brings out the striking fact that the Salmonella strains which cause food poisoning are just those types capable of causing disease in *both* man and animals. The hypothesis which best explains bacterial causation of most outbreaks is that the source of infection is derived, in most cases, from animals suffering from Salmonella disease or acting as carriers of these bacilli. Many facts favour this view, including the widespread extent of Salmonella infections in animals associated with food. Examples: Cows suffering from Salmonella infections with these bacilli in the milk; the widespread prevalence of such infections in rats and mice; the considerable extent to which pigs are infected with Salmonella bacilli.—Attention is directed to the presence of specific agglutinins in the blood of food animals such as bullocks and pigs, also to the fact that while in man a carrier condition for food-poisoning bacilli is extremely rare and at best a transient condition in animals. It is a well attested phenomenon. The causes of this difference are worthy of further study.

Not only are there numerous types within the Salmonella group but these types exhibit characteristic and definite differences of pathological behaviour. *Bacillus paratyphosus* B and *Bacillus aertrycke* furnish a good illustration and these two types are critically contrasted.

While our present knowledge is imperfect, it is suggested that the types in this group fall into three groups, i.e., (*a*) Strains pathogenic to man but not to animals; (*b*) strains pathogenic to animals but harmless to man; (*c*) strains pathogenic both to man and to animals. Only the last group cause food-poisoning outbreaks and represent the original and less specialized types.

Varying virulence of Salmonella strains and problems thus raised in relation to food poisoning outbreak are discussed.

APART from its intrinsic importance as a cause of much illness, food poisoning is of special interest because of the numerous scientific problems raised by its consideration. Some of these may at first appear to be mainly academic, but they are really closely related to problems of great pathological importance.

Modern researches have shown what a large part Salmonella group organisms play in food poisoning, and this being established; there is a tendency to regard the ætiology of food poisoning as solved. So far from this being the case we have to admit numerous lacunæ in our knowledge, both as to causation and as to the way these organisms react in the animal body. It is difficult to say if the number of outbreaks of food poisoning is declining or not, but, whatever the answer, certainly there is no conspicuous decline. The question of control is very much wrapped up in these problems, and, until we can solve them, it is not to be anticipated that the flow of food poisoning outbreaks will show any material diminution. Since Salmonella infections bulk so largely in the total, attention in this paper will be restricted to these outbreaks. A clear discussion of unsolved problems and of our difficulties is a useful preliminary to any efforts to solve them, and it is hoped that this contribution may serve as a stimulus to a more accurate knowledge of the subject.

1.—*The Primary Source of the Salmonella Bacilli and the Paths of Infection.*

Anyone who studies many reports upon food poisoning outbreaks is aware that the common sequence is—investigation of outbreak, conclusive evidence of the implication of a particular food, demonstration that the food was or must have been

infected with Salmonella bacilli, proof that the cases of illness were due to ingestion of Salmonella bacilli or their products. Beyond these points fact passes into conjecture, and only rarely is there evidence as to where the Salmonella bacilli originated and how they came to be transferred to the food responsible for the outbreak.

In a proportion of cases, but in this country only a small proportion, we can clearly establish the ætiology, and can trace back the food to the animal which furnished it and demonstrate a Salmonella infection in that animal. In Germany this proportion seems to be distinctly larger, and a conspicuous feature of these outbreaks is the high percentage due to the consumption of the meat of emergency slaughtered animals. Lentz [1] ascribes the increased number of outbreaks in Prussia during and after the war to shortage of meat and the consequent consumption of more necessity-slaughtered animals. In the great majority of cases in this country the facts conclusively prove that we have to deal with the infection of food which previously was perfectly sound.

Infection from a human carrier is often advanced, but it is the merest conjecture in almost every case and there are not more than two or three outbreaks in which it has ever been shown to be the cause. There is a mistaken analogy with typhoid and paratyphoid fever. In these diseases we get early blood infection and persistent carriers. In food poisoning we rarely get blood infection, and persistent carriers do not occur. Detailed data on this point are limited. By far the most extended is in the outbreak described by Perry and Tidy [2] due to *B. aertrycke*. Forty-four of the cases showing bacilli were selected, and careful stool examinations were made, as far as possible, daily. At the end of three weeks only 60 per cent. were positive, 33 per cent. positive after four weeks, 6 per cent. positive after six weeks, and at the end of seven weeks only two were positive. One continued to contain this bacillus at the end of fourteen weeks. Other reports are less systematic, but where details are mentioned it is to the effect that excreta samples examined several weeks after the onset had ceased to contain Salmonella bacilli.

Case-to-case infectivity is very rare, but when the sufferer, soon after recovery, acts as cook, direct human infection may occur. The following are the only two outbreaks I know of which illustrate this point.

In the Brixton 1920 outbreak, a woman (Mrs. H.) had an attack of food-poisoning on July 22, acute but not severe, for she continued her cooking and other household duties. She apparently infected the mid-day Sunday meal, and nine other persons had acute food poisoning, while she suffered from a second attack. She seemingly infected several food substances, since these cases were spread over several days, i.e., three on July 24, five on July 25, and one on July 26.

At Lynmouth, in 1925, twenty-three persons in a boarding house suffered from food-poisoning, the vehicle being beef and the infecting organism *B. aertrycke*. A few days after the primary outbreak a second group of cases developed in the same house, also due to *B. aertrycke*. One of the kitchen staff, a sufferer in the first outbreak, was still passing *B. aertrycke* in the fæces. As she had just returned to duty after convalescence, it is possible that she may have infected the food consumed by the patients in later cases.

Both theoretical considerations and actual records suggest that outbreaks spread by human carriers are of the rarest.

Considering other possible sources of infections, we note the striking fact that the Salmonella strains which cause food poisoning are just those types which are capable of causing disease in both man and animals. As long ago as 1913 [3] I advanced the hypothesis that the main source of infection is from animals which are either at the time suffering from disease due to Salmonella group bacilli or are acting as carriers of these bacilli. Fifteen years' further study has strengthened my view as to the fundamental accuracy of this hypothesis. Even, however, if the truth of this hypothesis is admitted, it does not solve our difficulties, since in the

investigation of individual outbreaks we are frequently unable to prove any direct connexion.

The Salmonella group contains a number of distinct strains and they differ as regards their distribution. For many years I have been impressed with the importance of accurate information as to the distribution in Nature of each type and its pathogenicity as regards animal types. Clearly, if certain types can be shown to cause animal diseases but never human infections, we can concentrate upon the significant types, and the task of preventing food-poisoning outbreaks will be immensely simplified. Unfortunately the great bulk of the existing literature has to be disregarded, since the type of Salmonella described is unknown, the label given being unreliable. We have to build up anew our facts and only include organisms the serological types of which have been accurately determined. Bruce White and I [4] made a start with this work, and the following table, with some added data, represents about how far our present knowledge extended:

TABLE I.—SALMONELLA GROUP DISTRIBUTION AND DISEASE CAUSATION.

Type	*Disease-producing rôle*	
	Man	Animals
B. enteritidis	Gastro-enteritis of food-poisoning type. Occasionally causes sporadic cases of illness	Enteritis and other infections in rats, cows, calves. Has been isolated from a pig suffering from swine fever
Derby	Cases of food poisoning	Pig. Exact rôle unknown
B. abortus equi	Not found	Abortion of mares
B. paratyphosus B.	Paratyphoid fever. Probably never causes true food poisoning	Not found (except possibly and very rarely in pigs in U.S.A.)
B. aertrycke	Food poisoning. Occasionally causes sporadic cases of illness	A widespread cause of enteritis in mice, guinea-pigs and other rodents; enteritis in parrots and other birds, and has been found in chickens. A cause of calf enteritis. Rare in rats
Stanley	Food poisoning	Not as yet isolated from animals
Newport	Food poisoning	Dog suffering from enteritis. Organs of a pig
B. morbificans bovis	Food poisoning	Cow suffering from septic infection
Reading	An outbreak of paratyphoid fever type	From a water supply (? origin). From an outbreak in guinea-pigs
suipestifer-Hirschfeld strains		
1.—*B. suipestifer*	Food poisoning	Secondary invaders in pigs in hog cholera. Occasionally as a primary cause of disease in pigs
2.—Voldagsen-Glässer	Not known in man	Enteritis in pigs
3.—*B. paratyphosus* C.	Disease of paratyphoid fever type (mostly of Eastern origin)	Not known in animals
B. paratyphosus A.	Paratyphoid fever	Not known in animals
B. pullorum	Not found	Disease of chicks
B. sanguinarium	Not found	Disease of chicks

Note.—A few other types have been isolated. i,e., Thompson type (by Dr. W. M. Scott), the L type (by Mr. Bruce White), and the Dar-es-Salaam strain, but as yet we know very little about these strains.

In addition to outbreaks traced to the use of meat from definitely diseased animals, in a few other cases some connexion with infected animals has been traced. The most clear-cut are the few outbreaks in which the infection of the vehicle (milk) has been traced back to a cow suffering from a Salmonella infection and passing this organism in the milk. The Aberdeen outbreak of July, 1925 [5], with 497 cases was due to *B. enteritidis*, and this was also isolated from the organs, including the mammary gland, of a cow suffering from an indurated udder and later from septicæmia, slaughtered the day following the human outbreak. The cow was part of the herd supplying the implicated milk. The same organism was the cause of an outbreak of milk-spread food poisoning at Newcastle-upon-Tyne in 1913, and was isolated from the organs of a diseased cow on the implicated farm. In all I have records of six food-poisoning outbreaks spread by milk in which the Salmonella strain was isolated from a diseased cow. Four were *B. enteritidis*, one *B. aertrycke*, and in one the type was not clearly identified.

There are strong grounds for implicating rats and mice as carriers of Salmonella bacilli and as causing food-poisoning outbreaks. Both frequently suffer from Salmonella infections. While there is a number of outbreaks associated with Salmonella strains used as a rat or mouse virus, direct proof that either rodent has actually caused an outbreak is hard to find. Salthe and Krumwiede [6] record an interesting outbreak in which fifty-nine persons in New York suffered from food poisoning, the vehicle being cream-filled cakes and éclairs. Salmonella bacilli were isolated from left-over samples of these foods. Rodent excreta was found on two storage shelves, and from one sample the same organism (*B. aertrycke*) as the one responsible for the outbreak was isolated. The cream mixture was the food at fault and the storage shelf was nearly over the place where the pail of mixture was placed to cool.

The relationship of Salmonella bacilli to pigs is complicated, but very interesting and important in relation to food poisoning. Only a short summary of the facts can be given. Swine fever, a disease peculiar to pigs, was originally considered to be due to *B. suipestifer*, but is now generally accepted as due to a filterable virus. *B. suipestifer* is frequently found in cases of swine fever, the percentage prevalence varying from 0 to 45, or even higher. More detailed study of the isolated strains, however, shows that many are not true Salmonella bacilli but belong to the para-Gaertner types which I described in 1906-08. Also, the Salmonella strains present are not all *B. suipestifer*, but include *B. aertrycke*, *B. enteritidis* and other true Salmonella types. For example, from forty-one swine fever pigs examined in our investigations, Mr. Bruce White isolated Salmonella strains from ten, i.e., *B. enteritidis* 1, Derby 1, *B. suipestifer* 7, *B. suipestifer* with *B. aertrycke* 1. Dammann and Stedefeder, Glässur, Manninger and other observers have also described outbreaks of enteritis in pigs, which were not outbreaks of swine fever, due to infection with Salmonella strains. The presence of Salmonella bacilli in swine fever naturally suggests that these organisms may be normally present in the pig's intestine, and that the resistance-lowering effect of the swine fever virus enables them to act as secondary invaders and so be present in many cases. Extensive studies of normal pigs, show, however, that Salmonella strains are either absent or only present very rarely. [Savage (1906-08), Bainbridge (1911), Aumann (1911), Savage (1918), Jordan (1918), etc.].

A consideration of all the facts suggests the following conclusions. Like many other animals pigs suffer from infections with Salmonella bacilli, and under suitable conditions these may assume epidemic proportions. The carrier state is fairly readily induced in these animals, and some of the animals which recover are likely to harbour the bacilli for considerable periods. These carrier strains may include Salmonella types other than *B. suipestifer*. This accounts for the rare positive findings in normal pigs. In swine fever it is more or less a matter of chance if any

of the pigs affected are carriers of Salmonella strains. If this is the case, the diminished resistance enables them to spread from pig to pig and play their part as secondary invaders. This explains the widely varying results already quoted, as regards their prevalence in swine fever. Tenbroeck [7] reports an illuminating experiment which bears this out. Thirty-three healthy pigs were infected either by exposure with swine fever pigs or by inoculation of bacteria-free material containing the virus. Five pigs were also examined which were spontaneous cases of swine fever. Most of the animals were killed in from seven to twelve days after their temperature began to rise. All showed characteristic lesions of swine fever. Of the large number of healthy pigs inoculated with bacteria-free virus, not one showed any Salmonella bacilli, indicating that healthy pigs do not carry these organisms in their gastro-intestinal tracts.

These Salmonella strains are capable of originating food-poisoning outbreaks if they gain access to foods, and these facts suggest that such carrier cases in pigs are an obvious menace and that it is not necessary to assume infection from a definitely diseased pig in each case. In Peckham's [8] Derby outbreak of human food poisoning, the organisms responsible for the outbreak were also isolated from water from a tank on the premises into which the large intestines of pigs were placed after slaughter. If one of these pigs was a carrier of these Salmonella bacilli, we can readily understand the probabilities of specific infection.

In connexion with animals acting as sources of Salmonella infections I published in 1918 [9] the results of a long series of observations upon the agglutination reactions of the blood of presumably healthy animals with Salmonella strains. The following indicates the percentage obtained:—

Animal	Percentage with No reaction	Slight reaction	Well-marked reaction
Calf	100	—	—
Cow and ox	11	35	54
Sheep	78	11	11
Pig	38	19	41
Horse	41	33	26
Rat	94	4	2

Kinloch, Smith and Taylor [5] have recently (1926) carried out similar examinations, using a much larger number of Salmonella strains than I employed, and their results show the same sort of thing, i.e.:—

Animal	Percentage with No reaction	Slight reaction	Marked reaction
Calf	100	—	—
Bullock	45	25	35
Sheep	24	48	24
Pigs	56	40	0
Rats	88	9	0

Considering the different methods used and material examined, the two results are fairly comparable. At present one is not prepared to say if these findings mean anything, but their absence in the calf and the considerable number of cases in which the adult animal shows positive agglutinins is an interesting and suggestive fact.

The importance of the carrier in spreading Salmonella infections from animals has been emphasized. The valuable investigations of Topley and those working with him contain numerous illustrations of the importance of latent infection and the carrier in mouse Salmonella infections. I will restrict myself to one quotation. Topley and Ayrton [10], in 1924, state that: "A high proportion of mice, which have survived for forty-two days after the oral administration of *B. aertrycke*, yield cultures of this organism from their spleens when examined post mortem. In many

series of experiments the proportion of survivors showing such positive spleen cultures has exceeded 50 per cent."

A point which I have repeatedly noticed in animal inoculation work with Salmonella bacilli is that, whatever the method of introduction, these bacilli rapidly find their way into the intestinal contents; indeed, in some cases when death follows parenteral introduction they are found only in the intestinal contents. I think it likely that these animals with latent infections are always liable to excrete bacilli with the fæces, and it is from this source that infection on to sound meat or other food is to be dreaded.

The data submitted are, I would suggest, ample to substantiate my hypothesis that it is to diseased, latently infected and carrier animals that we have to look for our reservoir of Salmonella bacilli which infect food and set up food poisoning. At the same time, we are ignorant of the natural history of these Salmonella infections in the domestic and food haunting animals, and an increase of knowledge is highly desirable. Veterinary records are singularly scanty on this subject, and the authorities I have consulted do not seem to know anything about Salmonella infections in animals. In recent years a little more interest seems to have been shown, but there is clearly a large unexplored field. We shall not prevent Salmonella food poisoning until we know more about Salmonella infections in animals.

2.—*The Specialization of the Salmonella Group as regards Animal Hosts.*

The problems now to be discussed are of great theoretical interest. They represent conceptions arrived at by Bruce White and by myself [3], but as yet they cannot be said to be generally accepted. The Salmonella group contains a number of different types which are culturally nearly identical, and it is only by the use of very special tests that cultural differences can be shown to exist. Serologically these strains show clearly-defined differences which are maintained with great constancy, while their serological relationships are also definite and constant. While, therefore, we are justified in considering the various types as different organisms, we cannot do otherwise than regard them as strains derived from a common stock, and probably of no long ancestry as individual types. What is not so generally accepted is that their pathological properties show equally decided and constant differences. The two strains, *B. aertrycke* and *B. paratyphosus* B, serve well to illustrate the point, particularly since they are so closely alike as regards their cultural and serological characters. For further particulars see Savage and White [11].

When introduced parenterally into mice or other laboratory animals, both cause a fatal septicæmia, but with the oral route *B. aertrycke* usually causes a fatal septicæmia, while *B. paratyphosus* B is harmless and the bacilli rapidly die out. Boiled cultures fed to animals cause no effect with *B. paratyphosus* B but set up marked gastro-intestinal irritation with *B. aertrycke*. In man feeding with living *B. aertrycke* results in acute food poisoning, and the same effect is seen with boiled dead cultures. *B. paratyphosus* B in man by the oral route sets up paratyphoid fever with early invasion of the blood-stream, while boiled dead cultures do not cause any gastro-intestinal symptoms. *B. aertrycke* is a widespread cause of disease in many animals; *B. paratyphosus* B has only been found as a cause of disease in man.

With these two organisms we have a most interesting differentiation as regards their pathological activities. We are all aware that such differences occur with other closely-allied organisms, e.g., tubercle bacilli, human, bovine and avian, but they are of special interest in such very closely-allied strains. Here, if anywhere, it would seem possible to reverse the process and, for example, to accustom *B. paratyphosus* B to acquire the power to kill mice by feeding, since it readily can do so when introduced by the subcutaneous route. Besredka [12] has succeeded in killing rabbits by feeding them with this organism when the intestinal mucous membrane

is previously damaged by ox-bile, but this is a very different thing, and the strains so used have not acquired any additional virulence. The superior penetration properties of *B. aertrycke* might be ascribed to the irritant properties which Bruce White and I demonstrated in cultures, particularly when boiled, but our endeavours to obtain infection *per os* with living *B. paratyphosus* B plus boiled aertrycke toxins were unsuccessful.

B. paratyphosus B seems to have become as specialized for man as *B. typhosus*, if our views are correct, and sets up the same kind of disease. *B. aertrycke* has a much less specialized rôle, but acts as a strong irritant, and attacks man and animals impartially. It would lead me too far into speculation to discuss the reasons for these differences, such for example as to whether we are faced with local intestinal conditions of penetrability, or whether the problem is mainly one of differing immunological reactions of the blood and other tissues. The work of Meyer and others throws interesting lights upon these points.

Clearly the specialization which has taken place, and is probably taking place, in this group is of considerable practical importance to the students of public health and of animal diseases. Our knowledge is yet so imperfect that any generalizations are hazardous, but existing knowledge suggests that the members of the Salmonella group as regards their relationship to man and animals fall into three classes :—

(*a*) A group of strains pathogenic to man, but of little or no pathogenicity to animals. *B. paratyphosus* A and B belong to this group.

(*b*) A group pathogenic to animals but harmless to man. *B. abortus equi*, *B. sanguinarium*, *B. pullorum*, are examples.

(*c*) *A large group of strains pathogenic both to man and to animals.* *B. aertrycke*, *B. enteritidis*, *B. suipestifer*, are conspicuous examples.

These types, undifferentiated as regards their host predilections, are possibly nearer to the original type. They are less specialized. It is significant that all the types capable of causing food poisoning seem to belong to this group, and this fact strengthens the conception of the animal origin of food-poisoning infections.

Bruce White [13] has recently developed somewhat the same point of view, but based upon the serological affinities of the different types.

3.—*Virulence of Salmonella Strains.*

No one can study many food-poisoning outbreaks without being puzzled by the very varying virulence exhibited by the Salmonella strains concerned in the different outbreaks. I am quite unable to offer any rational explanation of many of the difficulties.

We are all aware that food-poisoning outbreaks are decidedly more prevalent in hot weather while, as I have shown elsewhere [14], this is particularly noticeable when outbreaks due to living Salmonella strains alone are considered. To account for this prevalence various possibilities may be suggested, such as a greater virulence of Salmonella strains in hot weather ; a larger dose, due to greater multiplication on the vehicle before ingestion ; greater opportunities for infection of the food in summer, a greater sensitiveness of the alimentary tract of the human host in warmer weather. None of these possibilities seems really adequate.

I would mention another point of interest. There seems to be some relationship between virulence as judged by the severity of the attacks and by the number of deaths, and the type of food serving as the vehicle of infection. For years I have noted the low mortality in milk-spread outbreaks. I have particulars of seventeen British outbreaks spread by milk, the majority being Salmonella infections, and there were deaths in only four outbreaks. In all there were about 3,050 cases with six deaths, a case mortality of 0·2. Contrasting this with ten outbreaks with brawn or potted meat as a vehicle, we have 446 cases and twelve deaths, a case mortality of 2·9 ; practically fifteen times as fatal.

The explanation may possibly be some simple fact which I have never had time to investigate, as for example that *B. enteritidis* or other Salmonella does not produce powerful toxins in milk. Other milk infections may be very fatal, and Kinloch contrasts his milk-spread *B. enteritidis* food-poisoning outbreak in Aberdeen, in 1925, with 497 cases and one death (a person aged 83), with the Aberdeen outbreak of 1919, also spread by milk but due to a Flexner type dysentery bacillus, with over 1,000 cases and seventy-two deaths.

The Salmonella group produces toxins which will withstand boiling for many minutes, a remarkable property which does not appear to be possessed by any other group of organisms. Some *B. coli* strains have been stated to possess it, but I have never come across any such strains. I do not know of any explanation of this unique property. According to Gaertner, Holst and other investigators, this thermostability of toxins is a property which rapidly becomes lost when the bacilli are artificially cultivated outside the animal body for any length of time. This property may therefore be related to virulence as this diminishes with artificial subculture. We must regard this thermostability as a potent factor in food poisoning, but clearly it cannot be an adaptation to enable the strain to set up food poisoning.

I am by no means satisfied that this thermostability of the toxins is shared by every member of the group, and it is a subject worth detailed experimental investigation. It may be that differentiation has proceeded as regards this property differently in the different members of the group.

In the present paper I have not only discussed some of the unsolved problems of Salmonella food poisoning but, after being a close student of food poisoning for well over twenty years, have ventured to set before you certain conceptions in regard to these Salmonella organisms which, even if future investigations fail to confirm them, at least serve to clarify the issue and offer a basis for discussion.

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Discussion.—Dr. G. S. WILSON said that sporadic cases of infection in man, with organisms of the food-poisoning group, were not uncommon, and he suggested that they might serve as sources of infection in food-poisoning outbreaks. He was not satisfied with the evidence brought to show that cases of presumed Salmonella food-poisoning resulted from the ingestion of pre-formed thermostable toxins in the food. It was possible to feed mice with comparatively large amounts of a broth culture of *Bacillus aertrycke* without setting up any toxic manifestations.

Dr. SAVAGE (in reply) said that he did not attach much weight to the only point which Dr. Wilson had brought forward against the view that Salmonella toxins alone cause food poisoning. One of the difficulties in Salmonella work was the remarkable insensitiveness of all the laboratory animals, and also of kittens, to the irritant action of these toxins. Proof had to be looked for in other directions. Since these toxins did not permit direct proof, chemical or otherwise, of their presence, the evidence was cumulative rather than definite as regarded any one particular link. Such evidence was available along two lines. (1) *Epidemiological*: the majority of such outbreaks were associated with the consumption of canned foods, foods heated to temperatures adequate to kill Salmonella bacilli, but insufficient to destroy these heat-resisting toxins. Pieces of canned foods associated with these outbreaks were, in fact, sterile, or, if bacteria were present, these were due to surface contamination after opening. There were no living bacilli present which could possibly be associated with the outbreak. The course of the attack was most characteristic, and they were all alike except

in the rare cases when living Salmonella bacilli were present. The incubation period was from two to four hours, i.e., longer than that due to any metallic or simple chemical cause, and too short to be of living bacterial origin. They were of acute onset; very severe in type, in fact far severer than when due to a few living Salmonella, yet followed, almost always, by recovery, and by quick recovery, the only deaths being in the old, the very young, or those damaged by concurrent disease. The outbreaks were exactly what one would expect from an acutely irritant toxic substance, without living bacteria, and the association with canned foods, *par excellence*, i.e., just the foods in which they would be expected, granted an infection of the food with living Salmonella bacilli prior to "processing." To what else could they be due if not to undestroyed irritant toxins?

(2) *Bacteriological*: Here the evidence was cumulative. It was set out in detail in the Medical Research Council's Special Report, Series No. 92, and the lines of inquiry could only be indicated here. (*a*) In certain cases, by feeding with large doses of the food, they had been able to demonstrate an inflammatory action on the stomach or intestine of the fed animal. It did not always occur, and he (Dr. Savage) had never seen it in mice. (*b*) Injections of suitable emulsions of the incriminated food into animals gave rise in some cases to the development of specific agglutinins in the blood of the injected animal. (*c*) One could rarely demonstrate any agglutinins in the blood of sufferers in these canned food outbreaks, and it was not to be anticipated as a general feature. Very occasionally, however, some agglutination reaction was developed.

By one or other of these methods they had usually been able to demonstrate a positive reaction, and their successes had been quite as high as the limited utility of the procedures would lead them to expect. Negative results were no evidence that toxins might not be the cause, while positive results were valuable. Taking the two lines of evidence together, he (the speaker) claimed that they had solid grounds for definitely associating these peculiar and definite outbreaks with the undestroyed toxins of food-poisoning bacilli, and good ground, but less complete as regards proof, for ascribing these bacilli to the Salmonella group, the only group of bacteria with heat-resistant toxins.

Section of the History of Medicine.

[*December* 17, 1924.]

The Company of Barber Surgeons and Tallow Chandlers of Newcastle-on-Tyne.

By F. C. Pybus, M.S., F.R.C.S.

The earliest record of the Company of Barber Surgeons of Newcastle-on-Tyne is contained in one of the early histories of the town, which states the following:—

"The ancient ordinary of this society, dated October 10, 1442, enjoined that they should go together in procession on Corpus Christi day, in a livery, and afterwards play the "Baptizing of Christ" at their own expense. Every man to be at the procession when his hour is assigned him, at the Newgate, on pain of forfeiting a pound of wax; to go also with their pageant, when it shall be played in a livery, on the like pain; and that no alien born should be taken apprentice, or allowed to work within the town, or without, under a penalty of twenty shillings; that the Society should uphold the light of St. John Baptist in St. Nicholas' Church, as long as they were of ability; that no barber, apprentice, or servant should shave on a Sunday, neither within the town nor without, by a mile's space." "These paragraphs are from the olde book which is now lost."

The oldest Minute book, from which the above was taken, has long been lost, but it is mentioned in the earliest Minute book (1616-1686) which still survives.

What is known of the Company is to be gleaned mainly from the two Minute books still in possession of the Society.

About this date (1616) the Company had some sixty brethren, and their meeting place was the Tower over Pandon Gate—a gate in the South-East portion of the town wall. Shortly after they met in one another's houses and in All Hallows Church, consequent upon the outbreak of Plague in the City in 1636. The meeting was again over Pandon Gate, but in 1643, money was lent to the Mayor, presumably for the preparation of the defences of the town. In 1644, when the town was beleaguered, their place of meeting was occupied in its defence. We find, therefore, an attempt to settle in some more suitable abode by the following Minutes:—

April 28, 1646. Ordered by the consent of the Company that John Hall, Edward Bainbridge, George Durham, and dauid Sheuill and the 2 stewards Chas Clarke and Rob Anderson doe goe and tak a vew off a house for a metting house in Silver Street belonging to Mr. Ralph Cocke and uppon likinge to Agre with Mr. Cock for itt for ye use of ye Companij, and failing of yt house to looke out for any other.

4 August 1648. By the major vote of the Companey it is voted that the Lease be taken from the Maior and Comon Counsell in the names of John Hall sen. Edward Bainebrigg, Gilbert Browell, Henry Wills, Charls Clark, Thomas Smith, Robert Anderson, David Shevell, John Jowsey, Henry Shaw, Willm Clay and Robert Archbald in trust to the use of the whole societie and their Successors and noe otherwise,

The house in question was taken for a time, but in 1648 the Mayor and Corporation were petitioned for a site to erect a meeting-house. Such a site was granted for a period of sixty-one years at a annual rental of 6s. 8d.

In addition to the site for the building, a further portion of land was obtained for a garden in which to grow medicinal herbs. The Minutes are now largely concerned with disbursements on the new building. By 1652 meetings were held in the "Barber Surgeons' Hall."

In Corbridge's map of Newcastle, 1724, the Hall may be seen in the St. Austin Friars, near the Holy Jesus Hospital, a building which was of somewhat similar

architecture, and is still in use. In the margin of the map there is a larger view of the Hall, here reproduced (fig. 1).

We also have a description of the Hall from an outside source. The following passage from Celia Fiennes, in her book *Thro' England on a Side-Saddle*, throws an interesting light upon the anatomical proceedings of the Company and the state of their Hall and Garden in the time of William and Mary (1689-94) :—

"At Newcastle. I went to see the Barber Surgeons' Hall which was within a pretty garden walled in, full of flowers and greens In potts and in the Borders; its a good neate building of Brick. There I saw the roome with a round table in it railed round with seates or Benches for ye Conveniency in their dissecting and anatomising a body, and reading Lectures on all parts. There were two bodyes that had been anatomised one the bones were fastned with wires the other had had the flesh boiled off and so some of ye Ligeament remained and dryed with it, and so the parts were held together by its own muscles and sinews that were dryed with it. Over this was another roome in wch was the skin of a man that was taken off after he was dead, and dressed, and so was stuffed—the body and limbs. It Look'd and felt like a sort of parchment. In this roome I could take a viewe of the whole town, it standing on high ground, and a pretty Lofty building."

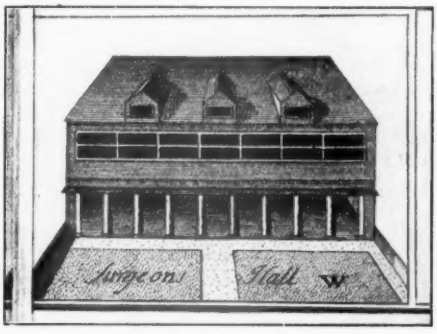

FIG. 1.—Surgeons' Hall (1652).

The Hall was rebuilt in 1730, in rather different style—a description of which is given in Bourne's *History of Newcastle* (1736) :—

"On the East of this same field is the Hall of the Barber Surgeons, which was re-built by them in the year 1730.

"It is a very beautiful one, and not a little sumptuous ; it stands upon tall Piazzas, under which is a very spacious walk. There is before it a fine Square, divided into four Areas or Grass-Plats, surrounded with Gravel-Walks, each of which is adorned with a Statue. The first of the two next the Hall is the Figure of Aesculapius placed upon a tall Pedestal, upon one side of which is a Motto. In the area, opposite to this, is the figure of Hippocrates, who bears an open book. This was set up in the year 1710, John Shaw and Robert Golightly Stewards. On the other two Grass-Platts are the figures of Galen and Paracelsus which were erected 1712, Robert Golightly and William Handby Stewards. I am informed by one of this Company that, in a little time, the wall at the foot of the garden will be taken away, and instead thereof will be iron rails. It will show the Hall and Gardens to some advantage, but be rather too great an ornament for such a dirty part of the town.

"There are besides this Square two other Gardens for Herbs which, together with the Hall and Garden we have been speaking of, are attended by a Gardner, who lives there for that purpose."

This building was occupied by the Company until near the year 1830, when, after standing empty for some time, it became the home of the newly-formed Medical School in 1834. Fig. 2 shows the house as it appeared at this time.

The Newcastle-on-Tyne College of Medicine carried on its teaching there until 1851, when the hall was demolished to make way for railway extensions. In the same year a new hall was built for the Company by the owners of the railway in

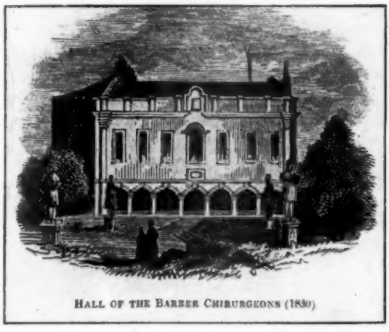

FIG. 2.—Barber Surgeons' Hall (1830)

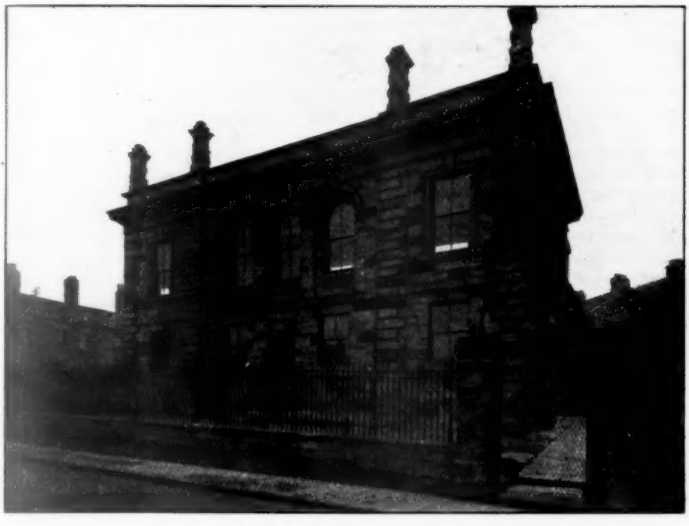

FIG. 3.—Barber Surgeons' Hall (1851).

Victoria Street. It is a conspicuous stone building, adorned with the Coat of Arms of the two Companies (fig. 3), and was occupied on its completion by the minority of the sections into which the medical school had divided. Teaching was carried on here until the union of the two medical schools in 1857, when it became, and remained, empty for a time. It was later bought by the Parish of St. Paul for a hall and school, and serves the latter purpose at the present time.

Of the many activities of the Company the minute books bear witness. In the first place there was their play, "The Baptizing of Christ," and the dinners associated with their head meeting day on Trinity Monday. That they might have a full repast at a very moderate figure, the following bill of fare and its cost shows:—

"A Bill of Fare for the Barber Surgeons' and Wax and Tallow Chandler Company, October 28, 1478, in the reign of Edward IV:—To 2 loins of veal, 8d.; 1 do. beef, 4d.; 2 leggs mutton, 2½D.; 1 pigg, 6d.: 1 capon, 6d.; 1 rabbit, 2d.; 1 doz. pigeons, 7d.; 1 goose, 4d.; 1 gross eggs, 8½d.; 2 gallons wine, 1s. 4d.; 18 gallons ale, 1s. 6d."

The disbursements show that meetings were not without refreshment; they were even augmented during the Plague in 1636.

Disbursets 12th june, 1620.

It. geuen to the poore and russell (a servant of the Comp.)		1	6
It. for the clerkes dinner and Launcelote (a servant of the Comp.)		2	0
Ffor washing the Linnen		2	0
It. to the maids and for wine		4	0
It. for dinners and wine on Michaellmas Mundaie	1	14.	8
It. for the Clerkes yeares wage		10	0
It. for dressing the Towre		1	0
It. paid at the Gardiners for the Companie		1	4

At a meetg on the xii June, 1620.

Received of Tobie Watson late apprentice to John Hudspeth Barber and Chirurgion and Chandler for a ffyne for marrieing and begetting a barne before he was made a free brother of this Companie whereof was taken xxs..... iij li.

28th May, 1632.

	£	s.	d.
Item. Spent on our head meting day in wine	00	10	08
Spent in beare the same day	00	04	04
Giuen to the poure the same day	00	02	00
Given to the Musicke and for there dinners	00	03	06
Spent on midsomers even in wine by consent...	00	12	00
Giuen to tow trauelers	00	03	06
Gieun to the Clarkes of alhallows at or meeting there	00	01	06.
fo 196. Disbrs. 14 June, 1652. Ittem paid for flaggin the Courtin ...	00	05	00
Ittem paid for dressing the garden and for seeds	00	17	04
Ittem giuen Benjamine Duffeild a distressed brother	00	04	00
Ittem paid for weeding the garden	00	01	04

Fo. 197. At a meeting holden the 9th day of November 1653 att the Barber Chirurgions Hall:

It is this meeting agreed that the order before written for the paying of 2d a weeks, towards the ffinishing of their Hall shall continue in force for a whole year, beginning the first of November instant.

	£	s.	d.
Disbrs. 1711. To the poor att the door 6s. To 6 widdows £1 15s. (often repeated)	02	01	00
To horse hire and expenses to Hilton in severall jorneys by myselfe 8s. and by Mr. Hanby 5s.	00	13	00
Jan. 11 (1710-1) Boatman for Statues p receipt	00	00	00
To the Labourers for carrying them	00	04	0
To Joynr for a Beater & Bowells	0	01	8
To Jno Sheerwood for ye Pedistells	0	15	0
To Mr. Handbys Bill for Skelleting	1	07	0
Feb. 11. To bringing Statutes (sic) from into the hall...	0	01	0
Disbts. To May, 1690:			
For Coles 2s. for new binding and paper to the book of orders 2s. 6d., hearth money 2s	00	06	06

Several isolated minutes may be noted apart from those mentioned in the general list :—

"June 3rd, 1698. The Companies has agreed to give 40s. for a treat at the Head Meeting at the Forth."

"June 20th, 1698. Expended at the Meeting House upon the Day, the thanksgiving was for the poor £4 16s. 4d."

"April 20th, 1727. Ordered that five guineas be spent on the Head Meeting Day for a dinner."

The Company did not forget Charity either at home or abroad, for we find them not only helping a "distressed brother" or a ship-wrecked surgeon, but sending contributions to the Edinburgh Infirmary and the Episcopal Clergy in Scotland. They also contributed to the expenses of the local races, and the entry of May 21, 1722, I strongly suspect, applies not to any article of furniture, but to the Northumberland Plate, a most important event on Tyneside. At one time the Company, or a certain number, on pain of fine in default, were to attend the funeral of any member, and cloaks were lent for this purpose. The practice was stopped in 1775, by the garments being banished to the workhouse.

	£	s.	d.
1st January, 1676(7) John Hall a brother in Charity by order	01	00	00
To three Chirurgions Shipwracked & traulers	00	06	06.

October 28, 1737. Ordered that two guineas be given to David Rae and Alex. Clark Collectors for the suffering episcopal Clergy in Scotland.

21 May 1722. By vote £2. 2. 0. to be given towards the Plate.

March 21 1739(40). Ordered by a majority of Votes in this Company that ten guineas be sent to the Stewards or Governors of the Infirmary att Edinburgh.

12 Octr. 1755. Ordered by consent of the whole Company that the Two Black Cloaths & the Cloaks be given to the Workhouse in Newcastle.

30 April 1760. Ordered by a majority of the Company this Day that two guineas be given to the Races.

14 Octr. 1777. Ordered that two Guineas be annually subscribed to the Dispensary.

Ordered that the sum of Twenty shillings annually subscribed to St. John's Charity School be withdrawn from this time.

The *teaching of anatomy* has, of course, a special interest. Unfortunately little is said about it. It seems to have been in use about 1690 as the following Minute shows :—

May 23rd, 1692. Disburst about ye man that was given the Company for dissection. £4 10s. 9d. Mending the table and coffin, and drinks and bread, 13s. 8d.

Disbts. to May 28, 1711. Ordered vote that a Schelaton be bought and sent for to London not exceeding six guineas.

Sept. 30th, 1753.—Ordered by a vote of the company that Mr. Halliwell is to have the bones of Dorothy Gatonby, who was lately executed. (This woman lived in Love Lane on the Quay, and was executed on the Town Moor for the murder of her bastard child. After hanging the usual time, her body was taken to the Surgeons' Hall, where it was dissected and lectured upon by Mr. Halliwell, Mr. Stodart, Mr. Lambert and Mr. Gibson, Surgeons. Her two sons, painfully affected by the disgrace brought on the family, drowned themselves, and her daughter, to avoid public odium, left Newcastle and went to a remote part of the kingdom).

As will be seen also a skeleton was procured in London and later bodies were obtained after execution in the city. A Minute of June 4th, 1683, states that "Dr. Peter Bates have this day presented the Company with Bartholinus, his Anatomy for their use and the Stewards to keep safe and deliver it to their successors every yeare." This is the English edition of Bartholinus (1668) by Culpepper and Cole, and it forms, with two old Minute books, the whole library of the Society to-day.

Although the Barbers may have given up anatomy, the practice of dissection was continued by the Medical School and some interesting and amusing events in this connexion occur in its early history and during resurrectionist times.

By an ordinary dated Sept. 17, 1671, they were constituted a body politic by the name of "Barber-Chirurgeons & Wax and Tallow Chandlers" ordering them to "meet yearly and choose two Wardens who were to be sworn, that apprentices should serve seven years, and that when any brother had taken a cure in hand no other should meddle with it till it was completed on pain of forfeiting 20s for the first, 30s for the second, and 40s for the third default, one half of which to go to the brother who first dressed the patient."

"It is further enjoined that none should wash, dress or trim on a Sunday on pain of forfeiting two shillings for every offence" and the above Ordinary gives "power to make by-laws, and to choose annually two searchers who are to be sworn." Brand, Vol. ii, pp. 342-3.

Much time seems to have been taken up in disciplinary matters. "Searchers" were appointed in 1618, and apparently as a result of their inquiries John Bawne was fined, as were many others.

1618. Mr. that John Hudspeth and Christopher Ridley are by the most voices of the Company appointed to search for trimmings on the Sabbath day, and to present accordingly.

1618. John Bawne fined for trimming two men on the Sabbath day, being the xvii of May—this is discharge qt he submitted himself to the Company, and a—— undertook for all his fines the pay, and is put to account ffoll. 5.

August 18, 1724.—Henry Chambers fined for shaving on the Sundays.

29 June, 1637. Meating houlden at John Pithy's.

It is ordered and agreed upon by the Consent of the Company of Barber Chirurgions and Chandlers that the ij Stewards with 4 of the Company shall goe about the toune unto all ffoorrenders which selleth Candells contrarie to our orders those who are appointed to goe are these Edward Joplin, Robert Maultby, John Ferron, Charles Clerke, Henry Wills and Thomas Dobsonne who are to meate uppon Thursday next at 2 of the Clock in the afternoone uppon penalty of 3s. 4d. every man that doth not appeare at Georg Durham's Shopp.

Ethical Rules were enforced and the type of "breach" is not unfamiliar to this day.

May 20th, 1642. Memorandu that Robert Maultby doth acknowledge to pay into the Company ten shillings upon the aresting of him for taking a cure out of Thomas Smith's hand.

ROBERT MAULTBY.

Attendance at meetings was necessary and unpunctuality was fined, as till quite recently the Company possessed a tyler.

Insubordination during meetings was a fruitful source of income, as was also the complaints of unbrotherly conduct.

The xiiith of May, 1618. Robert Spoure find this day. and John Hall, using ill speech one to another, before the companies, iiis, iiij.

John Hall fined at this time again for using evil speech to Robert Spoure, iiis, iiijd.

April 14th, 1637.—Ordered that if any brother shall swear any oath in their Meeting House during the time of their meeting for every such offence he shall pay 6d.

9 May, 1642 (fo. 123). George Durham fined by general consent of the Companie one shilling for swearing 2 oaths.

George Durham fined for swearing by god he would drinke tobaca being at our meting 0 0 6

George Durham fined 3s. 4d. for saying he cared not a button for the Company 3 4

Jan. 1 1670 (1) Richard Todd complaynes that Wm. Green servant to Thomas Varlay that he came into his shop with out liberty and gave him unsavill languish and not only dard him to the dore but Strock at him.

May 23rd, 1687.—Richard Atkinson fined 6d. for naming the God and the Devil.

May 29th, 1689.—Thomas Todd fined 3s. 4d. for going away before the meeting arose.

June 3rd, 1698.—Peter Bell complains of John Bell for beating and abuseful words.

May 29, 1727.—Mr. Pigg complains of Mr. Wilkinson for unbrotherly words by calling him swine; fined 3s. 4d; but upon asking Mr. Pigg's pardon he is forgiven by vote.

March 6th, 1771.—Philip Stoddart fined for speaking without leave; fined a second time; fined a third time.

Jan. 8, 1771.—John Raine having desired the stewards to warn the Company to repair the hall, and not appearing himself to say anything before the Company, fined 10s. William Jameson for the same.

15 June, 1772. It was unanimously ordered that for the future, if any Brother of this Company shall appear at the Guild intoxicated, or shall otherwise behave himself rudely there, That such Brother be fined at the discretion of the Stewards for the time being, so as such fine be not less than three shillings and fourpence, and that it does not exceed six shillings and eight pence.

Mr. Hanby complains of Mr. Thos. Hall for telling him he was a lyar and Mr. Thos. Hall also complains of Mr. Hanby for Calling him a Scoundrel & a Villain Mr. Trotter complains of Mr. Hanby for calling him a mean, pitiful ffellow.

Augt. 13, 1734. Robert Locksmith Complains of Robt Shaftoe for unbrotherly words, viz. God Dame You Chattering Dog fin'd 3s. 4d. pd. returned.

Measures had also to be taken in defence of the trade, either against those who defaulted in payment, or against those who practised without due qualification.

June 14, 1742.—On complaint of Mr. Alexander Williamson it is this day ordered that no person or brother shall shave Mr. Ben Sorsbie or Mr. Willowby until they shall respectively pay what they stand indebted to the said Alex. Williamson. On Complaint of Robert Shafto the like order against shaving John Robson Mason until he pays what he owes to the said Robert Shafto.

fo 171. Ordered that Charles Clark, Robert Richardson, John Hall the elder Richard Ogle, Thomas Smith and Tho Young shall assist the stewards in goinge about the suppressing of the soldier who useth the Chandler Traid and all others who transgresse in the same or the like kind.

Nov. 19th 1675:—Stopping tallow att the bridge severall times 00 04 09.

Jany ye 10t 1683 (4)—Robert Heslope informs against Widow Bates for Letting of Blood.

Jany 6th 90—Robert Harper for setting bones and exercising the trade of a chirurgion within
91 the toune & County to be persecuted att the Company's charge.

15 June 1722.—Ordered that Robt. Green be sued for exercising the art of a Surgeon.

The apprentices were perhaps almost as troublesome as the members, and duly contributed to the funds of the Company. The apprenticeship lasted seven years, during which time marriage was forbidden. Marriage, even amongst the members, necessitated the presentation of gloves to the Stewards, Clerk and Beadle. Apparently the apprentice could serve the remainder of his apprenticeship with his master's widow, who was allowed, at all events for a time, to carry on the business.

fo. 206. 16 May 1654. James Bland has chosen his Dame Ann Anderson widdow to serve ye remainder of his yeares wth. ye consent of ye Companey dureing her now widdow hood:

Cuthbert Richardson hath chosen his Dame Ann Anderson to serve ye remainder of his yeares, with ye consent of ye Companey, dureing her widdow hood.

fo. 235. 7 Octr. 1660. I Robert Lampton doe hereby ingage my selfe unto the Company, upon my admittance Amongst them a ffree Brother, yt if it shall att any tyme hereafter be made appeare yt I have gotten any Childe or Children in the tyme of my Apprentishipp, yt I will submit my selfe unto the Companey and pay according to the usual custome for such offence committed. ROBERT LAMBTON.

Ordered that no Apprentice during his apprenticeship be allowed to marry under pain of 40/- for every such offence.

10th Novbr. 1685 by Consent of the Major part of the Company this apprentice hath leave to goe to Morpeth to sett upp shopp there for his Master use; the Master being now in Gaol. Henry Parke son of Gilbert Park of Wharton in the County of Northumberland Gentle. Appr to Robt. Rooxby of New Castle upon Tine Barber chirurgion wax and tallow Chandler for seaven yeares from Sixteinth day of July 1682.

John Hancock son of George Hancock apothecary appr to Thomas Byerly of New Castle upon Tine barber chirurgion wax and tallow Chandler seuen yeares from the fifth day of October, 1682.

August 28th 1732. Ordered that leave be given to Mrs. Jane Brankston upon her petition to practice the business of a wax and tallow chandler untill Mayday next & no longer.

June 24th, 1733. Ordered that Councells advice be taken whether Mrs. Brankston shou'd not be prosecuted for Exercising the Trade of a Chandler.

May 21st, 1728. Wm. Hilton Journeyman to Tho. Emerson pd 1s 1d. Isaac Hamilton son of John Hamilton of Whitehaven in the County of Cumberd. Gen. app. to Geo. Punshon by Indres 1 May 1728.

Tho. Akenside Son of Marck Akenside of New : Butcher app. to Do by Indres 25th April 1728 recd 31.

Octr. 12, 1731. Robt. Douglas app to John Snowden was this day admitted to his freedom & pd towards repaireing ye hall pd for his stps £3.14.1.

Recd of Do for marrying in his apprenticeship ...	...	2	0	0
„ „ Do. fo: begetting a child ...	...	...	..	2	0	0
Given back by vote of ye Company ...	...	...	...	2	0	0

June 28, 1739. The same day it was debated whether John Carrick son of John Carrick, Gentleman, an apprentice, should have the liberty to go abroad for his improvement, and ordered by the majority that he be at liberty to goe.

February 10th, 1704.—Ordered that any Brother who shall marry shall present the two Stewards, Clerk and Beadle each with a pair of new gloves upon pain of 6s 8d fine.

July 7t, 1692. Stephen Grainge promiseth to give his gloves agt next meeting. John Harrison the like.

The Company also played its part in the Municipal and Parliamentary Elections, defence of the city, and in supporting the rights of other bodies or individuals.

6 March 1689.—The Company met together to consider who they would pitch upon to be a Parliament man in the place of Sir William Blackett, he having refused to stand or be elected—they have all unanimously agreed to elect Wm. Carr, Esq., Mayor of this Town.

27th May, 1771.—It was unanimously resolved this Day that this Company shall not accept, directly or indirectly, any Donation, Present or Money, for the purpose of an Entertainment from any Candidate, or any future Election of Members of Parliament, either previous to or subsequent to such Election.

June 21st, 1740.—Ordered that the Stewards of the Company have power during the present riotous tumult in Town to appoint any number of Brethern of this Company, not exceeding six at one time, to keep watch or quell the said riotous persons, and that no person so appointed shall refuse or neglect to attennd by himself or deputy for such time as the said Stewards shall appoint under the penalty of Ten Pounds.

October 3rd, 1745.—At a meeting of the said Company held this day, it is this day ordered, by a majority of the said Company, that this Corporation have the use of the Hall, for a reasonable time, for lodging officers and soldiers, or other necessary purposes, the Corporation having undertaken to repair and make good all damages done thereby.

The riot of 1740 was caused by the dearness of corn. It was very outrageous ; the mob carried all before them, and it was found necessary to call in the military force and fire upon the crowd before their violence was stopped. The riot cost the Corporation about £4,000.

In 1771, at London, Lord Mayor Crosby and two Aldermen were sent to the Tower by the Commons, for ordering the committal of their messenger, who had seized a printer in the city. The Barber Chirurgeons were prompt to express their sentiments on that procedure.

April 9th, 1771.—At a meeting held this day it was agreed by a great majority that an address be sent to the Lord Mayor of London and the two Aldermen of that City, all now prisoners in the Tower of London.

The Minutes leave no doubt that, for a time at least, the Barber Surgeon combined his calling with that of a Tallow Chandler, and in 1711, periwig making was added :—

Decr. 11, 1711. Periwigg makeing. Its ordered by a full vote in this Company that the same be & from henceforth be accted. as a part & Branch of this Company & noe Appr. to be taken by any brother of this Company for the future in the trade of a wigg maker for lesse than Seaven yea: & be subject in every respect to be inrolled & entred & pay all fines & penaltyes as in any other Branch of the Company.

June 14, 1731. Ordered by Vote of the Company That a New Stone be sett up instead of that already fixed with these additional words Wax & Tallow Chandlers & Perukemakers.

The Surgeons possessed a set of instruments which were lent out on occasion. They were kept in a glass-fronted cupboard at the hall. This case, surmounted by the arms of the Barber-Surgeons exists to-day in the museum of the Antiquarian Society at the Castle (fig. 4). Their loan was governed by statute, and they may have been used and loaned only for the major operations. The case contains some twenty-six instruments such as a saw, needles, probes, cautery-irons, rougines, tooth and bullet forceps.

Jany. 4t 1702(3): Ordered that Mr. Haslipp pay the xlvs that Robt Kells instrumts. are pawned for & sd instrumts. to be kept by the Stewards for the use of the Brethren of this Company & to be alwasy made good and delivered back by the Bro. borrowing them in such condicon as he receives them.

FIG. 4.—Instrument Case of Barber Surgeons.

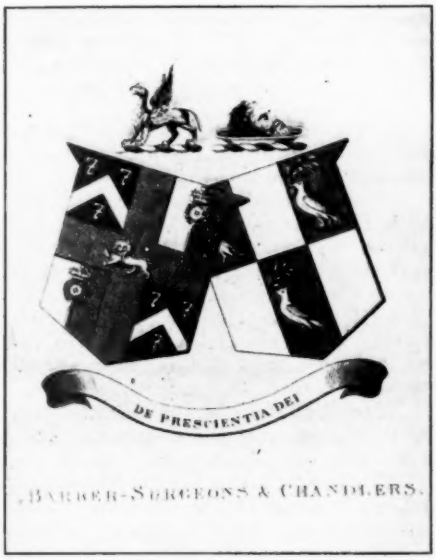

FIG. 5.

Disbrs. to May 1703. Paid Mr. Brumell for Instrumts by order of ye Company 02 05 00
Paid for Instrumts for the Company's use bought att London 01 13 00
(May xxiiijo. 1704). Instrmts. The Company having bought severall instrumts in the schedull hereunto annexed menconed Its ordered this day that the elder steward for the time being shall have always the keeping of them, and for his pains and care in lending them out, getting them in again, and keeping them cleane shall have yearly the sume of Tenn shillings, And is to lend them to none but free Brothers living in Toun who are to send a note under their hands of what Instruements they will borrow with moneys to the value of them as sett doun in the schedule and upon returning them safe in Seaven days, the moneys to be returned, but if he shall not return them again safe att or before the end of seaven days then to forfeit the full value (vigt.) the moneys deposited And the Stewards is to deliver them all in good order and very clean to the next Elder steward on the day of his electing or within a week after on the penalty of paying for what is Lost according to the

prices as sett doun. And if not delivred clean and in good order or refusing to lend them as above, is to forfeit his sallary for that yeares keeping of them.

Stewards { JNO. HALL
TH. THOMPSON.

Disbts. 1706. Painting the Instrumt Case lli expenses abt the wigg-makers 5s 9d Councillr Barns for his advice 15s Captn. Greens Surgeons mate 20s royall aid sesse 16s keeping Instrumts clean 10s.

ARMS.

The Coat of Arms (fig. 5) of the Barber Surgeons resembles that of the Barber Surgeons of London with a similar motto.

ARMS. First Shield. Quarterly: first and fourth sable, a chevron between three fleams argent; second and third per pale agent and vert, a spatula in pale argent, surmounted of a rose gules, charged with another of the first; the first rose regally crowned proper. Between the four quarters, a cross of St. George gules, charged with a lion passant gardant, or. CREST.—An opinicus, with wings indorsed, or, for the Barber-Chirurgeons. Second Shield. Per fess azure, and argent, a pale counter-changed, three doves of the last; each holding in the beak an olive branch, or. CREST.—A dish argent, glorified, or; thereon the head of St. John the Baptist, proper, for the Tallow Chandlers. MOTTO: De prescientia Dei.

The Present Position.

From the latter part of the 18th century the position of the Company seems to have declined, owing mainly, one may suppose, to the growth of Hospitals and Medical Schools, and the improvement and specialization of medicine.

With the lack of apprentices, the numbers continued to diminish till, in 1830, their Hall was vacant. The Company has been maintained but may be said to be almost "in articulo mortis." It consists at present of five members, two stewards and three ordinary members. It has no Hall—the old Barber Surgeons' Hall is now a Church School. Its meeting, still held on Trinity Monday, takes place in the Nag's Head Hotel, where even the festival dinner has been abandoned for lack of funds. When the Hall was sold, the proceeds were divided amongst the members, so that now only enough income exists to provide some liquid refreshment and tobacco.

Two Minute books, together with the copy of Bartholinus, are in the possession of the Secretary of the Company. The case of instruments is in the Museum of the Castle, and these appear to be all the material remains.

The Society has existed by the sole means of patrimony and, as no man can be made a freeman of the Company except by apprenticeship, or being the son of a member, it seems likely to die out in a few years.

The minute books were extracted by the late Dr. Embleton and my quotations from them are from an article by him in *Archæologia Æliana*.

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Section of Neurology.

[*December* 13, 1928.]

The Encephalomyelitis of Measles.

By J. G. GREENFIELD, M.D., F.R.C.P.

THE title of this paper is a challenge to two views of the nervous complications of measles which are current at the present time. The first view which I wish to combat is that these nervous complications are due to the chance coincidence of one of the common forms of encephalitis or myelitis. The second view is that the changes which are found in the nervous system of fatal cases are not inflammatory, but degenerative. I shall try to show that the encephalomyelitis of measles is, if not altogether peculiar to measles, at any rate quite different from encephalitis lethargica and poliomyelitis, and that it is an inflammatory disease, although the alterations in the nervous tissue may be chiefly degenerative.

In reviewing the history of this disease, one is struck by its extreme rarity, and by its apparent absence over long periods. Taking, for the moment, only British cases, it occurred in this country towards the end of last century, not frequently, but sufficiently often to get into the text-books. Sir Thomas Barlow described a typical case of the myelitic form, and Sir Dawson Williams one of the encephalitic form of the disease. In 1904 there seems to have been a mild epidemic and five cases were reported. But during the next twenty years it was extremely rare. Of the twenty million children in the British Isles who were probably taken ill with measles during that time, we have records of only one case of encephalomyelitis, that recorded by Box in 1920. That this paucity is probably a true one is borne out by the statements of such authorities on fevers as Claude Ker, who, writing in 1924, said that he had only seen one case of transverse myelitis, and none of encephalitis, in a series of 12,000 cases of measles, and was very doubtful as to whether measles played any ætiological rôle in these nervous sequelæ.

Since 1924, however, cases have become more numerous, perhaps not so much in this country as on the Continent and in America. In Germany, Sweden, France, and Italy, several articles have appeared detailing one or more cases, but rarely more than two have been observed by each author. In America, in three recent publications, the cases appear by eights and twelves, but these are collected from wide and populous areas. There may be some doubt as to whether all the American cases are true measles encephalomyelitis, but it is clear that there has been in the Eastern states a considerable number of cases of the disease. Neale and Appelbaum saw eight cases in the year 1926 alone, in the State of New York, and say that in their experience, measles is more often followed by encephalitis than all the other acute infectious diseases put together.

In this country only two further cases have been recorded, by Winnicott and Gibbs in 1926, but these are not typical cases. In the spring of this year, during the months of March, April, May, and June, I examined the cerebro-spinal fluid of three cases, two cerebral and one spinal. Two of these were fatal, but no post-mortem examination was allowed. In another fatal case I was able, through the kindness of Dr. Gordon Holmes, to examine the brain.

In these cases the clinical history was very similar to that which has been described by all the authors. Of this I shall only give a brief account, leaving the details to be filled in by those who have seen cases of the disease.

Clinical Types.—It is necessary for descriptive purposes to distinguish two main types, the cerebral and the spinal, although these are often combined. There is also the ponto-cerebellar type which may be considered as a sub-type of the cerebral. Both types are most apt to come on towards the end of the first week of the disease, while the rash is fading and after the temperature has returned to normal.

The *cerebral type* is ushered in by a recrudescence of fever, often with convulsions. These occurred in about half of the cases recorded by Neale and Applebaum.

Delirium, muscular twitchings and loss of speech are often observed. But the most constant and characteristic symptom is profound drowsiness or stupor, which is usually associated with neck rigidity, head retraction or tache cérébrale, and often with rigidity of all the muscles of the body. Kernig's sign is also often present. The convulsions may be so severe and frequent as to lead to the death of the patient. More often, death is directly due to lung complications; in the case which we examined post mortem it was apparently due to collapse of the lower part of the left lung. Recovery takes place in the majority of the recorded cases. The mind gradually clears, and with the return of consciousness a varying degree of damage to the brain may manifest itself in such changes of character and temperament as irritability, tendency to use bad language, and dirty habits. Usually this phase is of quite short duration, a couple of days, or a week or two at most. With further recovery there is often remarkably little evidence of cerebral disturbance, but in some cases there is permanent mental backwardness. The majority of the cases have recovered completely in the course of three or four months. Sometimes more focal sequelæ, such as hemiplegia, aphasia or ataxia of a cerebellar type follow a period of convulsions and stupor, but these symptoms may also come on suddenly, without the characteristic stupor, as they do in the course of whooping-cough and other fevers. Continued epilepsy is a rare sequela, but it has been observed in a few cases, and most often takes the form of petit mal. Convulsions may, of course, come on during the initial rise of temperature, as they may do in other acute fevers, although they seem to be rarer in measles than in some of the other exanthemata. They cannot be considered to indicate in themselves any organic lesion of the brain. It may, however, be of interest to note that some cases in which a typical attack of measles, encephalitis, or myelitis, has come on about the fifth day, have had severe convulsions at the onset.

A symptom which, although rare, is seldom met with in other forms of encephalitis is amaurosis, and this may be followed by a period of achromatopsia. Papillœdema is not uncommon and the pupils are often inactive to light during the acuter phases of the disease. Strabismus and other forms of cranial nerve palsy may occur.

In the *spinal type*, paraplegia with loss of sphincter control is the most characteristic symptom. A good account of such a case was given in the year 1790 by Mr. James Lucas, one of the surgeons to the General Infirmary at Leeds, under the heading of "An account of uncommon symptoms succeeding the measles." His case was that of an unmarried woman aged 23, who became feverish, and two days later was covered with a typical measles rash. A week after the onset she felt well and the rash was fading. On the following evening, however, "she perceived that she was deprived of the use of her lower limbs, although she had not any other uncommon complaint. The following morning she could not pass urine, and an obstinate constipation succeeded, which was scarcely to be overcome by the most powerful cathartics, administered not only by the mouth, but also by chysters." "She neither passed urine, nor had any voluntary inclination to void it until her limbs had so far recovered that she was able to move over the floor. The patient was a week before she could move a limb, and yet in two days more she was able to walk over the floor without much aid." This might pass for a case of hysteria were its main characteristics not borne out by the history of later cases, in some of which, as in the classic case published by Sir Thomas Barlow in 1886, a post-mortem examination has proved the presence of a spinal lesion.

In this spinal type, as in the cerebral form, there is often a recrudescence of fever.

The paralysis is sometimes of an ascending type. It is often accompanied by sensory loss, either to all forms, or, perhaps more often to pain and temperature only. Box's case is most interesting in this connexion, as although there was at no time any loss of sensibility to touch or of appreciation of movement of the limbs, there was such absolute loss of the sensations of pain and temperature that a hot

bottle put on the abdomen was allowed to cause a burn, the pain of which was not felt at all until power returned to the legs. The sphincters are constantly involved and in almost every case remain paralysed until the lower limbs have regained some power. The constancy of this symptom is remarkable; it is as constant in recent cases as in those of Lucas in 1790, and of Sir Thomas Barlow and Sir Dawson Williams at the end of last century. Only in very occasional cases of ascending paralysis which resemble Landry's paralysis or acute ascending polyneuritis, has it been absent. The paraplegia of measles differs from poliomyelitis thus in the constancy of severe sphincter paralysis, in the presence of sensory loss, and in the tendency for signs of pyramidal involvement, such as Babinski reflexes and hyper-tonicity, to appear after the acute phase has passed off. The degree of recovery is remarkable, and very little residual paralysis is usually left behind, although the legs may remain hypertonic and there may be for a long time some abnormal urgency of micturition.

Although it is convenient to describe the cerebral and the spinal forms of disease separately, they are very commonly combined, either one or other group of symptoms being most prominent. In the cerebral form the legs are often flaccid and weak and there is retention of urine. This may of course often be due to affection of the brain, but in some cases, weakness of the legs and paralysis of the bladder persist after the convulsions and stupor have passed off. Still more commonly the spinal form is associated at its onset with somnolence, stupor and rigidity of the neck. It is in fact impossible to classify many cases as either cerebral or spinal, as symptoms of both kinds are equally prominent.

The *cerebro-spinal fluid* has been examined in many cases and its nature has varied greatly from case to case. The pressure is often moderately raised in the cerebral form (up to 300 mm.) but is little altered in the spinal form. On the other hand the cell count tends perhaps to be higher in the spinal form. Wohlwill in a paraplegic case records 400 cells per c.mm., but such high cell counts are unusual. More often there is only a moderate excess of mononuclear cells, or even none at all. There seems to be no correlation between the acuteness of the symptoms and the degree of pressure on the one hand and the cell count on the other hand. The protein is often slightly raised but a coagulum is rarely observed. Neale and Appelbaum examined the cerebro-spinal fluid in each of their 12 cases, finding 3 to 87 cells per c.mm. with normal to high sugar values. They noted that sometimes there was cell-protein dissociation, sometimes the reverse. In my own examinations I found (1) 15 cells with 40 mgm. of protein and 80 mgm. of glucose; (2) 31 cells with 55 mgm. protein and 123 mgm. of glucose; (3) 6·5 cells with 50 mgm. protein (in the spinal case).

Morbid Anatomy.—A very good account of the changes in the nervous system of their case was given by Barlow and Penrose. The lesions found by them, although considered anomalous by many subsequent writers, agreed well with what has been found in some recent cases which have been carefully examined and with the appearances in the brain of our case. They found the changes to be evenly distributed throughout the cord but of greater intensity in some regions than in others. In addition to great engorgement of the vessels and some scattered petechial hæmorrhages, many of the vessels were surrounded by a zone of coagulated exudation, and beyond this for a considerable distance the tissues were infiltrated with leucocytes giving the appearance in transverse sections of a series of concentric rings. No changes were found in the nerve cells. This appearance of perivascular softening with cell exudate forming a concentric zone round the vessels has been confirmed by Siegmund in a case of Brock's, by Wohlwill in two cases and by myself. The cells composing the exudate have been variously described and interpreted, and no doubt vary to some extent from case to case. In early cases, as in Barlow's and our own, the cells may resemble lymphocytes or polymorphonuclear cells. In later cases there is more evidence of fat phagocytosis and many of the cells are definitely fat granule cells; others are swollen neuroglia cells. There may or may not be, in

addition, actual infiltration of the perivascular sheath with plasma cells and small round cells. This was abundantly present in our case, but does not appear to form an essential part of the picture. What seems to be *the most characteristic change* is the *zone of demyelination* of greater or less width *around many of the small veins*. Wohlwill, in a careful study, notes that in both brain and cord, as far as he could trace it, this perivascular demyelination extended along the whole length of a vein. He could not see a vein passing into or out of a zone of demyelination as one can in disseminated sclerosis. That is to say that these areas are elongated rather than spherical, and only appear circular when a vessel is cut transversely. In the cord he could trace the demyelination along a vein from the grey matter to the surface, for, as he notes, the veins are not so strictly limited to grey or white matter as are the arteries. In his cases however, and in Brock's, there was practically *no mesodermal reaction*.

This variability of mesodermal reaction seems to have led to considerable confusion. The question has been raised whether it is possible to have a form of inflammatory disease of the brain in which there is no evidence of cell exudation. And, as I indicated before, certain observers have considered that the changes found in the encephalomyelitis of measles are of a purely degenerative character and are not inflammatory. But when we consider the acute febrile onset of the nervous complications, the high-pressure of cerebro-spinal fluid, the frequent presence of papillœdema, and the congested and sometimes hæmorrhagic appearance of the brain and cord, one finds all the cardinal evidences of inflammation, "rubor, calor, turgor."

The similarity of the disease to post-vaccinal encephalomyelitis is seen not only in the morbid anatomy. In both diseases there has been a great increase in the number of cases during recent years. In both diseases also there is a definite delay in the onset of nervous symptoms till after the acute phase of the initial disease has passed off, and the time interval from vaccination or measles rash to encephalitis is remarkably constant. These facts seem to confirm the suggestion which has already been made as regards post-vaccinial and measles encephalomyelitis, that the virus which causes the encephalitis is not the same as that causing the exanthem, but is a special neurotropic virus which is stimulated to activity by the exanthematous disease.

It is impossible to say, without further evidence, that the encephalomyelitis of measles is identical with post-vaccinal encephalomyelitis. All one can say is that in some cases no clinical or pathological differentiation is possible. Whether they also have a relationship to the other demyelinating diseases (disseminated sclerosis, diffuse myelitis or Devic's disease and Schilder's encephalitis) is a question which at present it is easier to put than to answer.

In conclusion, one may say quite definitely, both on clinical and on pathological grounds, that the typical lesion of the encephalomyelitis of measles is not a "hæmorrhagic encephalitis." Certainly there have been petechial hæmorrhages in some cases, due to the intense congestion of small vessels, and some atypical cases have been frankly hæmorrhagic. But in other cases, as in ours, no hæmorrhage or thrombosis has been found. Nor is it possible to understand the complete and rapid disappearance of severe clinical symptoms on the ground of a hæmorrhagic encephalitis or myelitis. In demyelinating diseases, on the other hand, complete recovery is a commonplace. Our case, added to those of Barlow, Brock and Wohlwill, therefore definitely places the encephalomyelitis of measles among the diffuse perivascular demyelinating diseases, and relates it very closely to post-vaccinal encephalomyelitis, and this view is confirmed by the clinical and epidemiological similarity of the two diseases.

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The Pseudo-Polyneuritic Type of Amyotrophic Lateral Sclerosis.

By R. M. STEWART, M.D.

ONE of the most interesting types of amyotrophic lateral sclerosis is the form in which the lower motor neurones are severely affected without clinical evidence of involvement of the cortico-spinal motor path. As a rule, this pseudo-polyneuritic type pursues a rather chronic course, but in the following case the progress of the disease was unusually rapid, the patient dying almost exactly twelve months after the onset of symptoms.

Clinical History.

On January 2, 1926, a male imbecile, aged 50, was observed to be limping. On examination he showed drop-foot on the left side, with almost complete paralysis of the anterior tibial muscles. The left knee-jerk and both ankle-jerks were abolished. Three months later the clinical picture was very different. There was now a flaccid atrophic paralysis of both lower limbs, with marked fibrillar contractions and partial R. D. in the affected muscles. Though the patient complained of tingling and cramp-like pains in the legs there was no objective disturbance of sensation. The Wassermann reaction was negative in both blood and cerebro-spinal fluid. The disease progressed rapidly, and at the end of nine months there was marked paresis of the small muscles of the hands and the flexors and extensors of the forearms. Until within twelve days of death the sphincters remained unaffected, and at no period of the illness was there any evidence of involvement of the muscles innervated by the cranial nerves. Death from asphyxia occurred on December 20, 1926.

Two additional points require mention. When the patient first came under observation, several discharging rectal sinuses were discovered, and throughout his illness there was a polymorphonuclear leucocytosis of about 18,000 cells.

The post-mortem examination was held twelve hours after death.

Histological Examination.

The microscopic appearances may be summarized as follows:—

Sections of the left gastrocnemius muscle show the typical atrophic changes of amyotrophic lateral sclerosis, the muscle fibres being diminished in calibre, and their nuclei increased in number (fig. 1). In the muscle fibres of the diaphragm, which became paralysed a few days before death, degeneration is in an early stage, the most obvious feature being a proliferation of sarcolemma nuclei.

In Bielschowsky preparations of the left external popliteal nerve it is difficult to detect any loss of axis cylinders (fig. 2), but in Marchi sections there is obvious blackening of myelin sheaths (fig. 3). With the Weigert-Pal method considerable difficulty was experienced in obtaining satisfactory results, but sections stained by the old osmic acid method of Heller show in a rather crude way that certain bundles fail to take the stain as intensely as other and presumably more normal, fibres.

In sections of the extramedullary ventral root fibres from the fourth lumbar segment there is a slight but obvious degree of Marchi reaction, which is of a fairly late character (fig. 4).

In the fibres of the posterior nerve roots little or no degeneration can be found, but in the posterior root ganglia many of the nerve cells are distinctly morbid (fig. 5). Some are shrunken and irregular in shape, while others have their nuclei almost extruded. It is interesting, too, to note that similar changes are present in the cœliac ganglion.

In the cord the changes are very striking. Marchi sections show a widespread myelin degeneration, which involves practically all the columns (fig. 6). Degenerated myelin sheaths can be traced from the ventral horns to the margin of the cord; they are present in the anterior commissure, and can be seen in the root entry zone passing into the posterior horn of grey matter. In the posterior columns degenera-

tion is pronounced and its tabetiform character is more clearly seen in Weigert-Pal preparations of the lumbar segments (fig. 7), which also illustrates the severity of the degenerative changes in the antero-lateral columns.

In the grey matter of the lumbo-sacral enlargement few anterior horn cells have survived. Those that remain are altered in shape, the Nissl granules surrounding the nuclei are undergoing dissolution and the nuclei are displaced to the extreme

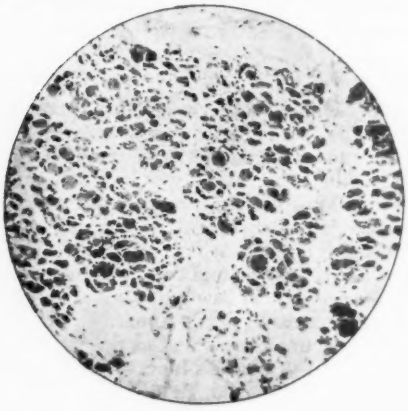

FIG. 1.

FIG. 2.

FIG. 1.—Advanced atrophy of fibres of left gastrocnemius muscle.
FIG. 2.—Normal axis cylinders of left external popliteal nerve. Bielschowsky stain.

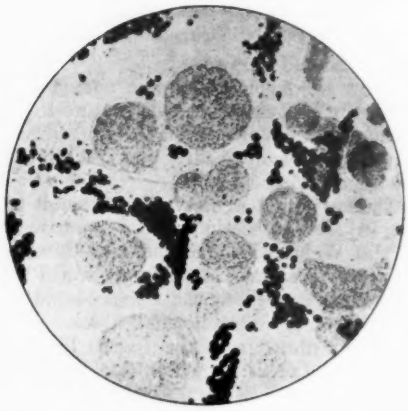

FIG. 3.

FIG. 4.

FIG. 3.—Degenerated myelin sheaths, left external popliteal nerve. Marchi stain.
FIG. 4.—Extramedullary ventral root-fibres showing myelin degeneration. Marchi stain.

edge of the cell bodies (fig. 8). Many of the cells contain masses of pigment, and in the ventral horns vascular engorgement is a prominent feature. Round some of the vessels there is a marked infiltration by small round cells (fig. 9). In this region of the cord the pia-arachnoid is thickened and spider cells are present in great numbers in the lateral columns.

In the dorsal segments the changes revealed by Marchi's stain are still pronounced, but not so intense as in the lumbar cord.

Marchi granules are numerous in the position of the direct and crossed pyramidal tracts and some degeneration is present in the posterior columns; a few blackened fibres can also be seen in the anterior nerve roots. In the base of the anterior horns there are small hæmorrhages, but in view of the patient's death from asphyxia, their pathological significance is doubtful. Weigert-Pal preparations of the dorsal segments have an appearance which is fairly typical of amyotrophic lateral sclerosis,

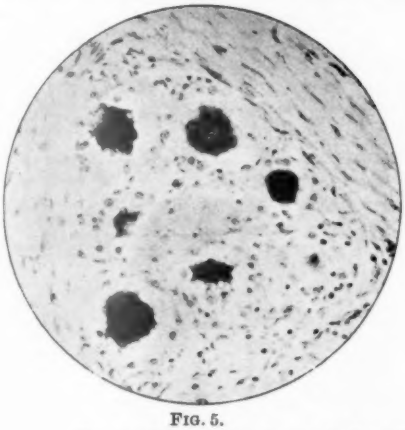

FIG. 5.

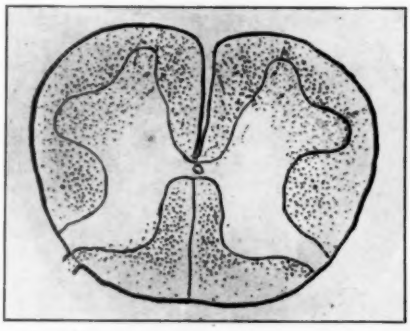

FIG. 6.

FIG. 5.—Degenerative changes in the cells of posterior root ganglion.
FIG. 6.—Diagram of first sacral segment showing distribution of myelin degeneration.

FIG. 7.

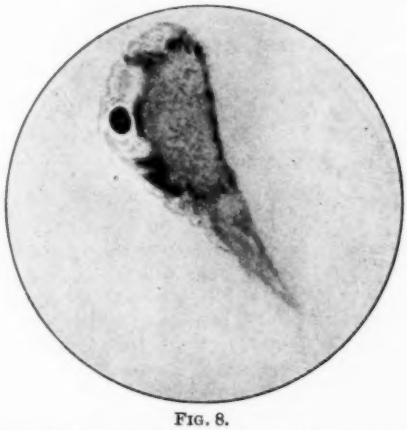

FIG. 8.

FIG. 7.—Lumbar cord. Weigert-Pal stain showing degeneration in ventro-lateral and posterior columns.
FIG. 8.—Nerve cell from ventral horn of fourth lumbar segment.

there being a diffuse degeneration of the ventro-lateral columns (fig. 10). In the posterior-median septum there is a slight degree of round-celled infiltration (fig. 11).

In the cervical enlargement degeneration of myelin sheaths is less obvious (fig. 12), and in sections stained by the Weigert-Pal method the posterior columns appear almost normal. Nerve-cell degeneration is still pronounced, although here and there one meets with cells presenting a normal appearance.

Of the peripheral nerves derived from the cervical cord, the ulnar shows a very slight myelin degeneration, and the phrenic a more marked degeneration of nerve fibres.

In the medulla oblongata the nerve-fibre degeneration cannot be traced above the decussation of the pyramids, and no pathological changes can be detected in the cranial nerves. The cells of the hypoglossal nucleus are normal, but those of the nucleus ambiguus show changes of a degenerative character.

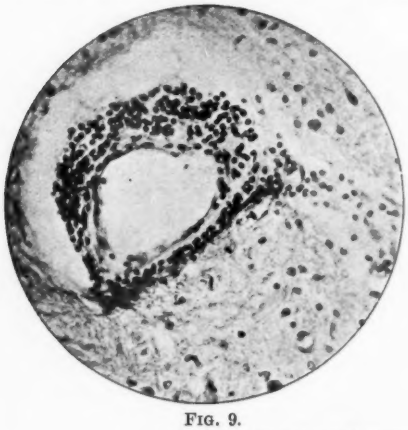

FIG. 9.

FIG. 10.

FIG. 9.—Perivascular infiltration of grey matter. Lumbar cord.
FIG. 10.—Mid-dorsal cord Weigert-Pal stain.

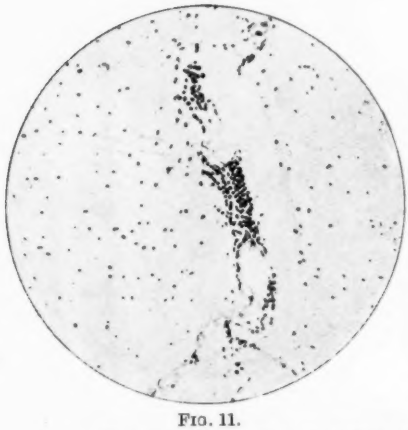

FIG. 11.

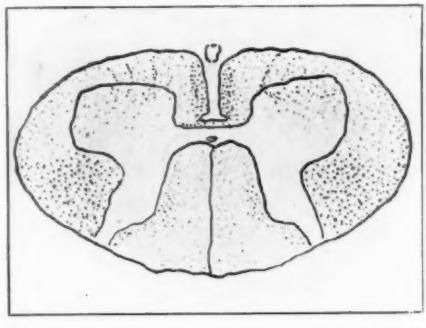

FIG. 12.

FIG. 11.—Mid-dorsal cord. Cellular infiltration in the postero-median septum.
FIG. 12.—Diagram of cervical cord to show distribution of myelin degeneration.

Perivascular infiltration is again evident.

No degeneration of nerve fibres can be found in the pons or midbrain, but a few Marchi granules are present in the posterior limb of the internal capsule and in the left precentral gyrus a few of the projection fibres stain black by Marchi's method.

The Betz cells appear to be present in their normal numbers. For the most part

they are normal, but a few exhibit typical axonal reactions (fig. 13). Although no evidence of perivascular infiltration can be found, many of the capillaries are surrounded by pigment which stains metachromatically with toluidin blue, and in certain situations, black by Marchi's method (fig. 14).

Conclusions.

The histological findings of most interest are those encountered in the lower segments of the cord. In this situation many of the nerve cells are destroyed, while the myelin sheaths of the nerve fibres are in an advanced stage of degeneration.

Although it has long been recognized that amyotrophic lateral sclerosis is not essentially a disease of the motor system, the posterior columns have seldom been found degenerated, and never, I believe, to the extent present in this case.

It is a moot point whether the degeneration of the pyramidal fibres in amyotrophic lateral sclerosis is primary or merely secondary to involvement of the cortical cells, but I think there can be no doubt that in my case the myelin degeneration set in earliest and was most complete in the distal segments of the

FIG. 13.

FIG. 14.

FIG. 13.—Betz cell showing axonal reaction.
FIG. 14.—Pigment stained black by Marchi's method surrounding a cortical capillary.

cord, from which it was propagated in an ascending fashion. Although the evidence is of a less conclusive nature, I am also inclined to believe that in the peripheral nerves the degeneration also proceeds in a centripetal fashion.

An interesting feature in this disease is the frequent absence of all evidence of degeneration in the extramedullary portion of the anterior nerve roots, even when pronounced Marchi reaction is present in these fibres as they pass from the ventral horns to their place of exit. This peculiarity is not, however, confined to amyotrophic lateral sclerosis, for precisely the same appearances were found by Orr and Rows [1] in clinical cases of lymphogenous infection of the nervous system. In their own words: "The extramedullary portion of the anterior and posterior . . . . . roots was examined for degeneration by Marchi's method and none was found, but a considerable degree of degeneration was observed in connexion with the intramedullary portion of the anterior nerve roots from the cord periphery to the grey matter."

Now if, as appears to be the case, toxins can ascend along the perineural lymphatics without producing marked parenchymatous changes in the nerves, the

question arises: May not this type of amyotrophic lateral sclerosis be caused by the existence, in some part of the body, of a living agent from which toxins are conveyed to the nervous system?

In my patient the persistent leucocytosis and the degeneration of the posterior columns in a region of the cord which corresponded to the nerve supply of the focus of disease near the rectum, is certainly suggestive, but I am bound to admit that there is little support in the literature for the theory of a lymphogenous infection. Trétiakoff and Amorius [2] report the case of a woman suffering from advanced intestinal tuberculosis who developed amyotrophic lateral sclerosis, and they suggest that the nervous disease owed its origin to the passage of toxins from the diseased intestine to the cord, but with this exception I have been unable to find any records of definite foci of infection in association with amyotrophic lateral sclerosis.

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Section of Obstetrics and Gynæcology.

[*November* 16, 1928.]

The Age Incidence of Carcinoma of the Body of the Uterus.

By Archibald Donald, M.D., and William Fletcher Shaw, M.D.

The symptoms and signs of cancer of the body of the uterus seem to us to offer a clear-cut clinical picture, such as comparatively few gynæcological subjects present. The age incidence of the disease, its symptoms, its type and its course, all seem to vary within narrow limits, and in these features it is in sharp contrast with cancer of the cervix.

It is not our intention to review in this paper the disease as a whole, but merely to direct attention to two points: (1) the age incidence, and (2) the relation of the onset of the disease to the menopause. We have been led to do so by the appearance lately of two papers, both of which are based on statistics which to us appear misleading. We refer to the pamphlet issued by the Ministry of Health dealing with the statistics of cancer of the uterus generally, and to a paper recently published in the *Proceedings of the Royal Society of Medicine*. Our belief and our teaching for many years have been that, with the rarest exceptions, cancer of the body of the uterus does not begin in subjects under 40 years of age or, alternatively, until after the retrogressive changes of the menopause have set in. Both of the papers mentioned above contain conclusions which are at variance with the results which our statistics reveal.

With the object of investigating the truth of our belief, we have collected 177 cases, which are summarized in the following table. All of these cases have been under the care of one or other of us. With the exception of twenty, only those cases in which the uterus has been removed, and has been examined after removal, have been included. In all of these twenty cases the disease was so far advanced that complete removal was out of the question. In six the use of radium was the only treatment; in eleven the abdomen was opened and secondary growth was found to be widespread and in three others the curette was used. In the radium cases and in the cases in which the curette was used, substantial pieces of growth came away. In no case was a diagnosis made only by microscopic examination of small pieces of tissue removed by the curette. The notes of the cases were obtained from the hospital register and from case books, and in the majority these were supplemented by notes which were taken by ourselves. No case has been included in which there has been the slightest element of doubt as to the nature of the disease. As a result of these restrictions, a considerable number of cases have not been included, as the notes, some of which go back as far as 1893, were so incomplete that the nature of the cases was more or less doubtful. The operations performed were vaginal hysterectomy and supra-vaginal hysterectomy in the earlier years, and panhysterectomy and Wertheim's operation in later years.

[Here followed the table of cases with initials of patients, age, residence, date of operation, period of amenorrhœa (if any), and complications (if any).]

Age Incidence.

This is best summarized in the following table:—

Decade	Number	Percentage
From 30-39	1	0·5
,, 40-49	21	11·8
,, 50-59	89	50 (or one half)
,, 60-69	59	33 (or one third)
,, 70-79	7	4
	177	

In the decade from 30-39 inclusive, there is only case (98), and even in this there is some doubt as to the point of origin of the growth. A supra-vaginal hysterectomy was performed for what was taken to be an enlarged, subinvoluted uterus, and it was

only when cutting through the cervix that the growth was discovered. The main mass of growth was found in the uterine body, and in the notes the case is entered as one of carcinoma of the body, but as the growth was found in the cervix when this was cut across, there is the possibility that the growth originated there and not in the uterine body.

The specimen has been destroyed, so it is impossible to re-examine it; hence we are including this case as one of carcinoma of the body at this early age, and before the menopause, though there is considerable doubt as to whether the body was really the point of origin.

In the decade of 40-49 inclusive, there were four cases in which cancer of the body of the uterus was associated with fibroids (2, 38, 69, 168). In three of these four cases of fibroids the patients were under forty-six years of age.

Relation of the Incidence of Cancer of the Body of the Uterus to the Menopause.

Unfortunately definite information is not available in every case, but the date of the menopause was noted in 147 cases. In four of these the menopause had not occurred. In two of the four cases the uterus was much enlarged by fibroids (38 and 168). There were, therefore, only two uncomplicated cases out of 147, or 1·3 per cent., in which the disease began before the onset of the menopause —one of these was a spinster aged 45 in whom regular menstruation has gradually merged into continuous hæmorrhage (145), and the other was a married woman of 35 (98) whose case is mentioned in a preceding paragraph as being probably one of primary cancer of the cervix.

The evidence from the thirty cases in which the onset of the menopause was not noted points in the same direction. In twenty-five of these the patients were aged 49 years or over—an age at which it is probable that retrogressive changes have begun. Of the five remaining patients two had fibroid tumours. The ages of the remaining three were 45, 46 and 49.

Summary.

As far as our joint experience goes, the result of our investigations is that in women under 40 years of age, primary cancer of the body was only found in one case out of 177, or about 0·5 per cent., and that the disease was pre-menopausal in only two cases, or 1·3 per cent.

These conclusions are incompatible with those arrived at in the two recent publications to which we have referred. For this reason we have thought it of importance to investigate the statistics contained in these two publications.

Report on Cancer of the Uterus issued by the Ministry of Health, 1927.

We have hesitated to criticize the statistics of others, especially statistics which have involved so much time, labour and expense as those in this report, but we must frankly state that we regard the figures dealing with the age incidence as completely misleading. When we compare the statistics in the report with our own we find a great difference. In the former we find that out of a total of 427 cases the disease is stated to have occurred in five patients under 30 years of age, as compared with none in our tables, and in thirty-three cases under 40, as compared with 1 in our tables, or 7·7 per cent. as compared with 0·5 per cent.

If statistics are to be of any value it is essential that those who collect them should carefully scrutinize the individual cases and the source from which they are obtained. A mere compilation of figures, without such scrutiny, is of no value. Considering the doubt as to whether carcinoma of the body of the uterus ever occurs in young women, one would have expected that all those who have recorded such cases would have told us something of their clinical histories and would have reported fully on the specimens obtained. In the references contained

in the report, which we have been able to look up, we can find no evidence of such steps having been taken. Thus, Jessett, Peterson and Meigs collectively contribute 147 cases, or about one third of the total number. Of these, seventeen cases, or 11·5 per cent., were under 40 years of age. Jessett (*Brit. Gyn. Journ.*, 1899) gives a list of twenty-one operations for cancer of the body of the uterus. This is a bare record in tabular form, with no particulars, published over twenty-nine years ago. In five the patients were aged under 40 (or about 23·8 per cent. of the twenty-one cases). Peterson's statistics (*Surg. Gyn. Obst.*, 1919) are derived from various sources, namely, the University, the Hospital and Laboratory in Michigan, and were presumably furnished by various operators. He states that the diagnosis in many cases was made only from the microscopical examination of scrapings. No details are given. He reports ten cases of patients, aged under 40, out of a total of ninety-four, or 10·6 per cent. We have been unable to obtain the original contribution of Meig's, but there is a summary of his article in the *Journal of the Medical Association*, October, 1922. In this there are no details, but he states that cancer of the body is more common in general practice than cancer of the cervix—a statement that is so much at variance with the opinion of practically all other authorities that we are unable to accept his figures. He reports two cases of ages under 40, out of a total of thirty-two, or 6·5 per cent.

It seems strange that some important and easily accessible communications by British authorities should have been omitted—for instance, the figures given by Wilson (Eden and Lockyer's "System of Gynæcology," 1917), and by Mrs. Garrett Anderson and Miss Platt (*Journ. of Obst. & Gyn. Brit. Emp.*, 1908) are not included. The first named records fifty-six cases with none under 45, and the two latter thirty-five cases with none under 40. But on the other hand we have Leopold (five cases) and Busse (three cases) from Germany, Cholmogoroff (two cases) from Austria, and Lauwers from France. The last named records twelve cases, with three, or 25 per cent., under 40.

In consideration of these facts, is it surprising that we should regard the conclusions arrived at in the report as of little or no value?

Communication on the Age Incidence of Carcinoma Corpus Uteri, by A. C. Palmer, F.R.C.S. (*Proc. Roy. Soc. Med., January, 1928*).—Mr. Palmer, in his paper, quotes fifty-six cases published by Professor Wilson, and fourteen cases by Mr. Gilliatt. The youngest patient in Wilson's table was 47; in Gilliatt's table the youngest was 39. Thus, out of seventy patients, only one was under 40, or 1·3 per cent. Mr. Palmer analyses 250 cases collected from the London Hospital and King's College Hospital, supplemented by Mr. Gilliatt's cases, but not with Professor Wilson. The result gives six cases under 40, or 2·4 per cent.

Mr. Palmer is free from the reproach of not having investigated the individual cases in patients under 40 and he gives notes of the five cases of this kind which are included in the figures of the London Hospital, but these notes appear to us not only to be incomplete but also to convey doubt as to the diagnosis. Thus, in Case I, the patient was an unmarried woman, aged 19, who had a mucous polypus projecting from the cervical canal. This was removed flush with the endometrium. No other operation was performed and there had been no recurrence in five and a half years. The belief in the malignancy of the growth was based only on microscopical evidence ("solid trabecular tubal polygonal-celled carcinoma"). It is hard to believe that a mere prolongation of the endometrium (which constitutes a polypus) could be removed "flush with the endometrium" without leaving malignant elements behind. The fact that there was no recurrence of the growth within five and a half years after such a minor operation is, to us, the strongest clinical evidence that the growth was innocent. In Case II the patient was aged 31, the diagnosis was made from scrapings, and on laparotomy, hysterectomy was found to be impossible owing to

fixation of the uterus. Fixation is such an unusual complication in the ordinary type of cancer of the body of the uterus that one suspects that the growth involved the upper portion of the cervix and may have originated there. In Case III the patient was aged 33, the diagnosis was made from scrapings and the uterus was removed. No account is given of the appearance of the uterus and the patient has not been traced. Cases IV and V occurred in patients aged 39, and it is possible that in both the disease was post-menopausal. The uterus was removed in each case, but no account is given of specimens.

We should like to emphasize the fact that the 177 cases in our table have all been under the supervision of one or other of us, and we have not included cases drawn from other sources.

Sources of Possible Fallacy.

In reviewing our cases we have had to reject a certain number, as it seemed to us that these were not genuine cases of primary cancer of the body of the uterus, or that the nature of the disease was open to grave doubt.

(1) *Sarcoma.*—While sarcoma is included under the heading of malignant disease, it cannot be classified as cancer or carcinoma. Sarcoma is not governed by any laws; it may occur in any organ and at any age from early childhood. There is no distinction drawn in the report we have referred to and it is possible that many of the cases in young people which were included by other observers as cancer were really cases of sarcoma.

(2) *Cancer of Cervical Origin spreading into the Uterine Body.*—We have noted that in one case which was reported as cancer of the body of the uterus in a patient aged 35, the origin was probably in the upper part of the cervical canal. We had several specimens in older women in which the malignant growth involved the deeper tissues of the uterus from midway in the cervical canal to midway in the body of the uterus and these seemed to be all primarily cervical in origin.

(3) *Diagnosis Solely from Microscopic Examination of Scrapings.*—It is our opinion that unless substantial masses of growth have been removed or the microscopic findings confirmed by examination of the whole uterus after removal or by subsequent clinical history, the case must be regarded as doubtful. The diagnosis of malignancy is often made from small scrapings of endometrium by a pathologist who knows nothing of the clinical history and who has not a very extensive experience in uterine pathology.

We have been informed by eminent pathologists that it is a matter of extreme difficulty to give a definite opinion on isolated fragments of tissue from the uterus, and some of those whose opinions may be regarded as most valuable refuse to commit themselves. There is also always the possibility that the fragments have come from the upper part of the cervical canal and not from the uterine cavity. It is true that many observers have pinned their faith to this single test and in America it seems to be regarded by some as infallible. Peterson is one of these. In explaining the way in which his table of cases was compiled, he states:—

> "Many cases admitted from University and private clinics were undoubtedly uterine cancer, the diagnosis being so easily made by the appearance of the growth that no tissue was sent to the pathologist for confirmatory diagnosis. In many cases the subsequent death of the patient was recorded in notes leaving absolutely no doubt as to the correctness of the clinical diagnosis."

And yet:—

> "It was thought best not to include such cases in the list to be analysed."

A microscopic examination of a mere fragment of tissue was apparently regarded as a better test than a typical naked-eye specimen of an advanced growth, even when the disease advanced to a fatal issue.

In 1924 Norris reported 116 cases of cancer of the body of the uterus and in many of these the microscope was the decisive factor (*Amer. Journ. Obst. and Gyn.*, 1924). Fifteen of the patients, or 13 per cent., were under 40 years of age. He remarks that "accuracy in histological diagnosis is possible in nearly 100 per cent. of the cases," and later, that "occasionally doubtful histological pictures will be interpreted with difficulty by even the most experienced pathologists," and he seems to see no discrepancy between these statements. A little doubt as to the accuracy of histological diagnosis may be aroused by his statement that "a number of cures of carcinoma by curettage are on record, and may undoubtedly occur." Scattered through gynæcological literature in America and Europe are many cases in which the diagnosis of cancer of the body of the uterus has been made only from a microscopic examination of minute fragments obtained from the uteri of women between 20 and 40 years of age.

Uterine Fibroids and Cancer of the Body of the Uterus.—In the course of our investigations we have found that these two conditions have co-existed in a number of cases. There is no doubt that the existence of fibroids in the uterus may delay the onset of the menopause. Fibroids associated with malignant growths may be divided into two classes:

(1) Those in which the malignant growth has been found in the substance of the uterus, and (2) those in which there is a definite glandular cancer, originating in the endometrium. In the former class the growth is probably sarcomatous, as the relation between fibroid tumours and sarcoma is well established. In the latter class the association may be a mere coincidence, but there is one striking circumstance which we have observed, and which has been mentioned by other writers. In no less than three cases of the five, between 40 and 44 years of age, we found fibroids of the uterus and glandular cancer of the body. This seems to suggest that there may be a causal relationship between the two conditions.

Cancer of the Body and the Menopause.—In discussing the possible fallacies we have so far been dealing with age incidence alone, but this in itself gives us no information as to whether the disease began before or after the menopause. It is quite possible for the retrogressive changes of the menopause to begin under the age of 40, and instances at the age of 37 are not very uncommon. In these cases the woman is sexually older than her years. In our tables there were two patients, aged respectively 41 and 42, who had symptoms of an early menopause.

General Conclusions.

It may seem to some that we have gone to considerable trouble over a question of comparatively little importance. Our inquiry has been undertaken for two reasons. We believe, as we have already stated, that cancer of the body of the uterus differs from most other forms of cancer in that it almost invariably attacks an organ in which retrogressive changes have begun or are well established. It may be that this peculiarity may be of help in the investigation of the disease. Our second reason is concerned with the teaching of gynæcology. There is no doubt that the two statistical publications we have referred to will have a widespread influence. The figures will find their way into text-books written for students and practitioners and will, as is the custom, be repeated in later text-books. The students and practitioners will have their attention directed to cases which are hardly ever found. All of us who have acted as examiners know how ready the student is to fasten on rare and exceptional cases. With a view to these examinations the text-books include everything, but the student does not see things in the proper perspective, and every fact seems of equal importance. He cannot see the shape of the wood because of the trees. It is better, in our view, that each clinical picture should be clear cut, that the essential characters of ninety-nine cases in 100 should be emphasized, even if the hundredth case should be omitted. If this were the

general method we should have fewer tragedies caused by the failure to recognize a few simple features. For practical purposes it is essential that every practitioner should realize that uterine bleeding after the menopause, if it does not come from the cervix or a cervical polypus, indicates cancer of the body of the uterus in the majority of cases.

Discussion.—Dr. ARTHUR GEMMELL: Thanks to the courtesy of all my colleagues at the Hospital for Women, Liverpool, I have been able to go over the case sheets and pathological reports of all the cases at that hospital during the last ten years. There is a total of seventy-three cases. The age incidence of these cases is as follows:—

Age	Number	Percentage
35-40	2	2·7
40-45	6	8·2
45-50	8	10·9
50-55	15	20·5
55-60	33	45·2
60-65	8	10·9
65-70	1	1·3

We regard carcinoma of the body definitely as a post-menopausal disease unless it is present as a complication of fibroids which may themselves postpone the menopause. In this connexion it is interesting to note that of the fourteen patients between the ages of 40 and 50 the carcinoma of the body was associated with fibroids on four occasions, and two other patients had come to the menopause three years or more before there were symptoms of the carcinoma.

The two patients between the ages of 35 and 40 call for some comment. One, aged 39, had had her ovaries removed and thus an artificial menopause produced eleven months before the onset of symptoms referable to the carcinoma.

In the case of the other patient, aged 37, the diagnosis of carcinoma corporis has only been accepted after much consideration of the clinical and pathological aspects of the case. She had had a daily brown discharge for four or five months, with copious menstruation lasting fourteen days. A diagnostic curettage was performed and copious débris removed. Professor Glynn reported these as strongly suggestive of adeno-carcinoma. On this report a total hysterectomy was carried out. On opening the uterus, which was thick walled, there was seen on the anterior wall an irregular, moderately firm but friable growth, roughly triangular in shape with its base at the fundus and its apex almost reaching the cervix. Sections through this growth appear to show an adeno-carcinoma as evidenced by the invasion of the uterine muscle, the heaping up of the epithelium in places, and traces of a round-celled deposit.

Dr. ARTHUR GILES said that the subject of cancer of the body of the uterus was one in which he had long been interested; and at the present time he was engaged in tracing the late results of hysterectomy for this condition. In view of the present paper he had looked up the age-incidence in his own cases, with the following result:—

	Age				
	Under 40	41-50	51-60	61-70	Over 70
Number of cases	4	10	38	20	4
Percentage	5·1	13·1	50·0	26·3	5·1

These results corresponded closely with those given in the paper, since the patients of between the ages of 51 and 60 formed exactly 50 per cent. of the total, and those between 61 and 70 formed over 26 per cent. There were four patients under 40 years of age; but one of those had also carcinoma in the upper part of the cervical canal. He had himself always regarded cancer of the body of the uterus as essentially a post-climacteric disease, and was surprised to find in his list as many as four patients under 40. The association of fibroids with this condition was very interesting: but in view of the wide prevalence of fibroids it was not surprising to find the combination. There could, of course, be no question of a carcinomatous degeneration of fibroids, but the presence of these tumours might well act as a predisposing cause; and it would be found that when cancer of the body of the uterus was present before the menopause, in a certain number of the cases there would also be fibroids.

The view developed in the paper, that carcinoma of the body was a disease associated with retrogressive changes, received support from the consideration of the incidence in

nulliparæ. This was in marked contrast to carcinoma of the cervix which occurred essentially in damaged tissues. The result was that cervical cancer was very rare in unmarried women; but in his series of cases of cancer of the body, eighteen, or 24 per cent., were in unmarried women, and about an equal number of the married women were nulliparæ.

With regard to the reliance to be placed on histological diagnosis, he appreciated the great help derived from the pathologist; but pathological findings were not so unerring as to lessen the importance of clinical evidence. Where the two were not in agreement he would be disposed to be guided by the clinical evidence when deciding on treatment.

Dr. F. J. McCANN said that he was in general agreement with the conclusions reached by the authors of the paper, but desired to know if they had excluded the possibility of some of their cases being examples of cancer beginning in the cervix and extending upwards to the body of the uterus. It was difficult (in some instances impossible) to be certain whether the disease had extended upwards from the cervix or downwards from the body. When preparing his book on "Cancer of the Womb," he collected some statistics on the age incidence, and found that the greatest number was between 45 and 55 years, the average age being 53.

Uterine cancer and uterine fibroids were common diseases, hence their existence in the same uterus was not rare, but the more frequent combination of cancer of the corpus with fibroids suggested a causal relationship. It was more often the smaller variety of fibroids which was associated with corpus carcinoma, and, in all probability, the changes in the endometrium, so frequently found in association with fibroid growths, predisposed to cancerous invasion.

Professor PHILLIPS (Sheffield) communicated his statistics which are as follows:—

60 Cases of Carcinoma of Corpus Uteri.

Age	*Average age*	...	57 years, 4 months
	Youngest	... ...	40 ,,
	Oldest	... ...	73 ,,
Incidence	...	...	40-45 years (8·3 per cent.)
	...	...	45-50 ,, (10·0 ,,
	...	...	50-55 ,, (15·0 ,,
	...	...	55-60 ,, (28·3 ,,
	...	...	60-65 ,, (18·3 per cent.)
	...	...	65-70 ,, (15·0 ,,
	...	...	70-75 ,, (05·0 ,,

Parity. *Nulliparæ* $\frac{30}{60}$ = 50 per cent. (of which 11 were unmarried, i.e., 18·3 per cent.)

Mr. McKIM McCULLAGH said he agreed that, so far as the Ministry of Health Report on the treatment of Cancer of the Uterus at the Samaritan Free Hospital was concerned, the statistics were not absolutely accurate, owing to incomplete notetaking during the war period. In this Report, 207 cases of cancer of the body, occurring between 1901 and 1920, were analysed, and in 5·79 per cent. of these the patients were under 40 years of age.

This was possibly an excessive proportion, but he would plead for massed British statistics, which he thought might be collected by this Section.

Professor FLETCHER SHAW (in reply) said that the paper was not intended to cover the whole subject of carcinoma of the body of the uterus, but only that of the age incidence, and was really a plea for the definite teaching that carcinoma of the uterine body only occurred after forty years of age, or, alternatively, after the menopause changes had commenced. The further cases quoted by Dr. Gemmell and Professor Phillips substantiated this statement. Altogether, in the paper and discussion, over 300 cases had been tabulated, and of these only three had occurred before the age of 40, and of these three cases one had occurred a year after the artificial menopause, in another there was grave doubt whether the growth had not begun in the cervix, so that there was only one, a case of Dr. Gemmell's, out of 300, which showed definite carcinoma of the body before the age of 40, or before the menopause had begun. This fact emphasized the great rarity of the disease before this age and warranted the dogmatic teaching that the disease only began after the onset of the menopausal change.

The paper probably gave the impression of an attack upon microscopical examination. This was far from being the authors' intention, but in cases of malignant disease the microscopical slides were far from definite, and in these cases the clinical findings were of much greater value than those of the microscope. If, in a case of post-menopausal bleeding, thick

pieces of friable material were removed this was a much greater indication of carcinoma than the microscopical report. He had had such a case only this week. A woman, aged 62 years, had had a little bleeding which he thought was due to a caruncle, but, when cauterizing the caruncle he took the opportunity of curetting the uterus and removed some large pieces of friable material. The microscopical report stated it was not definitely malignant, but, acting on the clinical findings, he had performed a panhysterectomy and had found the uterus filled with a friable malignant growth.

All cases of bleeding after the menopause must be considered as cases of malignant disease of the cervix or body of the uterus, until some other cause was demonstrated, and, if curetting was done in such a case, the removal of thick friable material was a definite sign of malignant disease, no matter what the pathological report might be.

The Prognosis and Treatment of Eclampsia and Albuminuria, with special reference to the Risk of Recurrence in Subsequent Pregnancies.

By JAMES YOUNG, D.S.O., M.D., F.R.C.S.Ed.

ABSTRACT.—(1) In a series of hospital cases treated for eclampsia and albuminuria there was found to be a recurrence of toxæmia or non-toxæmic interruption of pregnancy (abortion, acccidental hæmorrhage, etc.) in succeeding gestations in over 50 per cent.

(2) There is no evidence that this tendency to recurrence is directly or indirectly dependent upon any persisting renal defect. Chronic nephritis is found in only a small proportion of cases and there is evidence that this is the *result* of the toxæmia.

(3) In view of the large risk of recurrence the need for antenatal supervision from an early stage in the event of a subsequent pregnancy should be clearly explained to the patient.

(4) Toxæmia in two or more pregnancies is an indication for the prevention of any further pregnancy.

IT is in some ways remarkable that, despite the close study which the clinical features of the toxæmias of late pregnancy have received, we should now be discovering, not only that there are still some fields left for us to explore, but that some of our teaching on the basic clinical facts has proved to be incomplete and in some ways actually unsound. Take, for example, eclampsia. It has been almost a commonplace in our teaching that a woman who has suffered from this condition is little likely to suffer in any similar way in her next pregnancies. A study of such cases proves this to be incorrect. At the British Congress of Obstetrics and Gynæcology in 1927 I showed that in forty-seven consecutive cases of eclampsia in parous women admitted to the Edinburgh Royal Maternity Hospital eclampsia had occurred six times in a previous pregnancy and albuminuria nine times. This gave a total recurrence figure of 31·9 per cent. Adding to this six cases in which there had been abortion, accidental hæmorrhage, premature or stillbirth without toxæmia in the previous gestations, we obtained a total recurrence of abnormality of 44·7 per cent.

Dr. Jessie C. B. Sym has since carried out a similar investigation of the subsequent obstetric history of the surviving cases of eclampsia treated in the hospital from 1919 to 1926, and in a total of sixty-seven cases which were traced she found that forty-two patients had become pregnant again, with a total of sixty completed gestations. In three of these eclampsia had developed for the second time, with a fatal result in one case, whilst in fifteen of the other pregnancies "albuminuria" had occurred, giving a total of eighteen out of sixty subsequent pregnancies which were toxæmic, i.e., 30 per cent. Adding to these six more, in which abortion, etc., took place, we obtain twenty-four abnormal pregnancies out of a total of sixty, i.e., 40 per cent., a figure which emphasizes the risk which is run by an eclamptic patient should she become pregnant again.

A similarly sinister record is revealed when the "albuminuria" cases are studied. In the paper to which I have already referred I recorded a recurrence of toxæmia,

and of abortion, etc., without toxæmia, in forty-two out of a total of 118 studied, i.e., 35·6 per cent. These figures referred to the previous pregnancies in parous women admitted to the hospital with "albuminuria." By following the surviving cases into their subsequent obstetric life we have found that fifty-one out of eighty-one patients subsequently became pregnant again and had, between them, eighty-four completed pregnancies. Of these, forty-four had "albuminuria" (this diagnosis was based on clinical evidence supplied by the patient in five), that is, 52 per cent. In fourteen of the other pregnancies there occurred abortion, accidental hæmorrhage, etc., without toxæmia, making a total abnormality recurrence of fifty-eight in eighty-four (69 per cent.). The considerable increase in the percentage of recurrence in the pregnancies occurring after as compared with those occurring prior to the first toxic gestation treated in hospital is to be accounted for by the fact that in many instances the follow-up proved this attack to be the beginning of the recurring sequence.

We thus see that eighty-two out of a total of 144 subsequent pregnancies in cases of toxæmia (eclampsia and "albuminuria") were vitiated by toxæmia or intra-uterine disaster to the fœtus without toxæmia, that is a percentage of 57. Furthermore, three out of the ninety-three women, who became pregnant subsequent to their leaving hospital, died from toxæmia in a subsequent pregnancy, that is, more than 3 per cent. It is probable that this death-rate would have been actually higher could all the cases have been followed up, for it is known that "deaths" are notoriously difficult to trace.

Findings similar to those here recorded have been reported by other observers, e.g., Greenhill [1], Gibberd [2]. Greenhill studied the subsequent completed pregnancies of sixteen women who had had eclampsia, and found that in nine these were normal, whilst in seven they were toxæmic, that is, in 43 per cent. Gibberd found that in twenty-eight cases of albuminuria there was a recurrence of the toxæmia in nineteen, that is, 68 per cent.

This large incidence of recurrence in all classes of toxæmia carries with it implications of far-reaching importance. In my opinion this fact of recurrence, applying, as it does, to all types of the disease, eclampsia, pre-eclampsia, etc., constitutes the most significant addition to our knowledge of the subject made within recent years, and must be regarded as an important new clue in our efforts to trace the underlying causes.

The first lesson it teaches is the inadequacy of our present classification, for this has been based on the view that eclampsia and "pre-eclampsia" are essentially non-recurrent. We have thrown into a category by itself the type in which recurrence is the characteristic feature. We have classed it as "recurrent" or, in view of the observable kidney lesion sometimes accompanying it, as "nephritic." The new discoveries render this classification unsound, for in respect of the criterion of recurrence, there is nothing to distinguish one kind of late pregnancy toxæmia from another. Furthermore, the view which attributes recurrence to a defect in renal function must, if well-founded, apply equally over the whole range of these toxæmias.

I have elsewhere [3] discussed the discrepancies in fact and in theory which seem to lie at the back of any such view, and I cannot enter into the matter fully on this occasion. The attitude we adopt towards this question has implications not merely academic in their nature; bound up with them there are important issues of an essentially practical nature. We may, in the first place, in accordance with the usual teaching, attribute the tendency to recurrence to a chronic renal defect, which leads to a breakdown of function in succeeding pregnancies. In support of this view we may cite the nephritis which punctuates these recurring toxæmic gestations and, in addition, the evidence of nephritis which is sometimes found in the intervals between the pregnancies. On the other hand, we may regard the

nephritis as caused by the toxæmia, which in such women tends to recur during their pregnancies and of which the accumulated result in some instances may be persisting and chronic nephritis. These two standpoints are distinct and mutually exclusive. According to one, the recurring toxæmia springs from the kidney defect; according to the other, the kidney defect springs from the recurring toxæmia.

For many years I have been unable to satisfy myself that in endowing the kidney with ætiological properties of a primary character we were not theorizing beyond our facts. As I now see it I am satisfied that, whilst we are still far from a complete understanding of all the phenomena, the data, when fairly marshalled and analysed, drive us to regard the kidney as an organ which in the sequence of events plays an entirely secondary part, and which in its liability to suffer damage by the specific toxins elaborated during the development of a vitiated pregnancy is in no way different from such organs as the liver and the heart.

The arguments in favour of this view may be summarized as follows:—

(1) A recurrent attack of eclampsia or albuminuria is in no essential fact different from the initial attack in the first pregnancy involved. Now, there is no evidence that this initial attack is in any way, directly or indirectly, dependent upon a renal defect. There is, on the other hand, ample evidence that the kidney damage invariably occurring in such a case, is directly caused by the toxins elaborated in some specific manner in some other region of the pregnant organism. Why in a recurrent attack should the relation of the kidney in the sequence be different?

(2) Just as the initial eclampsia or albuminuria occurs typically in a woman whose kidneys are previously healthy, so typically in the intervals between the pregnancies which are recurrently toxic, the kidneys reveal no evidence of any deficiency. Furthermore, subsequent to a series of toxæmic pregnancies, in each of which the kidneys have suffered, the patient may enjoy good health with no cardio-vascular or renal disease, so long as she does not again become pregnant.

(3) The chronic renal lesion found occasionally in such women is easily explained as due to the effects of one bad attack or to the accumulated results of the damage sustained during a recurring series of toxic pregnancies. Dr. Jessie Sym has carried out an examination of the cardio-vascular system, of the blood-urea and the urea concentration test in sixty-six post-eclamptic and seventy-three post-albuminuric cases treated in the Edinburgh Royal Maternity Hospital. This revealed evidence justifying a diagnosis of chronic renal change in two of the post-eclamptic cases (3 per cent.), and in six of the post-albuminuric cases (8 per cent.). The significant fact is that in seven out of these eight cases the patients had two or more toxic pregnancies. Out of those cases, comprising approximately half of the total number, in which a single toxic pregnancy had occurred, in only one was chronic renal change found, and in this case the toxæmia lasted for eighteen weeks. These facts show clearly that the degree of involvement of the kidney is directly related to the interval over which it is exposed to the toxins of an abnormal pregnancy.

(4) Side by side with the tendency to recurrent toxæmia there is a well-recognized tendency to the succeeding pregnancies of such women being terminated prematurely by abortion, stillbirth, or accidental hæmorrhage. This tendency has usually been attributed to a chronic renal deficiency. In my previous papers I have called attention to the fact that these abortions, etc., may be unassociated with any toxic manifestations or any recognizable renal defect, although they occur in women known to have this tendency to recurrent toxæmia in their other pregnancies.

For lack of a better explanation, in the past we have, in similar contingencies, had recourse to the "nephritic" conception. In this view these damaged pregnancies would be attributed to a deficiency in renal function which is so meagre that it fails to reveal itself in the ordinary ways. How, it may be asked, could such a minimal defect, which is insufficient to cause any clinical evidence of its presence, precipitate the intra-uterine disasters which these crises necessarily imply?

It was considerations of this order that led me some years ago to approach these problems from a new angle. A critical analysis of the available data early proved that, whilst the fact of toxæmia was the dominating clinical phenomenon and as such had naturally obtruded itself into the forefront of the problem, it was by no means invariable and essential. It was rather of the nature of an accident or incident which might occur in some circumstances but not in others. Thus, for example, it might be present in one pregnancy ending in accidental hæmorrhage, whilst in the same woman it might be absent in the next pregnancy which ended similarly. Whilst toxæmia was purely incidental there was, on the other hand, one factor and one factor alone which seemed to run throughout the process as an invariable accompaniment of the abnormal pregnancies, toxic and non-toxic alike. This was disease of the placenta. In all the affected pregnancies, whether they ended by abortion, accidental hæmorrhage or stillbirth, and whether they ended in toxæmia or not, one could always find placental damage. The existence of such a pronounced lesion which, according to this reading, must be considered as arising independently of the toxic process, naturally pointed to a possible source of the toxic elements which all agree to be responsible for the toxæmia of pregnancy. As I have shown, it is possible to account for all the known facts by predicating in such women the existence of some factor or factors which during pregnancy may precipitate a toxæmia by involving the life of the placenta. These factors, whatever they be, are liable to persist in the intervals between the pregnancies, during which they may cause no apparent trouble, but they are apt to reassert themselves in the same way during the next pregnancy. Whether, after the placental damage has occurred, toxæmia will follow will depend on two variables, (1) the amount of placental tissue involved, and (2) the time during which the damaged placenta is retained in the uterus.

Observations on Treatment and Prevention.

From this study of the subsequent fate of toxæmic cases we may draw lessons regarding (*a*) the management of, and the indications for terminating an existing pregnancy, and (*b*) the question of future pregnancies.

Management of Existing Pregnancy.—In every toxæmic case the immediate outlook is uncertain and, even in the mildest case, it is impossible completely to exclude the risk that at any moment the clinical picture may change rapidly for the worse. For these reasons, were we to consult the interests of the mother alone, we would be justified in terminating the pregnancy in almost every instance so soon as the first toxic manifestations appear. In addition to these immediate dangers there is the more remote risk of the patient suffering lasting damage to her cardio-vascular system and kidneys. In this series of hospital cases, which, of course, comprise those suffering most severely, this risk assumes the proportion of 8 per cent. of the total. This risk, as we have seen, is minimal in the woman with her first attack, the major danger being reserved for those with repeated toxic pregnancies.

Against these somewhat gloomy admissions there must be urged the experience of all that many toxæmic pregnancies respond with rapidity to antenatal care and hygiene, with special reference to the diet, the bowels and rest. It is in this class of work that modern antenatal treatment has claimed some of its greatest triumphs. At the same time there is, it seems to me, a risk that we may over-estimate our powers in this connexion, for we must acknowledge that in this disease, which affects 3·5 of all pregnant women (Eden and Holland), the majority of the cases are destined from the beginning to pursue a simple course, just as we know that the greater proportion of the severe cases resist all our efforts to cure them.

The only sanction we have for an expectant line of treatment is that we may thereby carry the pregnancy over into a period that will ensure the viability of the child. In this matter, however, our decisions are still further complicated by the fact that the disease which so heavily prejudices the life and health of the mother is

even more unkind to the fœtus. Of all the maternal diseases, toxæmia is the one which is most lethal to the child. Thus, Holland in a series of 301 fœtal deaths showed that seventy-seven, i.e., 26 per cent., were caused by albuminuria, eclampsia, or accidental hæmorrhage. In the present record of albuminuric cases, forty-nine out of 168 children, i.e. 29 per cent., died *in utero*. Many of those born alive survive only a short time.

These considerations show that whenever we elect to treat a toxæmic patient expectantly we may be gambling with the life or health of the mother in the interests of a child whose survival is problematical. I am convinced that in the past we have often been over-zealous in our regard for the child and too little attentive to the interest of the mother.

Where and whilst the manifestations are slight and the patient is under adequate supervision, one may often safely temporize. In such a case, however, the termination of pregnancy as soon as the eighth month is well entered upon, may be in the best interests of the infant.

If there is marked œdema of the limbs and face, a blood-pressure raised 30 or 40 mm., or more, a heavy deposit of albumin in the urine, a distinct fall in the urinary secretion, and, more especially, if there are headache and visual symptoms, delay is dangerous. In less severe cases a delay sufficiently long to test the response to treatment may be considered justifiable, but any change for the worse, or a refusal of the symptoms to abate under rest in bed, watery diet and active eliminative treatment, indicates the need for interference.

In all toxæmic cases, except those that are mild, which are seen before the seventh month, I am convinced that delay is rarely if ever justified, for this necessarily implies a dangerously long interval of waiting before viability of the child is reached. I would like to see it laid down as an axiom that, except in very slight cases, no toxic patient should ever be kept under treatment for more than two weeks and then only if the progress is good. A disregard of this precept may damage a woman's health irreparably and, as we have seen, the prospect of a living child at the end is by no means certain. These risks, as we have seen, are especially to be guarded against in women who have suffered damage in their previous pregnancies.

For ordinary routine purposes, where the urgency is not immediate, the best means for the induction of premature labour is probably the gum-elastic bougie, or some similar method. Pituitary extract is of little avail for this purpose until the last days of pregnancy, although I find the hypodermic administration of five-minim doses of this drug of value in reinforcing the action of the bougies once uterine efforts commence. I then use 5 minims every half hour till a total of 2 c.c. are administered. Where the blood-pressure is high, pitocin, which contains the ecbolic principle of the posterior lobe without the pressor agent, may be employed.

For cases of grave urgency the bougie method for inducing labour is too slow and too uncertain and for such cases I employ Cæsarean section under spinal anæsthesia. With this operation we obtain immediate evacuation of the uterus in a case where every hour's delay may jeopardize the prospects. By means of spinal anæsthesia we reduce the shock and eliminate the risk of further damage to the organs associated with a general anæsthetic. Amongst the risks one of the greatest is irritation of the bronchioles passing on to bronchitis and broncho-pneumonia, from which toxæmic cases are specially liable to suffer.

For these reasons I have come to regard spinal anæsthesia as the ideal method in such cases. The immediate fall in blood-pressure attending its use in some ways may be regarded as an added advantage in cases in which an elevated pressure often constitutes a dangerous feature. At the same time I should like to take this opportunity of commending a wider employment of spinal anæsthesia as the routine procedure in Cæsarean section. One of the most striking effects of the removal of the control of the spinal centres resulting from its use is a spontaneous and

immediate contraction of the muscle of the uterine wound producing a sort of natural hæmostasis, which in many cases is so marked that the operation can be carried out literally without the loss of more than a few drachms of blood. In many instances, for example, in collapse from the exhaustion of a protracted labour or from blood-loss as in placenta prævia, this fact may lead to a considerable reduction in the risk associated with the management of the case. A series of about 40 cases of Cæsarean section so carried out by myself and my assistant with no mortality and in which are included a considerable number of grave cases of toxæmia, of placenta prævia, and of obstructed labour with potential sepsis, has led me to conclude that spinal anæsthesia has brought into the operation of Cæsarean section a measure of safety which it did not possess before. In cases in which a fall in blood-pressure is to be feared, as, for example, in the collapse from hæmorrhage or in bad cardiac cases, I have, in addition, employed adrenaline and, latterly, vasopressin.

At this point I would make it clear that once the toxæmia passes over into an eclamptic seizure, any active intervention is to be rigidly avoided and the patient must be treated along such lines of expectancy as those laid down by Strogonoff and others.

The Question of Future Pregnancies.—Amongst the lessons to be drawn from the data assembled in an earlier part of this paper there is one which is of profound importance. It is that the risk of a recrudescence of toxæmic symptoms in subsequent pregnancies is so great that all such women should have medical supervision from the early stages should they again become pregnant. In the present state of our knowledge on ultimate causation we are not equipped with any certain method of protecting a woman with this taint against the risk of recurrence, but we can do much to safeguard life and health by emphasizing the imperative need of this antenatal care. The necessity for explaining these facts is all the greater because of the rapid recovery which usually follows on one attack and which often gives to the patient a sense of security which is little justified. As we have seen, the woman who has already suffered from several toxæmic pregnancies should be the object of very special antenatal supervision in the event of a future pregnancy.

The next point we have to consider is the advice to be given in regard to preventatives. So great is the risk run by every woman who is liable to toxæmic gestations that this matter of prevention may be considered as arising in practically all cases. Personally I believe it may be laid down as a rule that no woman should be exposed to further risk if she has had two or more toxic pregnancies, for the risk of recurrence rises rapidly with the number of pregnancies so vitiated. Where, on the other hand, the disease has involved the loss of the child on each occasion, there may be such a desire for a further attempt that the medical attendant may require to be satisfied with urging that this can only be sanctioned if the patient adequately recognizes the need for supervision throughout the pregnancy and is prepared to accept the risk, at the end, of a further disaster.

In anticipation of a future pregnancy, everything should be done to build up the strength of the patient and to remove any factors that may even indirectly have a bearing on this tendency to recurring trouble. It has been suggested by some observers that focal sepsis may play a contributory part in the taint which these women carry over from pregnancy to pregnancy. In a previous paper I have referred to some considerations which are in keeping with this view, and have suggested that it is conceivable that some such factor is at the back of the tendency to placental disease, which, as I have pointed out, is the only essential lesion of the pregnancy in such cases. In a few instances I have found that, following the removal of gross sources of focal sepsis, for example, tonsils and teeth, a previously unbroken sequence of damaged pregnancies has been interrupted and has been succeeded by normal gestations. The data are still too meagre to allow of a definite pronouncement on this matter, and at this stage I merely throw out the suggestion

tentatively. In eliminating this factor of potential sepsis, however, we are adopting a step likely to help the general health and resistance, even should the subsequent history fail to establish it as a factor of primary value.

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Rhabdomyosarcoma of the Ovary.

By J. BARRIS, F.R.C.P., F.R.C.S., and WILFRED SHAW, F.R.C.S.

(From the Maternity Department, St. Bartholomew's Hospital.)

Clinical History.—A. K. (ref. No. 79399), aged 32 years, was admitted to St. Bartholomew's Hospital on June 18, 1928, complaining of "enlargement of the abdomen."

Past History.—Two pregnancies and two labours, the last two years previously. Menstruation began at the age of 14, was regular every twenty-eight days, and continued for four or five days, the loss being scanty. The last regular period was from June 15 to 18, 1928.

She was in her usual health until within three weeks before her admission, when she noticed frequency of micturition and difficulty in beginning the act. She had noticed a rapid enlargement of the abdomen with pelvic pain during the previous fortnight.

Laparotomy was performed June 21, 1928. The tumour was found to be a cyst of the right ovary. The uterus and left appendages appeared to be healthy. There was no ascites. The tumour was not adherent, and had a narrow pedicle. There were no scattered nodules on the peritoneum.

The patient made an uninterrupted recovery and was discharged from hospital on August 9. Previous to her discharge she was treated by X-rays over the abdomen and pelvis, it having been recognized that the tumour was malignant.

Naked-eye Appearances.—When examined after removal, the tumour was found to be roughly ovoid in shape, and to measure 8½ in. by 6½ in. by 3 in. It was dull grey in colour, the peritoneal surface was smooth, and numerous small vessels were seen immediately beneath the surface. The tumour was cystic, a single loculus was present; the contained fluid was thin, dark, and sanguineous. No pseudomucinous material was found in the cavity. Masses of solid, friable material, purple in colour, projected irregularly into the cavity. The wall of the tumour varied in thickness, in some places it was thin, while in the situation of the large projections the wall was 1½ in. thick. The tumour seemed unlike the common forms of ovarian growths, for it was not multilocular, no pseudomucin was present, and the projections into the cavity did not resemble those met with in cases of malignant papillomatous cystomata, as they were purple in colour with irregular surfaces, and the tissue of which they were composed was extremely friable. Again, the wall of the tumour in those areas where it was thin was firm, and, as has since been recognized, had the texture of plain muscle, corresponding closely to that of the large intestine. The naked-eye appearances suggested that the tumour was of a malignant nature, but it was difficult to correlate its appearance with those of the usual forms of malignant ovarian tumours.

Microscopic Examination.—The preliminary histological examination was made from pieces of tissue obtained from the friable material and from the wall where the latter was thin. There was no surface layer of cubical or endothelial cells and the peritoneal surface of the tumour was composed of connective tissue cells, amongst which were scattered elongated cells rich in protoplasm. Beneath this zone dilated capillaries were seen and interstitial hæmorrhage was well marked in this region.

Deep to this layer, bundles of elongated cells were found and still further towards the cavity of the tumour, large cells with eccentric nuclei and irregular cell outline, scattered amongst spindle cells, were seen. Such cells are typical of a rhabdomyosarcoma and it was this clue which indicated the nature of the tumour. Further towards the lumen the tissues of the tumour were necrotic, and degenerate cells and red blood corpuscles were found in this region. There was no evidence of the presence of any epithelial structures and it was clear that the tumour was of connective tissue origin. The elongated cells, rich in protoplasm, were then examined for cross striations and it was found that these striations could be easily demonstrated, particularly in preparations stained with Giemsa's stain. The striated cells were embryonic in type and in no region were well differentiated striped muscle cells found. The presence of such cells suggested that the tumour was of the nature of either a mixed tumour, a teratoma, a rhabdomyoma or a rhabdomyosarcoma. To determine the exact nature of the tumour, further sections were taken, pieces of tissue being removed from ten different areas. In no situation were epithelial elements found nor could cartilage or bone be detected. It was therefore clear that the tumour was neither a teratoma nor a mixed tumour. The next point was to determine whether the growth should be considered a rhabdomyoma or a rhabdomyosarcoma. As no healthy ovarian tissue could be demonstrated and as there was no suggestion of encapsulation of the tumour, it seemed probable that the growth was malignant. This view was borne out by the detailed histological examination of the growth. Apart from the tendency to the collection of the striated muscle cells into bundles, which was well seen near the surface of the tumour, the cells were irregularly arranged among themselves, so that striated muscle cells were found scattered haphazardly amongst spindle cells and round cells. Moreover there was a great difference between the sizes of the striated muscle cells; at the periphery of the tumour they were small, towards the middle very large, and lastly the cells differed markedly in shape. The walls of the blood-vessels varied considerably; near the surface of the tumour the endothelial lining was well developed but towards the middle it was recognized only with difficulty and in some places the tumour cells surrounded blood corpuscles without an intervening endothelial layer.

From these considerations it seemed clear that the tumour was malignant in type and that it should be considered under the term rhabdomyosarcoma.

Rhabdomyosarcoma of the ovary is a very rare tumour and it is because of its rarity that this case has been reported. From an examination of the literature it seems that only three previous cases have been described; one by Virchow [1] in 1850, the second, from a girl of 17, by Vignard [2] in 1889, and a third by Himwich [3] in 1920. This last case was in a child aged $1\frac{1}{2}$ years: the tumour weighed 26 oz., arose from the left ovary, and rapidly recurred after removal; the child died three months after ovariotomy.

Rhabdomyosarcoma of the ovary is very difficult to explain, for striated muscle cells are foreign to the ovary, although they have been described in dermoid cysts. If attempts are made to account for their presence in the tumour above described, difficulties at once arise. If it is supposed that the tumour is teratomatous in origin and arises from a totipotent cell it is difficult to understand why only myogenic cells are represented and why epithelial cells are absent. Again, although the tumour in this case arose from the ovary where totipotent cells are known to exist, the incidence of such tumours in the ovary is small and similar tumours occur more commonly in organs where totipotent cells are not present.

The alternative hypothesis of cell displacement is again not convincing. It is true that in the case now described the features can be explained by supposing that during the development of the ovary and in its migration into the pelvis, myotomes were included and that after many years they commenced to proliferate to produce

the tumour now described. It is, however, difficult to understand why such cells should remain dormant for so long and still retain extraordinary powers of cell proliferation. Lastly, there is no direct evidence to show that such primitive cells are ever found in the normal ovary.

It would seem, therefore, that in the light of our present knowledge no satisfactory explanation of the origin of these tumours can be given.

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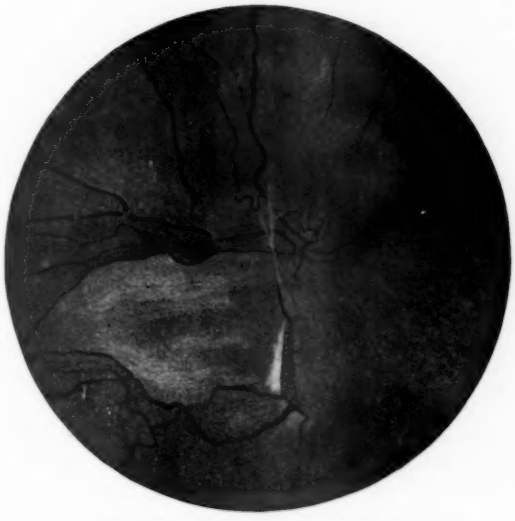
Section of Ophthalmology.

[*November* 9, 1928.]

CASES.

Mr. J. D. MAGOR CARDELL, F.R.C.S., showed a case of congenital cataract in which shrinkage of the lens had occurred.

Persistent Connective Tissue in Fundi.—Shown by Miss A. L. ADAM (for M. S. MAYOU, F.R.C.S.).—At a school clinic the right eye in this case was found to be defective, and the child was brought to hospital. He is an adopted child, therefore not much history can be obtained. So far as is known, there was no birth injury. The vision of the right eye is down to the perception of hand movements; in the left it is $\frac{6}{6}$. In the fundus of the right eye there is an extensive delicate membrane, obscuring the disc and partially obscuring the vessels, and accompanied by pigmentary disturbance.



Persistent connective tissue in fundi.

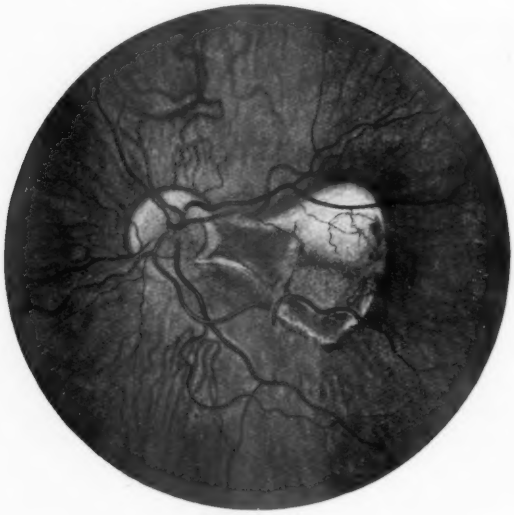
In the left eye there is a similar, much smaller, membrane, but on the nasal side of the disc. This favours the opinion that the condition is a congenital one.

Discussion.—Miss IDA MANN said this was an interesting case, and if it were not for the condition of the left eye, she would question its developmental nature, as the condition in the right eye did not resemble any normal stage of development. The membrane was far too extensive for persistence of the normal glial tissue on the disc. The appearance in the left was, however, so typically that of a persistent hyaloid artery—one could see the translucent sheath coming forward into the vitreous—that that seemed beyond doubt. Therefore she thought one was justified in saying the condition in the other eye was congenital also, though it must be regarded as an aberrance of development, and not a persistence of a normal embryonic stage.

Mr. E. TREACHER COLLINS said he regarded the condition as either an abnormal congenital formation of fibrous tissue around the central hyaloid artery, or as due to a hæmorrhage

into the sheath of the hyaloid artery at birth, which had subsequently formed a basis for the development of fibrous tissue. It was always difficult, in these cases of fibrous tissue formation in the vitreous chamber, to say definitely whether it was due to a birth injury with hæmorrhage into the sheath, or to an aberrant congenital malformation.

Retinal Cyst.—O. GAYER MORGAN, F.R.C.S.—G. Y., a boy, aged 10. Defective vision of left eye noticed at school examination four years ago. Mother does not think the eye was divergent before that time. In the macular region there is a swelling occupying a considerable area and raised about 3D. It has a greenish



Cyst of retina.

tinge, which suggests a hæmorrhagic origin. It is probably a type of Coats' disease with cyst formation, similar to the case shown by Mr. Hine earlier in 1928. The appearance has not altered in the course of the last year.

Mr. M. S. MAYOU said that at the last meeting of the Ophthalmological Society he had shown pathological specimens illustrating this condition. He considered that the first changes took place in the choroid, whilst in the retina the first sign was that there was a separation between the layers owing to the outer part of the retina becoming adherent to the retinal pigment layer and choroid. The separation between the layers of the retina began as a small cyst, as in the present case, and also in that shown by Mr. Hine. The separation might proceed to complete detachment of the inner half of the retina, leaving the outer half adherent to the choroid. Frequently these cases ended in glaucoma, which was probably due to thrombosis of the diseased venæ vorticosa.

Blood-Staining of the Cornea.—O. GAYER MORGAN, F.R.C.S.—J. R., a boy, aged 10. On April 8, 1928, a pistol went off at point-blank range and injured the left eye. At that time there was only a small wound of the sclera, and a small hyphæma, and except for slight œdema of the nasal retina, the rest of the eye was uninjured. After an extremely restless night a complete hyphæma developed, with increased tension. X-ray examination showed no foreign body in the eye or orbit, so that the injury must have been a glancing one. A paracentesis three weeks after the injury, showed that the aqueous was clear of blood, though the cornea was deeply

stained all over and quite opaque. The blood pigment had apparently passed through Descemet's membrane in its whole extent.

Discussion.—Mr. CYRIL WALKER (President) asked whether any Members had known this appearance to follow surgical interference in the eye when there had been extensive hæmorrhage into the anterior chamber, the hæmorrhage having been absorbed very slowly: Or was this a condition which only followed trauma?

Mr. E. TREACHER COLLINS said he had seen several instances of blood-stained pigmentation of the cornea, and all had occurred after concussion injuries, usually in cases having increased ocular tension. He possessed sections of several eyes with such staining, and had found it due to granules of hæmatoidin and hæmosiderine. Blood was effused into the anterior chamber, where it became stagnant, not being able to escape on account of blocking of the angle of the chamber. Then the hæmoglobin, dissolved out of the red corpuscles and diffused through Descemet's membrane into the cornea, where the crystals of hæmatoidin were precipitated in the substantia propria. This present case showed a clear margin of cornea at the periphery of the stained area, and when first seen the condition looked like a lens dislocated into the anterior chamber, for which condition he had known such cases mistaken. He had watched a case of the kind over a long period, and found that it took over two years for the whole cornea to become clear. The process of clearance was so slow because each crystal had to be taken up by leucocytes which wandered in from the periphery of the cornea.

Pigmented Tumour of Eyelid.—MAURICE H. WHITING, O.B.E., F.R.C.S.—N. H., a girl, aged 12. This patient was seen about ten weeks ago, when nothing abnormal was noted in the eyelids. The tumour is said to have started as a small nodule eight weeks ago and to have grown rapidly since. A rounded oval tumour, about $\frac{3}{4}$ in. long and $\frac{1}{4}$ in. wide, situated beneath the skin and adherent to it. The tumour is pigmented and there are also vessels in the overlying skin.

POSTSCRIPT.—Subsequently to the meeting the tumour was removed with a good portion of skin and the surrounding soft tissues.

It was possible to suture the lid, with very little resultant ectropion, and that only at the outer angle.

Macroscopically the tumour appeared to be cystic, and to contain a centre of white granular material.

Microscopic Appearance.—The tumour was cystic, the wall being formed of stratified squamous epithelium, varying in thickness from two to four or five cells. The contents comprised a quantity of granular débris showing no actual structure, and a good deal of blood. The latter, no doubt, accounted for the increase in size of the cyst and for its dark colour.

The cyst was probably a dermoid, though, in the absence of any epithelial structures in the contents, this opinion cannot be definite.

High Hypermetropia.—F. A. WILLIAMSON-NOBLE, F.R.C.S.—G. B., female, aged 28. This is the highest degree of hypermetropia I have ever seen. The patient has been wearing plus 20 glasses—not prescribed by myself—but she sees better with plus 18. With that, the right vision is $\frac{6}{60}$, the left, $\frac{6}{24}$. She was sent into the Queen Square Hospital as possibly having a cerebellar tumour, since there were cerebellar symptoms, and it was a question whether or not there was papillœdema. We made the provisional diagnosis of pseudo-papillœdema.

Osteitis Deformans with Central Senile Exudative Retinitis. Iritis.—M. S. MAYOU, F.R.C.S.—F. M., male, aged 61. Has suffered from osteitis deformans for many years. There are well-marked bone changes in the skull and long bones and calcareous degeneration of all the arteries (see skiagrams, figs. 1 and 2, p. 12). Vision: right eye, hand movements; left, $\frac{6}{6}$. First attack of iritis one year ago, second attack three weeks ago "rheumatic type." There is definite arterio-

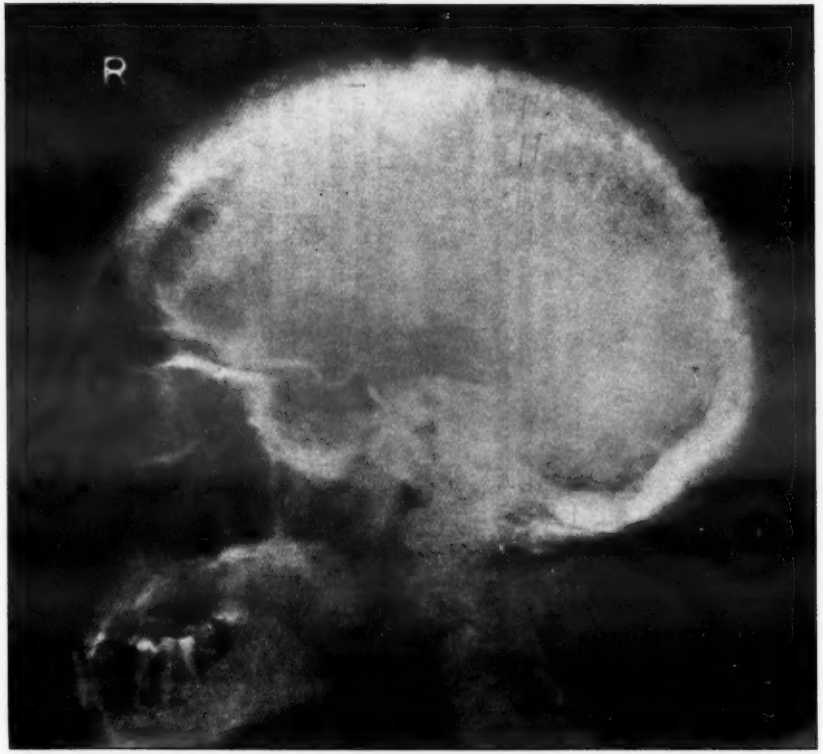


FIG. 1.—Osteitis deformans showing thickening of the skull, etc. (Photograph by Mr. Ulysses Williams.)

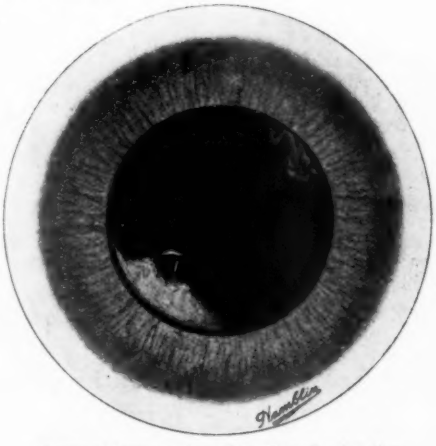
FIG. 2.—Osteitis deformans: showing calcification in the femoral artery (Photograph by Mr. Ulysses Williams.)

sclerosis and tortuosity of the veins of the retinæ, but no calcareous changes are to be seen in them. In the region of the macula in the left eye there is a mass of exudate similar to that seen in the senile form of Coats' disease, and there are several hæmorrhages in its neighbourhood.

I have been unable to find any literature relating to iritis and retinal changes associated with osteitis deformans, but such changes must be frequently present in a disease which is associated with so much vascular degeneration.

Injury of Lens causing Alteration of Refraction.—A. H. LEVY, F.R.C.S.—This is the case of a man aged 31 who whilst ski-ing in Switzerland in January, 1928, stumbled and pitched forward so that his right glass came in contact with the top of his ski-ing stick. The glass broke and some of the fragments were driven into the eye, cutting the cornea, just nicking the pupillary edge of the iris, and cutting the anterior capsule of the lens.

He remained in Switzerland for a week under the local doctor. When I saw him ten days after the accident, the eye was perfectly quiet and quite white. The anterior chamber was of normal depth, but contained a fair amount of lens matter. The cut in the lens capsule could be seen easily with lens substance protruding from it. The tension was normal and there was no discomfort. The vision was a bare $\frac{6}{60}$ but with a pinhole he got $\frac{6}{12}$.



Injury of lens causing alteration of refraction.

Under the circumstances, there seemed to be no reason for immediate interference and so it was left. I fully expected the lens either to absorb or at least to become completely opaque, but neither of these things happened. The masses of lens matter in the anterior chamber disappeared in about four weeks. The wound in the lens capsule closed up, leaving that part of the lens at the point of the injury opaque, but the rest of the lens remained perfectly clear so that a month after the accident the vision with a pinhole was $\frac{6}{9}$ partly.

Two weeks later, i.e., six weeks after the accident, vision was + 2·25 sph. = $\frac{6}{12}$, and eight weeks after the accident, with + 1·75 sph. = $\frac{6}{9}$. Three months after the accident with + 1·50 = $\frac{6}{6}$ partly.

All this time the eye was kept under atropine, but by the end of March it was obviously doing perfectly well, so the atropine was discontinued and it became possible to measure the accommodation power. This was 6 D in the left uninjured eye and 2 D in the right eye.

Six months after the accident the vision in the right with + 1·00 sph. + 0·25 cyl. Axis 80° = $\frac{6}{6}$ partly, the amplitude of accommodation had risen to 4 D, that of the left remaining at 6 D.

Now, before the accident, this man was myopic, 3·75 D in the right eye and after the accident is hypermetropic, 1·0 D, so that the effect has been to change a myope of 3·75 D into a hypermetrope of 1 D.

The left eye also is myopic, 3·75 D, and has $\frac{6}{6}$ vision. In actual fact he wears this combination quite comfortably for distance, but the difference in accommodative power worried him when reading, though he gradually became accustomed to the defect and now has very little inconvenience.

Similar cases have been reported before. In 1867 Rydel, and in 1877 Kruckow, reported cases of injured lenses clearing up, with alteration in the refraction, but without giving any definite figures.

Lawford (1887), and Treacher Collins (1896), reviewed a number of these cases, but it seemed to be worth while to place this case on record.

The case has perhaps a moral, and that is that in injuries to the lens it may be worth while to wait and see what Nature will do before proceeding to operative measures.

Discussion.—Miss IDA C. MANN asked whether healing of wounds in the lens capsule was the usual course in lower animals. She had once attempted a series of experiments on rabbits in which as a preliminary it was necessary to produce cataract. She needled the lenses, but, to her annoyance, every time she did it, the wound in the capsule healed up, and the only result was a slight opacity of the capsule. She did it five times, on three rabbits, and in each case the wound healed and nothing happened. She also remembered seeing two children shown by Mr. Whiting at a meeting of the Section. They were a brother and sister with dislocated lenses in whose cases Mr. Whiting had attempted needling, but the only result had been a small opacity of the capsule.

Mr. LEVY (in reply) said that the difficulty of producing experimental cataracts in rabbits had been noted by other investigators. It was not an uncommon experience in the needling of lamellar cataracts in young people to find that the laceration in the capsule closed up and the needling had to be repeated, but this was much rarer in the adult and this man was 31 years of age. It was now ten months after the accident and the chances were that the eye would remain as it is.

Some Ocular Manifestations of Focal Sepsis.

By A. F. MACCALLAN, C.B.E., F.R.C.S.

THIS paper continues the account of the clinical observations, which I reported last April at the Annual Congress of the Ophthalmological Society, on the association of certain pathological states of the eye with septic conditions of the body. These observations were carried out on a consecutive series of 100 patients seen in private consultation. The series began on August 27, 1927, and included all private patients [1] in whom the visual acuity, after correction of ametropia, was $\frac{6}{6}$ or $\frac{6}{5}$ in each eye.

In order to establish the clinical findings I placed in the hands of the members of the congress before reading my paper, printed details of each case referred to. I desired thereby to facilitate criticism. These clinical findings, however, were not challenged; they were as follows:—

1 The reason that the clinical material utilized in this paper is taken entirely from private patients is that among hospital patients gross oral sepsis is generally recognized to be the rule rather than the exception. All the patients here referred to had full vision with glasses, and the majority of them belonged to the upper middle class, and were quite able to obtain general or special medical attention. It was in such patients that it seemed best to study early changes in lens and vitreous.

(1) *Ocular Changes found Associated with Septic conditions of the Body in a Consecutive Series of* 100 *Private Patients who attained full Normal Vision* ($\frac{6}{6}$ *or* $\frac{6}{5}$) *after Correction of Ametropia.*

	Per cent. of patients
Lenticular opacities seen by ordinary focal illumination ...	20
Macular hyperæmia	13
Early pigmentary changes at macula	10
Vitreous opacity as seen with slit-lamp	74

The age of 52 per cent. of the patients of the series was under 40 years, the details being as follows :—

Years	No. of patient
1 to 20	11
21 to 30	18
31 to 40	23
41 to 50	29
51 to 60	14
Over 60	5
	100

My curiosity having already been aroused, I made an effort to determine in the series under discussion, whether any pathological state of any other part of the body was commonly associated with such ocular conditions. Some of my efforts were fruitless, but many of them were successful, as will be seen below :—

(2) *Septic Conditions of the Body found Associated with Certain Ocular Changes in a Consecutive Series of* 100 *Private Patients who attained full Normal Vision after Correction of Ametropia.*

	Patients
Chronic inflammation of appendix requiring immediate operation	1
Enlarged septic tonsils requiring operation	2
Suppurating antrum	1
Dental conditions :—	
Apical abscesses or granulomata	22
Large dental cyst	1
Buried fragments of previously extracted teeth	5
Unerupted teeth mostly impacted	8

Many of these conditions were serious and required immediate surgical intervention : others postulated latent dangers. In any case it was of importance to the patient to be made aware of them.

It has been stated that the patients included in the consecutive series were seen in ordinary private practice, and that 95 per cent. of them consulted me for the prescription of spectacles. No verbal investigation of the patient's history was attempted unless ocular changes were discovered after dilatation of the pupil. If such changes were found, a few general questions were asked. A superficial examination of the throat revealed whether there was need for consultation with a throat surgeon. In the two cases instanced, of enlarged tonsils, the throat surgeon operated on the tonsils and found them septic. A similarly superficial examination of the teeth revealed obvious sepsis, or the presence of crowned teeth, which experience has shown to require further investigation.

In cases in which the condition of the teeth is under discussion I obtain dental skiagrams in duplicate by a professional radiographer; one I keep and the other I send to the patient's dental surgeon, for his opinion, when making a further clinical examination of the teeth.

Thirty-three per cent. of the cases in the series were radiographed, and in most of them dental lesions were found.

The search for a septic focus may be difficult. In one of my cases, in which marked peripheral striæ were detected in the lenses, as well as a trabecular condition of vitreous opacity, skiagrams of the patient's teeth and sinuses were perfectly clear and revealed no striking abnormality. After this the patient told me that from time to time, when she held her head on one side pus dripped out of one nostril. She had a chronic inflammation of the mucous membrane lining the left antrum, the result of which was a chronic toxæmia, with occasional attacks of conjunctivitis, due to direct infection by way of the lacrymal passage.

Varieties of Dental Lesion most commonly associated with Ocular Changes.

Certain forms of dental disease are especially frequently associated with ocular lesions; these are apical abscesses, chronic periodontitis with pyorrhœa, residual sepsis in the bone, buried fragments of teeth which have broken during extraction, the presence of unerupted, impacted teeth, the X-ray appearances of which suggest the presence of sepsis and septic dental cysts.

Apical Abscesses.—"The term is a bad one, because no pus is present, and, most important of all, *they give rise to no pain.* The term 'apical granuloma' has been used; this also is unsatisfactory, because the microscopical appearances are not those of a granuloma. When a tooth with an apical dental lesion is extracted, a mass of solid granulation tissue is found adherent to the apex. This contains pathogenic streptococci and necrotic substance, with very few leucocytes. The term peri-apical bone necrosis would accurately describe the condition actually present" (Sir William Willcox).

These apical abscesses are the most serious lesions found in connexion with dental sepsis and its ocular complications. From them there is a constant flow into the blood-stream of either virulent streptococci or their toxins, and the anatomical position of the lesions prevents an adequate supply of leucocytes and bactericidal body fluids to the part.

"When it is considered that with each bite of the jaw and the normal bite has a pressure of seventy pounds to the square inch, bacteria and their products around the roots of septic teeth are pumped into the circulation, the fact that we have so few rather than so many diseases is the marvel." (Bowdler Henry in *Dental Cosmos*, 1925.)

Chronic periodontitis, causing pyorrhœa, is always accompanied by gross changes in the vitreous.

Residual sepsis, whether in an edentulous jaw, or in a jaw which bears teeth, may be in rare cases an important cause of ocular inflammation. I have found intractable corneal ulceration due to this cause.

Buried fragments after extraction are a fertile source of ocular and general trouble. They may be left behind even by the most experienced operators. The only certain way of ascertaining whether or not the teeth have been removed entire is by having skiagrams taken after the extractions have been performed.

Unerupted, impacted teeth may be entirely free from sepsis. When, however, skiagrams indicate the presence of sepsis there is no doubt that they should be removed. This may mean a serious operation under a general anæsthetic.

Repetition of Investigation with a Second Series of Cases.

The statistics of the results of the ophthalmic examination and of the further investigations surprised me considerably. In order to check my results I have taken a second series of fifty consecutive private patients who attained $\frac{6}{6}$ or $\frac{6}{5}$ vision after correction of ametropia. During the period under survey, twenty-two other patients were examined in whom the visual acuity was lower than the normal ($\frac{6}{6}$ in each eye), owing to various reasons; these are excluded from the new series.

(3) *Ocular Changes found associated with Septic Conditions of the Body in a Second Consecutive Series of 50 Private Patients who attained full Normal Vision* ($\frac{6}{6}$ *or* $\frac{6}{5}$) *after Correction of Ametropia.*

N.B.—Percentages are given and the statistics of the previous series of 100 similar cases are added.

	Fresh series		Previous series
	No.	Per cent.	Per cent.
Lenticular opacities seen by ordinary focal illumination ...	13	26	20
Slit-lamp only	6	12	—
Vitreous opacities seen with slit-lamp including gross opacities	38	76	74
Macular hyperæmia	7	14	13
,, early pigmentary changes	5	10	10

The age of 46 per cent. of the patients in the second series was under 40 years, the details being as follows :—

Years	No. of patients	Percentage
1 to 20	8	16
21 to 30	8	16
31 to 40	7	14
41 to 50	12	24
51 to 60	9	18
Over 60	6	12

(4) *Septic Conditions of the Body found associated with certain Ocular Changes in a Second Consecutive Series of 50 Private Patients who attained full Normal Vision after Correction of Ametropia.*

	Patients	Percentage
Tonsillar sepsis : operation	5	10
Chronic gonorrhœa	1	2
Dental conditions :—		
Apical abscesses	5	10
Pyorrhœa (chronic periodontitis)	1	2
Buried roots	2	4
Obvious septic stumps	5	10
History of previous sepsis (including four edentulous patients)	18	36
Bone infection	1	2

Dental radiography was used in twelve cases to elucidate the conditions. By this means the apical abscesses were detected, and were afterwards proved to have existed by extraction of the affected teeth.

The number of patients who admitted that they had previously suffered from serious dental sepsis was eighteen. Even if these be omitted in reckoning up the number, twenty patients are left, or 40 per cent., with some definite focus of sepsis. This is exactly the same percentage of cases with focal sepsis as was found in my previous series of 100 private patients.

Focal Sepsis as a Cause of Lenticular and Vitreous Opacity.

The statistical information with regard to two series of patients seen in private consultation, may be summed up as follows : A surprising number of patients who attained full vision after correction of the ametropia, exhibited changes in the eye. The most striking of these changes were opacities in the lenses and in the anterior part of the vitreous chambers.

Lenticular opacity was present in 20 per cent. of the patients in the first series of cases, and in 38 per cent. of the patients in the fresh series. The difference is owing to greater care having been taken in the second series in the detection of minute opacities with the slit-lamp.

The character of the opacity is that described as "cataracta senilis præmatura punctata," in Parsons' "Pathology of the Eye."

Vitreous opacity was noted in 75 per cent. of the patients. According to Butler, *British Medical Journal*, May 31, 1924, the vitreous in some cases is full of fibrillæ, of membranes, and of particles, and yet may be perfectly transparent, while in other cases it seems to be almost optically homogeneous. Miss Mann describes the vitreous as possessing a fibrillar scaffolding permeated by a jelly (*Transactions of the Ophthalmological Society*, 1927).

Nevertheless, in ordinary clinical examination with the slit-lamp, carried out on private patients, I have frequently been unable to detect any opacity, apart from vestigial filaments.

It has therefore been difficult to resist the conclusion that opacity in lens and vitreous is a variation from a physiological standard, and may have a pathological significance.

With this in view I undertook in two consecutive series of private patients with full visual acuity, after correction by glasses, the line of investigation which has been described. I found that 40 per cent. of the patients presented some form of focal sepsis, sometimes actually dangerous, sometimes only potentially so.

During the past year I have not seen any case of opacity of the lens, whether slight or complete, in hospital or private practice, in which some focus of septic or toxic absorption was not demonstrable in the body or in which there was not a history of some such focus.

There seems, therefore, ground for the theory that chronic focal sepsis is the most important factor in the causation of lenticular and vitreous opacity.

There appears to be no difference in the effects of this focal sepsis whether situated in the mouth, pharynx or intestine.

If there is a possibility of focal sepsis being an ætiological factor in the production of cataract, it seems to be of importance to recognize early lenticular opacity and to endeavour to track it down to its source, in order to check further change.

I have never observed alterations in opacity of the lens which can be attributed to any form of treatment. Opacity in the vitreous can not infrequently be observed to diminish when a focus of sepsis has been dealt with. I have observed this several times; while it is not uncommon for a patient to volunteer the information that he sees better after extraction of a tooth with an apical abscess, and actually to improve from $\frac{6}{6}$ to $\frac{6}{5}$, or from $\frac{6}{5}$ to $\frac{6}{4}$.

I have found it to be unwise to neglect the subjective symptom of muscæ volitantes. In most cases a careful investigation will show that there is some actual or pre-existing sign of sepsis to account for the definite opacity which is always present in the vitreous in these cases.

If the ocular changes are the result of various forms of focal sepsis the exciting agents must be living or dead bacteria, or the toxic products of bacteria or the disintegration products which result from the action of bacteria on living cells. It is probable that toxins and disintegration products most usually create the exciting agent in intra-ocular changes, and their path must be by the general blood stream.

It has been pointed out to me by Professor Elliot Smith that there is a bare possibility of toxic material passing up within the sheaths of the dental nerves to the Gasserian ganglion: thence within the sheath of the ophthalmic division of the fifth nerve to any part of the eye.

In conditions of oral sepsis there is the obvious route through the pharynx, nose and lacrimal duct to the conjunctival sac. This is the route whereby bacterial infection or toxic material travels in certain conditions of blepharitis and conjunctivitis, as is shown by the rapid cure of the ocular condition when the focus of sepsis is removed.

How the toxic material reacts on the ocular tissues it is difficult to say, but presumably it is by interfering with their nutrition, and perhaps by changes in the vessels of the ciliary body and choroid.

Importance of Ocular Evidence of Focal Sepsis.

A distinguished foreign ophthalmologist has sent to me a reprint of a recent paper in which he states that he has never seen any condition of ocular disease dependent on dental sepsis. It is my hope that a report of the present discussion will indicate to him the methods of setting to work to find foci of sepsis whether they be in the teeth or elsewhere.

I believe it to be of the utmost importance that all branches of the medical profession should realize the extraordinary frequency of dental sepsis even among people of means sufficient for obtaining skilled dental treatment.

Such sepsis may lead to the development of slight changes in the eyes, which in certain persons is compatible with the retention of full normal vision, but in others leads to grave ocular sequelæ.

It is not desirable for me to discuss now the other ocular changes which, in my experience, commonly occur as the result of focal sepsis. Among these are, severe blepharitis, Meibomian cysts (acute and chronic), conjunctivitis, episcleritis, corneal

ulcer, dacryocystitis, certain forms of iritis and cyclitis, central choroiditis, thrombosis of the central artery of the retina and of the central vein or its tributaries, and detachment of the retina. In all these conditions I have seen in hospital or private practice, during the past year, numerous examples in which I attributed the cause of the condition to gross focal sepsis, this and no other cause being present.

Whether or not the conclusions which I have drawn are accepted, I think it must be conceded that it was to the advantage of the patients concerned that they should have been made aware of dangerous physical conditions. While no advantage accrued to the ophthalmic surgeon, it is a satisfaction to have felt that within the limits of one's experience, professional advice has been given which, in many cases, has resulted in better health, and in some cases in an improvement of the ocular condition.

No originality is claimed for the theme of this paper. I learnt the outlines of the importance of focal sepsis from William Lang a quarter of a century ago, and more recently from Sir William Willcox.

I have been fortunate in having colleagues who have given essential assistance in the investigation of cases. Among them are Mr. Gilbert Chubb, Mr. Ormerod, Mr. Bowdler Henry, Mr. Frank Lawrence, Dr. Coldwell and Dr. Allchin of Westminster Hospital, and Mr. Hope and Dr. Livingston of King's College Hospital.

Discussion.—Mr. C. BOWDLER HENRY said there were two questions which these investigations would help considerably to settle. (1) Could the ophthalmic surgeon show that present sepsis was active, and that the patient's system was suffering from it? (2) Could the ophthalmologist take such a record of the eye conditions, for example, vitreous opacities, as could be checked in a year or two to find out whether they had increased in cases which had been doubtful? Mr. MacCallan had rather weakened his argument by saying that he had met none of these cases in which there was not either sepsis, or a history of focal sepsis. It was almost unknown for a dental surgeon to examine an adult patient and not find sepsis, or a history of sepsis; and it was correct to say that 95 per cent. of the population had some sepsis in the mouth. The dental surgeon's greatest difficulty was to know whether the sepsis was doing harm to the patient at the time. Mr. MacCallan and himself were on the staff of the same hospital, and he (the speaker) had taken the ocular signs found as an indication of the appropriate dental treatment. An illustrative case was that of a middle-aged man from Egypt, who would not confess to being ill. There was a medium degree of pyorrhœa, and he (Mr. Henry) thought that the teeth should be extracted, but was not quite sure. Mr. MacCallan's report of the ocular condition was adverse, and so it was decided to take the teeth out. Before this, however, they were carefully cleaned, and during this process the vision improved, a newspaper could be read without glasses for the first time. After removal of the teeth there was a further improvement in the vision.

In another case this year he had been considerably troubled. The patient was a girl, aged 16, who had only had unskilled dental treatment. The dead teeth had been left and the pulp canals were septic, fillings having been put in so as to cork up the sepsis. Over the central incisor there was a large area of rarefaction, and there were five other dead and septic teeth. Mr. MacCallan examined the patient, but no ophthalmic abnormalities were found. It was decided, however, that she should be examined at three-monthly intervals for a year. He (the speaker) began by conservative means to make her teeth as healthy as possible. The pulp canals, before they were sealed up, were sterile. Six weeks ago Mr. MacCallan had found slight signs of vitreous opacity, probably due to the dental manipulations having stirred up the septic material. He would be interested in the next examination of the eyes.

The common belief that mere extraction of the teeth meant the end of the sepsis was a mistake. Sepsis in the tooth did little harm if it was draining away, but when it was in bone surrounding the tooth, extraction of the tooth did not eliminate the sepsis from the bone. Two years ago he had had a patient whose teeth had all been removed because of rheumatism; there had been severe pyorrhœa. During the five years following removal, the symptoms had improved so much that she was almost normal. But she then declined again, and though edentulous, suffered a return of the rheumatic condition due to toxæmia from residual sepsis in the jaws.

Sir William Willcox had reported a case of dental toxæmia which was fatal, yet often one could not convince patients that they were actually ill owing to the sepsis. These ocular signs would be a great help.

The PRESIDENT asked whether some teeth, the incisors or the canines for example, were more likely to give trouble than others. A dentist had told him that the lower incisors were often to blame; they did not cause pain, but were often the seat of sepsis. After all the teeth had been removed, how soon could one be assured that the sepsis was removed? For instance, how soon afterwards could a cataract extraction be done?

Mr. M. S. MAYOU said all ophthalmologists were aware that dental sepsis was a serious condition; it was said to be present in 95 per cent. of the population. Lens and vitreous opacities were also very common, especially if looked for with the slit-lamp. How far one condition was dependent on the other it was difficult to decide. Mr. MacCallan had submitted a postulate, but there was still proof wanting to show that the connexion was as close as contended.

Sir ARNOLD LAWSON said he agreed with Mr. Mayou. Because one found dental sepsis in a patient with eye trouble, one was apt to assume that the former condition caused the latter. In many cases of eye sepsis he (the speaker) was convinced there were other possible causes, even though the teeth were found to be septic. As he had said at the last meeting of the Ophthalmological Society, it was fantastic to consider dental sepsis as necessarily the prime cause of eye trouble, because it happened to be present. He had had the necessity of searching for every possible cause brought home to him in the case of a patient who had come from Brazil to see him. In Brazil this patient had been treated energetically for syphilis, but he had no syphilis. His condition was very serious, and he (the speaker) had him pathologically examined. There was oral sepsis, as shown by the skiagrams. The teeth were treated, but the patient did not improve much. He gave a history of chronic abdominal trouble, and there was a family history of sepsis about the gall-bladder; a sister and brother had had the gall-bladder removed. This patient underwent an operation, and a septic gall-bladder was found and removed. From that moment he began to improve, and he returned to his work in Brazil in about three months' time.

Mr. B. CRIDLAND asked whether there was any relationship between an impacted tooth and sepsis; because a tooth was impacted, did it necessarily become septic? Mr. MacCallan had mentioned five cases of impacted teeth.

Mr. BOWDLER HENRY said that impacted teeth probably did no harm, provided they were not infected, presuming, of course, they did not cause pain by pressure on some nerve. Probably a tooth which was lying against another, as in the case of impacted molars, often became infected in its crypt, around the crown of the tooth, so forming a pocket of pus, infection passing down the side of the neighbouring tooth. But in the case of an impacted tooth, which showed no radiographic signs of infection in the surrounding bone, it was difficult to know whether the crypt around the tooth was infected.

Mr. MACCALLAN (in reply) said he agreed that dental sepsis was not the only cause of the eye conditions to which he had alluded; indeed, he had shown that a number of other conditions had existed in the patients, e.g. appendicitis or tonsillar disease. The teeth, however, were among the most usual sites for sepsis. He did not think it possible to say how soon after extraction of teeth such an operation as that for cataract could safely be carried out. Unless skiagrams were obtained it was not possible to tell whether the whole of the diseased teeth had been removed, and there might still be residual sepsis. He agreed with those speakers who said that more proof was required to establish the thesis, but he hoped that in the future some experimental inquiry would take place as to the formation of cataract resulting from dental sepsis.

Section of Psychiatry.

[*December* 11, 1928.]

Delinquency.

By W. A. Potts, M.D.

(*Psychological Expert to the Birmingham Justices; Physician to the Mental Wards, Hallam Hospital.*)

Delinquency is an extensive subject and can be approached from many different angles. In the time available I can only advance some ideas, and describe a few cases.

The problem is: Why did the offender act as he did? If that can be solved he may be dealt with satisfactorily. But here is the difficulty—and until the public have more understanding, we may not be able to make necessary arrangements for treatment or training; nor shall we always be given sufficient time to set the patient on his feet with a fair chance of success. We have all heard of lightning changes of character, or at least outlook, apparently permanent; every practising psychologist knows the almost magical results that occasionally follow one interview. But such happenings are so exceptional that we must put them on one side, and recognize that time and hard work are essential. The time required will be shorter only when we are asked to tackle the earliest indications, not the results of years of misunderstanding and neglect. When we seek the reason for going wrong we cannot hope to elicit it from police reports, by questioning the offender—least of all in court—or by interrogating the parents, or through reports of school attendance and medical officers. We can get at the roots only by studying the delinquent in his environment either ourselves or with trained assistants. Briefly, the cause is failure to adjust to social life as he finds it, either from mental or physical inadequacy, or both. It may be said that often it can be traced to natural instincts and emotions, which the culprit neither understands nor know how to control, coupled with ignorance on his own part or on that of those responsible for directing his life.

It is simplest to start with broad psychological and physical classifications; on these lines I will give one or two examples.

An attractive little school girl, 12 years old, was found taking pennies out of the pockets in the cloak room. The headmistress said she must leave unless she came to me for treatment, and I guaranteed it was safe for her to remain. Physically there was nothing abnormal; she had a capable and successful father and an invalid mother. She was clearly a "Mary Rose," and some of you may accept an unrecognized mental concept, the formula of which was: "As long as I take pennies, of which I have no need, you cannot say I am growing up and must accept the responsibilities of adult life." She did not dream, but as she admitted writing fairy stories, I made her write some and we analysed them together; the unexpected ideas and sentiments extracted from her compositions gave her a new conception of herself and her importance and duty in the world. She attended only a few times in three months and there was no subsequent trouble. One item in treatment was to stop her indulgent father asking her when she started for school in the morning if she wanted any money, and, if the answer was in the affirmative, handing her half-a-crown or five shillings; instead she had a regular allowance. It was also pointed out to her that her mother's invalidism was not the lot of all women, but the result of neglected illness.

Shortly afterwards a little girl of 10 came from the same school; she had appropriated money, books, pencils, etc. As she appeared to be well, and at once admitted a wish to be a boy, partly, no doubt, because her mother had to divorce the father after a long record of irresponsible and unsatisfactory conduct, I began with some psychological talks. Unfortunately there was a serious recrudescence

of the offences. Remembering that there was an indefinite account of a possible attack of " sleepy sickness " two years previously, I took swabs of the nose and throat ; a profuse infection of streptococci and pneumococci was found, and so vaccines instead of homely instructions were administered. A few weeks later the mother wrote to say that the vaccines were doing far more good than my moral talks. Here the vaccines were important, but it is only fair to say that I induced the mother to develop in the child satisfaction with being a girl, and conviction that to belong to one sex and wish it were the other leads to an unsatisfactory state. There was no repetition of the offences.

This draws attention to the importance of slight and often unrecognized attacks of encephalitis lethargica, one of the outstanding results of which is permanent or occasional diminution of the sense of responsibility. This explains more bad conduct and suicides than is generally realized. The post-encephalitic ends his life not because, like the typical melancholic, he feels so miserable that he commits what he considers a wicked act, but because he is a little bothered or bored with life, and has lost any sense of duty to himself, his wife or his family. Serious disasters may follow mild, neglected or even unrecognized cases of encephalitis, unless the condition is thoroughly treated, and it is certain there is no lingering infection left.

[Dr. Potts then described a serious case of stealing and burglary by a young man, the details of which it is not desirable to publish. The abnormal behaviour was due to a number of personal, family and school difficulties. Treatment consisted of analysis once or twice a week during four months, after which no further treatment was necessary.]

Two and a half years later a satisfactory report was received and at a later date I saw the patient again. Although his conduct had been good and he had earned his own living during the whole time, I was not sure about him because his father had not trained him for any profession or special business; also he looked ill. He had suffered from malaria when abroad, and was said to have been treated. As I was sure he was still suffering from malaria, I sent him to a specialist in tropical diseases, with marked benefit. Malaria neglected, or treated but not cured, is, like encephalitis lethargica, responsible for some forms of strange conduct. He returned a few months ago, quite fit physically and holding a good post in a large business, with possibilities for the future. After six years I believe he is safe, especially as his whole outlook on life has changed and he has good prospects.

The history shows that this young man was not mentally defective, yet at one stage of his career he was certified under the Mental Deficiency Act. A diagnosis of moral imbecility, just like that of " nerves," or a neurosis entirely due to the patient's own fault, is sometimes glibly given by those who do not understand. But moral imbeciles are rare, and some who are branded as such no more deserve to be than did this patient.

[Dr. Potts then described the case of a student who was arrested but not prosecuted for stealing. The chief cause of the unsatisfactory and idle life he had led for four years was that, intended by Nature to be an engineer, he had been studying medicine, because his father, although ostensibly allowing him a free choice, had, without realizing it, so discredited engineering as a satisfactory and profitable occupation, that the son could not see that it was the only satisfactory work for him.]

This case is a good illustration of the young man supposed to be free but with his eyes turned in the wrong direction by the misleading maxims of the parent. In addition to the change of profession, he had other help, because, when he first came, looking ill, there was a history of repeated nasal catarrhs and a profuse growth of pneumococci, streptococci and *Micrococcus catarrhalis* in the nasal and throat swabs. Twelve vaccine injections, with a short talk at the time of administration, was all the treatment he had in addition to the change of work, which was the core of the whole matter.

Suitable occupation is essential, especially with sensitive natures, but suitable recreation which provides an outlet for any special talent is as important.

A married man with a family, in an important position, convicted of indecent exposure, was fined. Imprisonment, which was nearly his lot, would have broken and ruined him. An undeveloped sense of responsibility associated with giving first consideration to his old father, with whom he was tied up in business, rather than to his wife and children, was a factor. But he volunteered the statement that though my talks about duties and relationships had helped him, that help was as nothing compared with the benefit from my insisting on his going to concerts regularly, and especially playing the piano, a recreation he had not had for several years.

Ignorance sometimes explains stealing. The child brought up in a poverty-stricken home where there are no private possessions, and most articles are common property, may not understand that it is not right to take for himself any cap or coat he sees. Every grade of life has its special difficulties. The lack of privacy in an overcrowded home makes the development of modesty, self-control and many other desirable qualities difficult, if not impossible. Besides, for proper healthy development everyone, even children, must have a chance of being alone sometimes. This the poor may never have.

There are special difficulties for an enterprising young woman going out to remunerative work if she lives in a poor and overcrowded home, with a strict father, who believes in all the family being at home after working hours. Even if she only spends some of the money she has earned in going to a dance in the evening with a group of boy and girl companions, she comes home to hear her father denounce her as a "bad lot" because she stays out after dark and has been in the company of the opposite sex.

I cannot explain to you how it works, but it is a fact that unsatisfactory conduct on the part of a parent, however carefully concealed, may be responsible for misdemeanour by the child. There must be an honest, healthy atmosphere in the home. In the same way friction between the parents, though supposed to be kept from the children, may be enough to explain their unsatisfactory conduct.

Good home relationships are essential for good conduct in the young. But unless they are hopelessly unsatisfactory or there is bad example and training, home is the proper place for a young delinquent. Institutions are a poor substitute: a better plan is to place the child with a foster-mother or guardian. Suitable persons to fill such posts are hard to find and the system is expensive. An alternative is a hostel or small home, with sympathetic people in charge.

If we think over the requisites in the environment for the healthy development of a child, we find a key to the help, training and instruction required. First, he must have love, and develop the sense of security that love brings. Then he must have an opportunity for developing his own individuality; this freedom must be tempered by appreciation of the rights of others, a training achieved only by association with other children, preferably brothers and sisters. With other children, too, he gets the opportunity of initiation into team-work, for which games, properly played, are valuable. He must have good examples in front of him in the older members of the household, and through them must get glimpses of the end result of training and teaching. He must not be encouraged to accept his father and mother as extraordinary people who have attained heights far beyond him. If his father is a successful man, the child must understand that this is only because he had the proper preliminary training and chose the work for which he was best fitted.

All ordinary instincts, such as curiosity, love of adventure, love of display, fighting, etc., must be guided into satisfactory channels. He must learn that it is not wise to settle a dispute by blows, but by giving due consideration and attention to the claims of both sides and insisting that justice shall be done. In connexion with animal instincts, he must gradually understand through his mother, and by simple answers to his questions, from an early age, what sex and life mean.

The problem with delinquents at a late stage is often to find out what earlier training has been faulty or neglected and to see that something is done to compensate for this.

The improvements in dealing with delinquency in recent years—no imprisonment for children, children's courts, probation and so on—are great and far-reaching. But we shall only get the full benefit when those in charge of such work understand what they are doing. Probation is a great help, but it must always be conditioned, the conditions being made after full consideration of the individual and the home. It is important that it should be conditioned as regards time, which should never be short —e.g., for six months only—but continued or renewed as long as necessary.

Our aim should be prevention of serious misdemeanour. This can be achieved by social betterment, including better housing and more public parks and open spaces. The School Medical Service should be developed so that it can deal with both physical and psychological difficulties. As teachers are often unable now, with large classes and other heavy demands, to study individual lives, there must be child guidance and organizations for helping in the choice of occupation.

Finally, magistrates should never dismiss the first offender who has often been going wrong for years and is probably living in an unsatisfactory environment, for which reason he requires much help.

Discussion.—Dr. J. CARSWELL said that although for many years past juvenile delinquency had compelled the authorities, both central and local, to make provision for the institutional care of young delinquents, the tendency during recent years, at all events since the war, had been to use that method much less. The expense was considerable and the results were doubtful. Magistrates were using the system of probation for juvenile offenders to an increasing extent.

Psychiatrists need have no regrets about the decline of the institutional method. In any case, in practice, it was sadly lacking in the scientific spirit, and few medical officers to these institutions had taken interest in the medico-psychological problems presented by the children under their care.

As one who had given twenty-five years' personal service to this cause—mainly on the administrative side—he could assure Dr. Potts that his work was well worth doing, and full of scientific and administrative value for the future.

Dr. E. J. BOOME said that he would have liked Dr. Potts to discuss more fully the differential diagnosis between the ordinary delinquent and the moral defective. He (the speaker) suggested that the offences might be more motiveless in the case of the moral defective. [Dr. Boome quoted a case in support of this view.]

He agreed that to put young offenders on probation for too long a time was a mistake, as was also the actual dismissal of cases by magistrates, which led to a habit of crime more difficult to remove.

He would plead for a more extended use of industrial schools, where the children nowadays had much more sympathetic treatment than in the past. Of course, when possible, "boarding out" had advantages over institutional care.

Dr. CRICHTON-MILLER said he was glad that the lecturer had cast doubt upon the category of the "moral imbecile." He (the speaker) suggested that in almost every case this was a phrase used to denote the pathological swindler. There was a common tendency to classify on a basis of supposed congenital defect, behaviour of which the motivation was obscure. In a high proportion of juvenile delinquents, truancy had been the first manifestation of maladjustment, and he urged that if truancy were studied as a significant conduct reaction, we would frequently find the future delinquent before he had reached a more serious stage in his career.

With regard to the claim that the industrial schools were "superbly well" managed, he (Dr. Crichton-Miller) would like further information. It had been said, for instance, that half the accommodation was at present unused. If this was the case, would it not be possible to use the accommodation and funds available for something more constructive in the service of the maladjusted adolescent?

Section of Electro-Therapeutics.

President—Dr. J. E. A. LYNHAM.

[*December* 14, 1928.]

Treatment of Pelvic Inflammations by Diathermy.

By C. A. ROBINSON, M.B.

ABSTRACT.—The paper consists of two parts: (1) A brief history of the manner in which the treatment arose and a description of the methods now used. (2) An analysis of the cases treated from 1921 until the present time.

The treatment of pelvic inflammations by heating by the diathermy current arose out of the method of treating gonococcal infections of the cervix and urethra by this current. An essential part of the treatment is in many cases the treatment of the cervical infection.

Diathermy treatment of gonococcal infection was founded upon the fact that the gonococcus is vulnerable to small rises of temperature above the normal. Before the war Dr. E. P. Cumberbatch found that recovery of gonorrhœal arthritis could be procured by heating the affected joints by diathermy. During 1919 the method of heating the cervix and urethra by means of metal electrodes introduced into them to rid them of gonococcus was arrived at.

The maximum temperature to which cervix and urethra are heated is 115° F. The duration of the application is ten minutes and the number of repetitions is generally from three to eight.

It was found that in cases of gonococcal arthritis it was necessary to treat the cervix only, recovery from the arthritis then taking place.

Recovery from arthritis due to infection of the cervix by other organisms than gonococcus, was found to occur and in such cases there was recovery from cervicitis and erosion.

When salpingitis was present in cases of gonococcal and other infections of the cervix, a method of producing a milder and more general heating of the pelvic contents was introduced. This was to some extent founded upon the known effects of heating the infected prostate.

The temperature produced in the neighbourhood of the electrode is 106° F. The applications last twenty minutes.

Tables summarizing the clinical results are attached.

I.

IN recent years an instrument producing high-frequency currents has been put into the hands of those practising electro-therapeutics, enabling them to heat deep tissues, and some at least of the deeply situated organs of the body. Except by the production of a general rise of body temperature, such as occurs in artificially-produced pyrexia, there is no other means which will do this. It is well recognized that a higher temperature can be produced locally by these high-frequency currents than by means of a general rise of temperature.

There is no reason to assume that the effect of these high-frequency currents is due to anything other than heat. When we consider the effects of all chemical processes of a rise of temperature, the effect of heat is a sufficient explanation of the phenomena.

The inception of the work which is about to be described took place in Dr. E. P. Cumberbatch's Electrical Department at St. Bartholomew's Hospital, and it is with his kind permission that I am able to present the cases which follow.

These cases came with a diagnosis made by a gynæcologist. Although I have carried out this work according to the methods which seemed to me the best and have had a free hand in the fashioning of instruments and the working out of the technique, valuable advice on all matters connected with the subject has always been open to me and has been freely and generously given by Dr. Cumberbatch. Dr. Alastair McGregor has also been of inestimable service on account of his encouragement and advice.

The method to be described is a method of treating pelvic inflammations by heating the pelvic contents by a high-frequency oscillating current.

A diathermy current passed between electrodes, placed back and front, may or may not be good, but the pelvic contents are not heated by this means.

If, during a treatment so administered, a thermometer is placed in the cervical canal, no rise of temperature will be recorded.

The treatment of pelvic inflammations by heating by diathermy current arose, as will be seen later, out of the method of treating gonococcal infections of the cervix and urethra by this current.

Diathermy treatment of cervix and urethra for gonococcal infection was founded upon the fact that the gonococcus is vulnerable to comparatively slight rises of temperature above that of the normal temperature of the human body, but whether this is the only factor concerned is open to question.

It was known that attempts had been made to free the male urethra from gonococcal infection by means of sounds, heated by hot water circulating through them. These attempts failed, because heated sounds would, of course, not heat the tissues immediately underlying the mucous membrane. These deeper tissues can, however, be heated by a diathermy current to some depth, and it was decided to try the effects of heating by this means.

Dr. E. P. Cumberbatch, just before the war, had treated arthritis by heating the joints by this means, and had observed that those cases which made a recovery were always due to gonococcal infection, and that there was no such beneficial effect on other cases of arthritis.

As a result of this it was determined to attempt to rid the cervix and urethra of gonococcal infection by using the diathermy current to heat these tissues. During 1919 the method of heating the cervix and urethra by means of metal electrodes introduced into them was arrived at. The cervix can be heated to any required temperature without an anæsthetic as it is insensitive, but this of course is not so in regard to the urethra. An anæsthetic is undesirable because these patients must be treated as out-patients.

The temperature at which the sensation of heat changes to pain is 115° F., and the urethra cannot therefore be heated beyond this without an anæsthetic. Moreover, up to this point no inflammatory reaction occurs and, if it is not exceeded, no pain or discomfort follows the treatment. If it is even slightly exceeded some symptoms indicating inflammatory reaction, such as pain on passing water, will occur on the day following the treatment. As it is necessary in cases of gonococcal infection to treat both urethra and cervix at the same sitting and an anæsthetic is practically out of the question and inflammation following the treatment is to be avoided, 115° F. is taken as the maximum temperature allowable. It was realized that if this temperature was insufficient the method would fail. There are two factors in the production of the lethal effect on the organism, namely temperature and time. It was determined empirically that this temperature of 115° F. was sufficient if it was maintained for ten minutes. The number of times this had to be repeated in the case of gonococcal infection was found to vary considerably, but generally six to eight bi-weekly treatments are sufficient.

The technique now used is as follows: An electrode is introduced into the urethra and by means of diathermy currents the tissues immediately surrounding the electrode are heated to a temperature which is believed to be 115° F. This temperature is estimated by the patient's own sensation and is the point where the sensation of heat changes to pain.

The insensitive cervix is then treated in a similar manner, the current used to produce the temperature in the sensitive urethra being taken as the standard for producing the same temperature in the insensitive cervix.

This method was first used nine years ago and up to the present very few cases refractory to it have been met. Moreover, it was found that in cases of gonococcal arthritis in women it was necessary to treat the cervix only, recovery of the arthritis

then taking place. In 1920, cases of arthritis not gonococcal in origin but associated with a cervicitis, were similarly treated with success. In cervicitis due to infections other than gonococcal, recovery may be procured and erosions healed. In such cases a greater number of applications may be required.

To illustrate the effect in non-gonococcal infective arthritis of treating an associated cervical infection the following case is used. It is taken because it is the last case of the kind to be so treated at St. Bartholomew's.

387. 28. A. B., aged 42, confined in January, 1928. In March, 1928, the right knee became swollen and painful, and two weeks later the left became similarly affected. She had had vaginal discharge since the confinement.

On July 17 she was in bad general health, the hands could not be clenched and there were spindle swellings of the mid inter-phalangeal joints, especially of the right ring finger. The wrists had limited range of movement and there was swelling of the back of the wrists. There was a definite cervicitis with erosion.

Diathermy was applied to the cervix. After two treatments the wrists were more freely movable and less painful. On August 11 after nine treatments there was no pain. She could sit up in bed and the wrists moved through 60°.

On September 6, after eighteen treatments in all, she could walk, but not well. All joints were free from pain, painless on movement, and moved through full range, except that the wrists were somewhat restricted. She had no further treatment after September 6.

She was seen again on October 9, when all joints moved painlessly through a full range except the right wrist, where flexion was slightly restricted. There were no swellings and no pain. The general health was good. There was no discharge, no cervicitis, and the erosion was healed.

During 1919 I had been engaged at St. Bartholomew's in treating cervicitis and cervical erosion by means of ionization, using some modification of the methods of Apostoli and Leduc.

French writers on uterine ionization always insist that direct current applications must never be made if there is any inflammation of the uterine appendages. Moreover, French authors state that pain in the lower abdomen during such direct current treatment, is diagnostic of inflammation of the pelvic organs.

In treating for gonococcal infection and for arthritis, I had noticed that pain in the lower abdomen had sometimes occurred during treatment by diathermy. This occurred in cases in which other signs of salpingitis were absent. On September 20, 1921, a patient came for treatment of vaginal discharge accompanied by definite signs and symptoms of salpingitis.

395. 21. A. S., aged 31 (S.T.C.). There was a history of vaginal discharge for many months. Signs and symptoms of salpingitis were present. Gonococci were found in the smears of cervix and urethra. The first application of diathermy was given on September 20, 1921. After this treatment the patient developed pain in the abdomen with tenderness and rigidity in the right iliac fossa. Vomiting occurred and the temperature rose to 102° F. The patient was admitted to hospital where she remained two weeks. Symptoms had subsided by October 4. After this diathermy was resumed. A week later the discharge was decidedly less, but there was still pain in the lower abdomen and tenderness on deep palpation. On October 14, there was no pain or tenderness.

On November 7, although symptoms of salpingitis had subsided, gonococci were found in abundance in the urethra but not in the cervix. It was known that the patient's husband was attending the Special Treatment Centre for gonorrhœa. On January 16, 1922, gonococci were found in the cervix, not in the urethra. Shortly after this the patient got a judicial separation from her husband after which diathermy treatment was resumed. Applications were made on January 24 and 27. On February 23, and March 2, no gonococci were found in urethra or cervix. On April 14 there was no discharge, her general health was good and she appeared to have made a complete recovery. She had then remained free from salpingitis for six months.

It was determined in future to use a milder degree of heating in such cases hoping that a subsidence of the salpingitis might thus be produced. The cervicitis

might be satisfactorily treated afterwards. I was at this time familiar with the effects on prostatitis due to gonococcal and other infections of heating the prostate by diathermy by means of a metal electrode introduced into the rectum. The resulting subsidence of pain, and frequency of micturition, recovery from arthritis and other metastases, and even recovery of the prostate from the infection, were remarkably constant.

The electrode at first used for this mild heating of the pelvic contents in women was the largest sized Hegar's dilator with a terminal screw attached to the lower end. The indifferent electrode was a 10 per cent. saline pad on the abdomen.

The intrapelvic electrode now used is 1 inch in diameter. It is sheathed in vulcanite except for $1\frac{1}{2}$ in. at the end which is to be in contact with the upper end of the vagina. This end must be rounded so as to give an even distribution of the current on account of the tendency of high-frequency currents to come off at points or edges. The indifferent electrode used at present is a lead belt, $2\frac{1}{2}$ in. wide made of one pound of lead (gauge 24 S.W.G.) and fastened by a clip which also serves for an attachment to the machine.

With these electrodes a current of 2 amps. gives a temperature of 106° F. in the tissues in the neighbourhood of the metal part of the vaginal electrode. How quickly the temperature falls off from one electrode to the other is not known, so that the actual heating of the tubes and ovaries cannot be determined.

The treatments last twenty minutes each and are given twice a week, Later on in describing the cases this may be remembered and the total duration of the course of treatments may be roughly reckoned from the number given.

II.

After the case 395. 21, already described, a few cases were treated at the West Middlesex Hospital and elsewhere. In 1923, cases began to be available at St. Bartholomew's, on account of the interest taken by Dr. Wilfred Shaw.

The cases here tabulated and described were sent by one of the gynæcologists at St. Bartholomew's, from Dr. Malcolm Donaldson's department, by Dr. Wilfred Shaw, Mr. Lane Roberts and Mr. Burt White, and by Dr. Oldershaw from the Soho Hospital for Women. I am indebted to Dr. Oldershaw for his interest, kindly criticism and help, and for some of the reports on the after-result of the treatment.

TABLE I.

Type of case	With cervicitis	Recovered	Partly recovered or relapsed	Failed	Inconclusive from short course	Unsuitable	Total
Inconclusive from short course ...	—	—	—	—	6	—	6
Unsuitable	—	—	—	—	—	4	4
Salpingitis of undetermined origin ...	10	12	4	—	1	—	17
Following confinement	7	9	1	2	—	—	12
Due to gonococcal infection	5	7	—	—	—	—	7
Following operation for extra-uterine fœtation	—	2	—	—	1	—	3
Parametritis with deep laceration ...	1	1	—	—	1	—	2
Salpingitis following operation	2	2	1	—	3	—	6
	25	33	6	2	12	4	57

The total number of cases to be reported upon is fifty-seven. Four of these were unsuitable. One of them, for instance, was operated on afterwards and found to have a cyst of the broad ligament.

Of the forty-one cases which were considered suitable, thirty-three recovered (i.e., about 80 per cent.), six obtained some relief of their symptoms or relapsed after

apparent success, and two were unrelieved. There are therefore eight failures (about 20 per cent.).

Recoveries.—When cases are marked as having recovered it means that (1) they are relieved of symptoms connected with the pelvic condition. Many of these patients have been complaining of symptoms for a long time, and such an impression has been made upon them that any recovery from pelvic inflammation is not immediately followed by an alteration in their frame of mind in regard to their former complaints. Pains, discomforts or headaches are enlarged upon on inquiry as to their present condition. "Recovered" implies that they were relieved of symptoms which can reasonably be referred to pelvic inflammation. (2) Their general health is definitely improved. Instead of having a toxic appearance and being worn out from pain and sleeplessness they look healthy, sleep well and have an improved appetite. (3) There are no physical signs of pelvic inflammation; examination reveals no swelling which is tender on manipulation. Though there may be in some cases some thickening or other abnormality which the examining hand can still detect there is nothing which could be considered sufficient to justify the assertion that a return to health has not taken place. In some cases, so far as can be ascertained by inspection and palpation, a complete return to normal has occurred. The appearance of the cervix is normal and the erosion is healed.

The treatment is safe. No harm has ever resulted, except the acute exacerbation of the symptoms referred to as occurring occasionally during treatment of the cervix. No patient coming under the treatment has died and no operation has been necessary as a result of it.

In the tables which follow, the description of each case has had to be very brief in order to get it into the space available. *I.P.* indicates the mild heating by the vaginal electrode. *Cerv.* means that the cervix has been especially treated. The total duration of the course of treatments may be roughly reckoned by remembering that the applications are made twice a week.

TABLE II.—SALPINGITIS OR OVARITIS, OR BOTH, OF UNDETERMINED ORIGIN.

It is possible that some of these were gonorrhœal in origin.

No. and Date	Remarks	Treatment	Results
50. 24 ...	Tense, tender mass Douglas' pouch, 3 months ...	I.P. 3 ...	Pain relieved, still tender, operation
51. 24 ...	Inflamed left appendages. Duration 2 years ...	I.P. 10, interval 2 months, I.P. 9	Recovery. Slightly tender nodule in left fornix
55. 24 ...	Right tube and ovary inflamed, 3 months ...	I.P. 12 ...	Recovery. Pregnant
85. 24 ...	Inflamed right ovary and tube, 18 months ...	I.P. 12 ...	Recovery followed 1 month
116. 24 ...	Fluctuating swelling. Cervicitis, 18 months ...	Cerv. 5. I.P. 3 ...	Recovery
209. 24 ...	Both tubes enlarged, left is tender ...	I.P. 10 ...	Recovery
380. 24 ...	Left tube and ovary enlarged and tender ...	I.P. 10 ...	Recovery
407. 24 ...	Salpingitis. Cervicitis erosion, 6 to 7 years ...	Cerv. 4. I.P. 2 ...	Recovery
441. 24 ...	Salpingitis. Cervicitis erosion. 2 years ...	I.P. 3. Ions. 11 ...	Recovery
558. 24 ...	Dry pyosalpinx. Cervicitis, 6 years ...	I.P. 11 ...	Recovery
112. 26 ...	Double salpingo-oophoritis	I.P. 5 ...	Recovery
108. 27 ...	Tender mass right fornix. Cervicitis ...	I.P. 15. Cerv. 3 ...	Recovery. Subjective symptoms remain.
433. 27 ...	Tender mass both iliac fossæ. Cervicitis and erosion ...	I.P. 5. Cerv. 3. I.P. 5 ...	Recovery. Subjective symptoms remain
486. 27 ...	Double salp.-oophoritis, cervicitis and erosion ...	Cerv. 20 ...	Recovery. 9 months later well. 7 months, preg.
599. 27 ...	Salpingitis and cervicitis	I.P. 12. Ions. 5 ...	Recovery. Relapse of cervicitis
160. 28 ...	Swelling right tube, less so left. Cervicitis ...	I.P. 10. Cerv. 5 ...	Recovery. Followed two months
528. 28 ...	Cervicitis, subsiding salpingitis, left ...	I.P. 9. Cerv. 3 ...	Recovery

In 441. 24, healing of the erosion was procured by ionization. This in some cases is a more effectual or more rapid method than diathermy.

TABLE III.—CASES IN WHICH GONOCOCCUS WAS FOUND.

No. and date	Remarks	Treatment	Result
395. 21 ...	Salpingitis. G. found ...	Cerv. 5 ...	Recovery. Observed six months
162. 24 ...	Fluctuating swelling. G. found in cervix and urethra ...	I.P. 1. Cerv. 3. I.P. 6 ...	Recovery. Followed one month. No relapse
540. 24 ...	Arthritis and salpingitis. Aged 8 years ...	I.P. 8. *Per rectum* ...	Recovery. No arthritis, no salpingitis. Followed five months
165. 25 ...	Appendix. Right tube and ovary removed, tender swelling left fornix ...	I.P. 5. Cerv. 3 ...	Recovery
282. 25 ...	Enlarged thyroid and salpingitis. Cervicitis ...	I.P. 4. Cerv. 8 ...	Recovery. Thyroid not enlarged
301. 26 ...	General peritonitis 7 months ago with inflammation of tubes and ovaries ...	I.P. 3. Cerv. 3 ...	Recovery. Followed three months. Five examinations. No G. found
223. 27 ...	G. found in pelvic pus of peritonitis. Pain and tenderness. Rigidity right iliac fossa ...	I.P. 11 ...	Recovery

Of these, all made a good recovery after a comparatively short period of treatment. This is nothing more than anyone familiar with the effects of diathermy treatment of gonococcal infections would have predicted and requires no special comment.

In five of these cases gonococci were found in cervical smears—in one of them in peritoneal pus and in one, a child of 8 years of age, in vaginal secretion.

540. 24. Aged 8 years, was admitted to hospital October 9, 1924, with fever, abdominal pain, vomiting and rigidity with gonococci found in the vaginal discharge. October 18, 1924, she developed acute arthritis in the left wrist. Diathermy given per rectum began October 27, 1924, and was continued to December 14. On December 29 there was no discharge, the abdominal condition and the wrist had recovered. On May 1, 1925, there was complete recovery.

The final report on smears in all the cases was negative. The case 223. 27 showed no gonococci in cervical smears but these organisms were found in pelvic pus. The patient was last seen six months after the termination of treatment and was then quite well.

TABLE IV.—FOLLOWING CONFINEMENT.

No. and Date	Remarks	Treatment	Result
584. 23 ...	Cervicitis 2 years. Pain left iliac fossa ...	Cerv. 4. I.P. 12 ...	Recovery
84. 24 ...	Pain 2 years. Both appendages large and tender ...	I.P. 11	Recovery
359. 24 ...	Pain 1 year. Left salpingo-oophoritis. Cervicitis. Erosion ...	I.P. 1. Ions. 9 ...	Recovery
500. 24 ...	Pain 4 years. Tenderness both iliac fossæ. Cervicitis ...	I.P. 2. Cerv. 7. Ions. 3	Recovery
168. 25 ...	Bilateral-salpingo-oophoritis	I.P. 8	No improvement
375. 27 ...	After miscarriage 2 months ago. Swelling right fornix. Cervicitis and erosion ...	I.P. 5. Cerv. 3. Cerv. 11	Recovery. Relapse of cervicitis after 14 days. Curetted
514. 27 ...	After miscarriage 3 years ago. Swelling left fornix ...	I.P. 8	Still has pain. Failure
7. 28 ...	Pain 2 years. Erosion and cervicitis ...	Cerv. 7	Recovery. 7 months later still recovered
36. 28 ...	Post-puerperal. Swelling right fornix. Cervicitis ..	Cerv. 3. I.P. 16 ...	Recovery. Followed one month
137. 28 ...	Followed miscarriage	Cerv. 3. I.P. 11 ...	Recovery. 4 months later well and preg.
210. 28 ...	After confinement 5 years ago... ...	I.P. 7	Recovery. 5 months later 2 months preg.
295. 28 ...	After confinement one year ago ...	I.P. 11	Recovery

These cases made a good recovery as a rule. They all had physical signs of salpingo-oophoritis. 168. 25, and 514. 27, definitely failed after a month's treatment. I can give no explanation of this fact.

357. 27.—In this case the cervicitis and erosion recurred. In January, 1928, the patient appeared to have recovered, and again, on examination, on February 2. She was not seen again till October, when she gave a history of having been curetted in the meantime. There was a bright red rim round the cervical os which returned to normal after a few diathermy treatments. The salpingitis had not returned.

TABLE V.—PARAMETRITIS, WITH DEEP LACERATIONS.

There is sometimes good recovery from this condition.

No. and date	Remarks.	Treatment	Result
567. 23 ...	Left parametritis. Two years' duration ...	Cerv. 3. I.P. 6 ...	Recovery
562. 24 ...	Parametritis ...	I.P. 4 ...	Recovery incomplete. Was to have attended as an out-patient, but did not do so

TABLE VI.—FOLLOWING OPERATION FOR ECTOPIC FŒTATION.

No. and date	Remarks	Treatment	Result
524. 23 ...	6 to 7 months after operation	I.P. 3	Relief of symptom. She was to have attended as out-patient, but did not do so.
418. 25 ...	Tenderness and swelling left ovary and tube. Operation 10 years ago ...	I.P. 8	Recovery
576. 25 ...	Large hard tender mass low down in Douglas' pouch After operation 12 months before. Thin, ill and toxic looking ...	I.P. many	Small, hard, not tender swelling can still be felt. General health good

576. 25.—E. C., aged 42, married. She complained of pain in lower abdomen and increased frequency of micturition and dyspareunia dating from operation at the Waterloo Hospital in 1914 for removal of ectopic gestation. She complained also of pain in the back. She was admitted for operation but this was considered inadvisable until her condition improved. There was a large, hard, tender, rounded mass low down in the pouch of Douglas, which was diagnosed as uterus with adhesions.

She menstruated every three weeks with pain until the flow was established. She was thin, ill and septic-looking. On October 20, 1925, intrapelvic diathermy was begun. The introduction of the electrode caused pain. Treatment was given twice weekly until November 7, when she had a period lasting three days. It was resumed on November 10, till November 27. There was then no tenderness or abdominal pains and the mass was smaller. She had some pain in the back but the general condition was improved. A period occurred at the end of November which lasted only one day. She was kept under observation and had an occasional course of treatment between November, 1925, and March 2, 1928, on which date (two and a half years after the beginning of treatment) the cervix was found to be sclerosed but not red. There was no excess of discharge; the abdomen was not tender or rigid. The general health was good. The swelling, which could still be felt low down posteriorly, was quite small and not tender.

Her subjective symptoms varied considerably but at the time of her last visit she had none, and her general health was good.

The following case does not form one of the series used for the purpose of this paper as she was not treated at the hospital.

34. P. Mrs. W., married, aged 38. She had been living in South America. She has no children, but had a miscarriage in early married life. Four years ago she had an operation for extra-uterine fœtation and the right tube was removed. Since then she has never been well, suffering with abdominal pain and general ill-health.

In January, 1926, she is said to have had a miscarriage, but if so it was a very early one. Since this, loss has been excessive and almost continuous. On March 29 a loss began and

lasted till April 14. The amount lost was excessive on the first day. On May 1 she had another severe loss. She came home in order to get treatment for severe hæmorrhages with a presumed diagnosis of fibroids.

On May 11, when she was first seen, the bleeding was slight. The vagina and cervix were normal. There was no pain or tenderness. A hard, nodular mass was felt for a hand's-breadth above the pubes. She was blanched and breathless. On May 11 intrapelvic diathermy was begun and repeated twice a week. The slight bleeding ceased after two days.

Slight loss occurred on May 27, but was cleared up by May 31, except for a slight red discharge in the morning, which continued up to June 5. On this day her general condition was improved. Her colour was better. There was slight uneasiness on the right side, noticeable on walking.

After June 5 no blood-stained discharge was seen until July 4, when she had what appeared to be a period lasting three days and followed by a slight red discharge up to July 12.

Intrapelvic diathermy was given continuously twice a week from May 11 to July 30.

A period began on August 7 and lasted till August 11 without excessive loss. After this she had normal periods and appeared quite well till January 6, when she was last seen. On this date examination per vaginam revealed nothing abnormal, except that there was some thickening in the right fornix. She was quite well in general health. Soon after this date she left this country for South America.

TABLE VII.—CASES OF SALPINGITIS IN WHICH OPERATIONS HAD BEEN PERFORMED.

No. and date	Remarks	Treatment	Result
583. 23 ...	Two abdominal operations performed. Left appendages enlarged and tender	... Cerv. 3	... Symptoms relieved
139. 24 ...	Left ovary removed two years ago. Tender swelling left fornix	... I.P. 4	... Symptoms improved. She was withdrawn from treatment for curetting
160. 24 ...	Operations 1920 and 1921, for ovarian prolapse, tender swelling left fornix, and a larger one on the right	... I.P. 7	... Symptoms improved
147. 25 ...	Tender swelling right fornix. Left tube removed 14 months ago	... I.P. 3	... No pain or tenderness, swelling less
154. 25 ...	Left tube removed five years ago. Now has pain right iliac fossa. Cervicitis	... I.P. 4. Cerv. 6	... Pain relieved by I.P. treatment. After treatment to the cervix it was normal
600. 26 ...	Retained products of conception, January, 1920. Ventri-fixation 1926. Both appendages enlarged and tender	... I.P. 13	... Recovery with subjective symptoms, not connected with pelvic condition

The following is the kind of case I like to see :—A chronic case without rise of temperature or constitutional disturbance ; pain in one or both iliac fossæ, which may be severe ; no rigidity, but, of course, voluntary contraction on palpation of the tender part. *Per Vaginam.*—Tender enlarged appendages possibly prolapsed into the pouch of Douglas.

Pus is not necessarily a contra-indication ; the chronic pus sac may disappear as far as physical examination goes. Cases are likely to do well if there is evidence of infected cervix, with or without erosion, especially, of course, if there are gonococci present. From the point of view of the effect of treating the cervicitis the figures are very striking. Twenty-five cases have been associated with cervicitis. One of them was inconclusive, as the patient received three treatments only and was then withdrawn. This leaves twenty-four. In twenty-two of these cases the patients have recovered from the salpingitis, but two of them have had a return of the cervicitis. Two recovered as far as physical examination was able to establish the fact, but subjective symptoms remained.

Although in two cases the recurrence was of the cervicitis, not of the salpingitis, they can hardly be called successes. If they are included as failures

we have twenty successes, two failures and two doubtful successes, out of twenty-four cases, i.e., 83 per cent. successful and 17 per cent. unsuccessful.

Follow up.—A "follow up" of cases was carried out by Dr. Simon, of St. Bartholomew's.

There were fifteen cases of which one failed. Of the fourteen cases of recovery no reply was received in four; ten replied by letter or by attendance. Out of these ten, recovery was confirmed in eight, and three of them had become pregnant. Return of the cervicitis occurred in two. The confirmed recoveries are therefore 80 per cent. of those from whom answers were received. This is about the same percentage as for the whole series including those not systematically followed up.

In infections of the cervix and urethra none of the many different drugs applied to them are really satisfactory. This is due no doubt to the anatomy of the parts concerned and to the infection being in the depths of the tissues of the glands as well as on the mucous surfaces.

Chronic pelvic inflammation is often hardly a serious enough state to warrant an operation which is often difficult and involves removal of an organ essential to child bearing.

These infections and inflammations have always presented a difficult problem in gynæcology. I have hoped that diathermy offers a new method which will obviate a long and tedious period of treatment with swabs, tampons and douches, will render mutilations unnecessary and will bring about recovery in some cases in which operation is not likely to succeed.

Discussion.—Mr. MARTIN OLDERSHAW said: I have been in close touch with Dr. Robinson's work during the last few years. My attention was first drawn to the success of diathermy in cases of chronic cervicitis, and I soon became impressed with its value in more widespread gonococcal pelvic inflammation.

The present treatment of acute and subacute salpingitis is not very satisfactory. The usual rule is to adopt expectant treatment during the initial acute stage, limiting operative measures to the drainage of any abscess that may form. Many cases clear up more or less completely, but if at the end of six weeks, a pyosalpinx persists, this is removed by abdominal operation.

Recently, immediate operation in the acute stage has been urged by some. This seems unduly drastic, and also wrong in principle in the case of a disease which is rarely dangerous to life, which occurs typically in young women, and in which functional recovery is possible.

Our aim should be to avoid any mutilating operation whenever possible, confining operation to cases where the tubes are hopelessly damaged, or where there is actual abscess formation.

My initial scepticism of the value of diathermy in suitable cases of salpingitis was shaken by seeing the prompt improvement following its use in several typical cases—notably in a patient (Mrs. W.) who attended my Out-patient's Department at Soho Square. Both tubes were enlarged and thickened, especially the left, and there was the usual syndrome of backache, menorrhagia and leucorrhœa. Treatment with douches, tampons, etc., for three months had no effect, and I was considering operation, but decided first to send her to Dr. Robinson. She began to improve from the beginning of the treatment, and at the end of the course all physical signs had disappeared and she was quite well.

I have less experience of the use of diathermy in septic pelvic infections, e.g., a puerperal cellulitis, but my impression is that it is less helpful in these than in gonococcal cases.

Kelly, in his work on gynæcology, briefly mentions the work of Corbus and others in America on the use of diathermy in the treatment of pelvic disease, but Dr. Robinson has been largely ploughing a lone furrow.

I should like to see his work become widely known, and taken up by others, as I am convinced that we have in diathermy, when used in properly selected cases of pelvic infection, a safe and valuable means of treatment of an otherwise somewhat intractable condition.

Dr. WILFRED SHAW said that he had had opportunities of watching Dr. Robinson's work and had been impressed by the care with which the treatment had been carried out. He considered that there was a wide field for diathermy treatment in cases of pelvic infection. The treatment of cervicitis by diathermy was well recognized at the present time and he

wished to limit his remarks to cases of adnexal inflammation. In such cases the affections were frequently bilateral and involved not only the Fallopian tubes but also the ovaries, and surgical treatment directed towards removing the infection completely would, in consequence, necessitate the excision of both Fallopian tubes and ovaries. In the case of women over the age of 40 there was no great objection to this procedure, but he did not approve of such drastic measures in women of less than this age and in such cases conservative treatment was to be preferred. The usual method of douching was unsatisfactory unless skilfully carried out, and pelvic diathermy used after the manner of Dr. Robinson, was more satisfactory because the whole of the pelvic cavity could be subjected to heat. It was clear, however, that only certain cases were suitable for treatment of this kind. It was unlikely that pyosalpinx or hydrosalpinx would respond, and in such cases operation was necessary, but many cases were suitable for diathermy, and in these selected cases Dr. Robinson had obtained very satisfactory results.

One of the main difficulties in cases of pelvic infection was the elimination of the re-infection factor. This elimination was particularly difficult in cases of gonorrhœa, and attempts should be made so far as was possible to treat the ultimate cause.

He had also been much interested in the results obtained in cases of arthritis. It was easy to understand how in cases of cervicitis, diathermy was responsible, by clearing up the local infection, for the improvement, but diathermy had been used in some cases in which there was no infection of the cervix and yet improvement had followed. He was inclined to the view that in cases of arthritis the effects might be due in some degree to a stimulation of the ovaries.

Dr. AGNES SAVILL said that before diathermy was known, ionization had given excellent results. Before the war she had treated many cases of ordinary discharge, but only one of profuse discharge, in which the gonococci were abundant. Three treatments had cured the urethra and ducts. The cervix had been cured in seven treatments, copper ionization being given with a thick electrode. Pregnancy had occurred within eight months afterwards. There had been no relapse; the patient had been seen at intervals up to 1928.

Now that diathermy was so widely praised for the treatment of gonorrhœa, warning as to its correct use should be given. Difficulty lay in the fact that with some instruments the ammeter reading gave no true indication of what the patient was receiving. It was understood that pain during treatment indicated danger of tubal involvement. But, when an old high-frequency of the early type, with condensers instead of Leyden jars, and multiple spark gap, was used, pain was generally caused. Two severe cases had been cured with three applications of diathermy and seven to ten of copper ionization. They had been followed up and no relapse had occurred in six years. She (Dr. Savill) had tried a combination of diathermy and ionization on the same day for a few patients who could only come to town for occasional treatments; she had found this method inadvisable. With a new diathermy machine she found that half an ampere caused more pain than one ampere with the older instrument. In the first case treated with the new instrument, the cervical discharge had been cured, but tubal trouble had been lighted up. The lesson to be drawn from this failure was that it was wise to begin with the "pelvic warming," with the vaginal electrode, as advised by Dr. Cumberbatch and Dr. Robinson.

Section of Dermatology.

[*November* 15, 1928.]

CASES.

Keratosis Punctata.—H. C. SEMON, M.D.—Patient, female, aged 45, a masseuse, has had affection of palm of right hand for over fifteen years. This appears to have followed inunction of liniment (constituents unknown) for a brother's bronchitis. Palm always became sore and red after playing tennis. She took up massage during the war, but, being in charge of department was able to minimize actual manual work. For the last year she has noticed in addition to redness and roughness, the slow formation of groups of little horny plugs, which themselves seem to cause irritation. Removal of these with a needle gives some relief, but thickened ridges at points of flexion of fingers often give rise to cracks, which at the time are very disabling.

Clinical Appearances.—Lesions are limited to palm and flexor aspects of first joints of fingers and thumb of right hand. General diffuse thickening of skin in these areas, with chronic hyperæmia of passive type. Over the points of maximum flexion, the thickening is accentuated, and there is a tendency to the production of transverse fissures, which do not appear to penetrate lower than corneous layers. Scattered irregularly over affected region are small groups of pin-head sized pits, some of which, if viewed with a lens, can be seen to contain minute keratinous plugs. Skin of palm, as a whole, appears to be drier and more scaly than is normal.

A punctate form of keratosis may occur in such widely differing conditions as lichen planus, arsenical dermatitis, dyskeratosis follicularis vegetans (Darier), common warts, and psoriasis. Syphilis has also been accused in this connexion (Brocq).

In the case exhibited, in which the dystrophy has persisted and slowly increased over a period of fifteen years, without making its appearance in other areas, we are, I think, justified in excluding all the above diseases. As the patient is a masseuse I was at first inclined to suspect the local effect of some irritant used in her profession, but such a theory is discounted by the fact that one hand only is affected, and that the condition had arisen before she began her training.

The disease has been studied by Hallopeau and Claisse (*Bull. Soc. Franc. de Derm. et Syph.*, 1891, p. 116) who, on histological grounds, assign to it a nævoid origin. Galloway and Adamson described two cases in a husband and his wife (cousins), and demonstrated them with microscopic slides to this Section in 1918 (*Brit. Journ. Derm.*, 1918, xxx, p. 123). Their evidence goes to support that view still further. They were unable to find any pathological alteration "other than considerable thickening of the horny layer at the site of a warty lesion."

If the disease was the result of a toxic irritation of the sudoriparous glands or their ducts, as has been suggested by various authorities, there would, I think, be some evidence such as a round-celled infiltration in the region of the sweat ducts, and it would furthermore be exceedingly difficult to account for the limitation to the palm of one hand.

So far as I am aware, in all the recorded cases of this peculiar condition the lesions have been symmetrical, or on both palms and soles, and I venture to think, therefore, that if my diagnosis is accepted, this case is of some value as evidence against the toxic ætiology, since on that hypothesis the condition could scarcely have remained limited and localized over so long a period.

For the present, then, the most probable explanation is that of a nævoid origin, although it is not easy to understand the excessively delayed latent period—in this

case thirty years—during which there was no indication whatever of the impending dystrophy.

No biopsy has been attempted in view of the patient's occupation. Examination for yeasts and fungi, by Dr. W. Broughton-Alcock, gave negative results.

Discussion.—Dr. H. G. ADAMSON said that his attention was called to this disease in 1911 when preparing an article for Allbutt and Rolleston's "System of Medicine," with the late Dr. Pringle, on diseases of the sweat glands. A short paragraph on this disease referred to the earliest case described by Besnier, 1892, under the title "Kératodermie erythémateuse symétrique des extrémités; forme ponctuée. Kératose localisée à l'ostium sudorifère.

Illustrating Dr. Semon's case of keratosis punctata. This illustration should be carefully compared with that on p. 1063 of the "Textbook of Skin Diseases," by Professor MacLeod.

Paume de la main." (Baretta model, No. 560, St. Louis Hospital Museum.) Dr. Pringle had called the condition "punctate keratosis," and distinguished it from the porokeratosis of Mibelli and Respighi. In 1918 he (the speaker) had seen, with the late Sir James Galloway, the two cases mentioned by Dr. Semon. Those two cases had been diagnosed by comparison with an excellent coloured picture of a case by Buschke and Fischer in the "Iconographia Dermatalogica," of Neisser and Jacobi (1906). Both husband and wife, who were first cousins, had had the condition as long as they could remember. It was regarded as a congenital affection. Microscopical sections showed no inflammation, cell infiltration, acanthosis or parakeratosis; there was simply a horny thickening. The sweat

glands seemed not to be involved and the sweat ducts passed through the horny thickening unchanged. In 1918 ten of these cases had been published, most of them in France. Generally they had been described as cases of porokeratosis, but in sections made by Buschke and Fischer and by the speaker, the sweat glands and ducts seemed not to be involved, although, clinically, the lesions appeared to arise round a sweat duct. In making a diagnosis it was necessary to remember, as Dr. Semon had said, that other conditions produced a punctate keratosis, e.g., psoriasis, Darier's disease, lichen planus, arsenical hyperkeratosis and multiple warts. He (Dr. Adamson) thought that the condition in this case might be lichen planus, and some whitish patches in the mouth seemed to confirm this. In the cases recorded by Sir James Galloway the lesions were discrete, small, raised nodules, with a central depression, looking rather like small beads imbedded in the skin. In Dr. Semon's case the punctate lesions were not raised, and seemed to be set in an inflammatory base.

Dr. SEMON (in reply) said that Dr. Adamson's opinion was of pre-eminent value, because of his special experience of keratosis punctata. If the case were really an example of chronic lichen planus he felt encouraged to try the effect of small doses of X-rays, which, if an inflammatory cause were present might be effective. Did Dr. Adamson agree? [Dr. ADAMSON: Yes.]

Dr. ARTHUR WHITFIELD (Chairman) said he agreed that this case belonged to the lichen planus type, but his experience led him to think that it would not react well to X-rays. He had introduced a treatment some time ago, namely, rubbing thoroughly into the part an ointment consisting of camphor, 5j; crystalline phenol, 5j; blue ointment, 3ij; lanoline, ad 3j. The site should be covered with zinc strapping, and the application renewed daily. Under this treatment in most cases of hypertrophic lichen planus the condition disappeared within three weeks.

Unusual Lichen Planus.—H. W. BARBER, M.B.—This patient, male, aged 21, attended my out-patient department some weeks ago for a bald patch on the frontal region of the scalp. He was seen in my absence by one of my assistants, who diagnosed lupus erythematosus. I saw him shortly afterwards and found: (1) On the scalp an oval area of cicatricial atrophy, completely denuded of hair, and red in colour from capillary dilatation: on vitro-pressure the dilated vessels were not obliterated entirely. (2) On the buttocks and thighs grouped follicular lesions resembling lichen spinulosus. (3) In the perinæum and on the mucous membranes of the cheeks, typical lichen planus.

The case is of interest as showing the combination of cicatricial alopecia of the scalp with follicular keratoses on the trunk, such as has been described by Dr. Graham Little (*Brit. Journ. Derm.*, 1915, 183), Dr. Dore (*ibid.*, 295), and Dr. Wallace Beatty and Dr. Speares (*ibid.*, 331), and the association at the same time of typical lichen planus.

Dr. G. B. Dowling showed a child for me, some years ago, in whose case folliculitis decalvans of the scalp, with lichen spinulosus of the body, was followed by lichen planus of the ordinary variety.

It is difficult to avoid the conclusion that some cases, at least, of so-called folliculitis decalvans or pseudo-pelade are really cases of lichen planus, and that the "lichen spinulosus" described in association with it by several observers is merely the follicular variety of this disease.

Dr. H. MACCORMAC said he had seen several similar cases of lichen spinulosus in adults, and was inclined to think that they all represented a form of lichen planus, because in some there was coincident lichen planus, while in others lichen planus had developed later. He had also observed, in the accompanying eruption on the scalp, the atrophic condition mentioned by Dr. Barber.

Blastomycetic Mycosis.—H. G. WINTER, L.R.C.P., M.R.C.S. (Major, R.A.M.C.) (introduced by Dr. MACCORMAC).—Patient, aged 21, a soldier, employed in the ration store, now serving at Hounslow; came from the Rhine area with his regiment in March, 1928; has never been in the tropics. No history of association with animals.

He was admitted to the Military Hospital at Hounslow on October 9, 1928, with a diagnosis of tinea circinata. It is stated that he had a typical patch on the front of the neck, which was clearing, when an acute dermatitis developed.

When first I saw him (October 28, 1928) he had clinically a typical parasitic mycosis. In spite of repeated attempts, however, I was unable to demonstrate any fungus in the hairs or scales. On culture (on Dimond's special nucleo-protein serum agar medium) staphylococci and diphtheroids were obtained from the pus, but from the hairs a yeast, in almost pure culture. It was at first thought that the yeast was a monilia, but I am now rather inclined to the view that it is one of the blastomyces.

The only treatment I have given him to date has consisted of fomentations locally, and, on October 31, 1928, one intradermal injection of an autogenous vaccine prepared from the organisms obtained from the pus. The condition has considerably improved in the last week.

[Sub-culture of yeast and smear, from the culture, stained by Gram's method shown.]

I am inclined to think that the condition is due to the yeast infection and that the other organisms found are a secondary infection.

Discussion.—Dr. A. WHITFIELD (Chairman) said this was a rare and interesting condition. Sir Aldo Castellani had, within the last two years, shown cases of an infective condition of the scalp somewhat resembling this. Sir Aldo's term for it was cryptococcic dermatitis, and he (the speaker) thought the cryptococcus was a close relative of blastomyces. He believed that Sir Aldo would regard this present case as one of cryptococcus infection, though in all his cases the scalp only was involved.

Dr. H. G. ADAMSON said that he regarded this condition as ringworm of the beard. That it had begun as a tinea carcinata, and afterwards became nodular, was very suggestive, and it now had all the characters of a beard ringworm of animal origin. It was often difficult to find an infected hair in these cases, because, when the inflammation had been present for a time, most of the infected hairs were extruded.

Dr. H. C. SEMON agreed with Dr. Adamson's view. Dr. Barber might remember a patient of his (Dr. Semon's), an officer in the South African Police, in whose case, during the eleven months he was under treatment for a beard infection closely resembling the present one, repeated efforts by pathologists to grow a fungus had been unsuccessful. Dr. Gray had also seen the case and had agreed with the diagnosis of ringworm infection, which was further supported by the prevalence of that disease among the horses at the depôt.

Dr. H. MACCORMAC said he had previously seen this case with Major Winter, when the question of ringworm was considered. Direct examination of the hairs had failed to reveal any ringworm fungus, and, although this fact did not exclude ringworm, the positive evidence of the culture, and the failure to grow ringworm directly from the affected hairs, seemed to him to favour Major Winter's view. He did not think the case mentioned by Dr. Semon could be taken into consideration, because the causal parasite had not been identified.

Dr. A. WHITFIELD (Chairman) said that his experience of ringworm of the beard was that, when one could not get the fungus from the hair of the beard, one could usually do so from the pus. If one obtained a considerable amount of pus, and made a dozen slides, some with potash, some stained, the fungus could almost always be detected though at times the search might be a long one, and in the later stages of the condition success might be impossible. If the disease in this case were due to an ectothrix fungus one might be able to carry out a skin test with trichophytin.

Lymphangioma Circumscriptum.—J. H. T. DAVIES, M.B.—Patient, an infant, aged 6 months, was brought to me three months ago on account of cysts in the scalp which had been present since birth. He is a healthy baby of normal size and weight. Along a line encircling the cranium in a more or less horizontal plane, and about 3 in. above the auditory meatus, in its posterior half there is a

Lymphangioma circumscriptum.

group of cysts varying in size from 1 in. in diameter downwards. These are translucent, but not fragile, and can be seen to communicate with deeper structure by holes in the bottom. They are sometimes tense and sometimes flaccid, their condition not apparently depending on whether the child is crying or not. The hair around them is about 2 in. long while the rest of the scalp has only a short lanugo covering.

Chronic Circinate Eruption.—J. H. T. DAVIES, M.B.—This patient is the mother of the child described above, and, apart from this, I could ascertain nothing significant in the family or personal history.

Two years ago she had measles, and shortly afterwards she noticed a spot on the left wrist, and later one on the right, both of which have spread over the anterior surface of the wrists, clearing in the centre without scarring.

On the anterior surface of each wrist there is a lesion of polycyclic outline, roughly 3 in. in diameter. In the middle of the one on the left there is another similar annular lesion 1 in. in diameter. The lesion consists of a more or less raised continuous ridge $\frac{1}{8}$ in. wide, dark red in colour and brownish on vitro-pressure, covered in places with a thin scale. It gives rise to no symptoms. Histologically, there is slight epidermal hypertrophy: moderate vascular dilatation, œdema, and a diffuse infiltration of cells with collapsed nuclei, in the papillary layer of the cutis. There is scarcely any perivascular infiltration in the deeper layers, and no plasma cells were seen.

Discussion.—Dr. H. W. BARBER said he thought the mother's lesion was a syphilide, and he had been wondering whether one could connect the child's condition with that disease.

Dr. A. C. ROXBURGH said he thought the lesions on the mother's wrist were most likely to be annular lichen planus. He did not think a syphilide would have lasted for two years without becoming more extensive, or else disappearing altogether. Also, the eruption was symmetrical. If it was a syphilide it was a late one, and a late syphilide was not likely to

be symmetrical. As regards the section, the granular layer was about four cells thick, and there was a fairly dense round-cell infiltration, pretty well limited to the papillary layer, appearances which he thought were consistent with a diagnosis of lichen planus.

Dr. H. G. ADAMSON considered the eruption in the mother a syphilide, a "recurrent circinate syphilide." He could not diagnose the condition in the child.

Postscript.—Wassermann reaction, positive.—J. H. T. D.

Sarcoid of the Hand resembling Granuloma Annulare.—H. MACCORMAC, C.B.E., M.D.—Patient, a girl, aged 10. The condition first manifested itself in February, 1927, as a small nodule over the knuckle region of the right index finger: this gradually extended to assume a form bearing a resemblance to an imperfectly developed granuloma annulare. Recently two other nodules have appeared over the knuckle of the adjoining finger. There are also flat patches of a similar nature above the heels, one of which appeared at the time the first lesion developed on the hand, the other being of more recent origin.

This case is exhibited because it appears to belong to an intermediate type which forms an important link in a chain connecting an accepted form of cutaneous sarcoid with granuloma annulare. In 1925 I showed a similar case, in which the condition had begun in the same way, but had finally assumed the classical features of granuloma annulare. This patient also presented a sarcoid on the left ear.

Lichen Planus.—E. G. GRAHAM LITTLE, M.D. (shown by Dr. MACCORMAC).—The unusual feature in this case is the association of lichen planus of linear type with ordinary lichen planus. The eruption has been present three months, and the linear stripe, extending from the buttock down the leg, appeared at the same time as the lichen planus on the body. The presence and simultaneous development of the two forms of eruption are, Dr. Graham Little considers, very uncommon.

Section of Otology.

President—Mr. Somerville Hastings, M.S.

[*November* 2, 1928.]

PRESIDENT'S ADDRESS.

The Estimation of Hearing Capacity.

By Somerville Hastings, M.S.

If we review the progress made in otology during the last twenty-five or thirty years, we cannot be altogether satisfied with the results achieved. We have learned how to *prevent* some of the affections of the middle ear with their complications, mastoid disease, meningitis, septic thrombosis of the lateral sinus and intracranial abscess by treating foci of infection in the nose and throat, and we have improved our technique in dealing with these complications. We are able also, in some cases, to arrest the progress of the deafness resulting from the same causes by early treatment of the nose and throat, but we have to admit frankly that a great deal of the middle-ear deafness that we are called upon to deal with, by whatever name we may call it, is little influenced by any treatment, and that we are not even yet agreed as to its exact pathology. And what is true of middle-ear deafness is true with even greater force when the trouble has its origin in the internal ear or nerves of hearing. Cases of severe or complete internal ear deafness, often apparently of sudden onset, not infrequently come before me, but too often I can offer no suggestion as to their probable cause, nor advise any treatment giving the slightest hope of alleviation or cure.

Now, I cannot help feeling that our comparative ignorance concerning both the pathology and treatment of so many cases of deafness, of both the middle and internal ear types, is due, at any rate in part, to the methods of examination which we have used in the past. In the first place I think it must be admitted that the methods of estimating hearing capacity by the distance at which a whisper, the conversational voice, a watch or Politzer's acoumeter can be heard, though of value clinically, are quite useless for scientific purposes. We should at least have a relatively pure source of sound, such as that produced by a tuning fork or a wireless valve. In the audiometer, shown to the Members of this Section by W. S. Tucker and myself [1], the source of sound is a wireless valve, but the estimation of hearing is by means of purely arbitrary units. Harvey Fletcher [2] estimates hearing capacity in what he calls sensation units, a sensation unit being the minimum change in the intensity of a sound perceptible to the normal ear, but this does not carry us a great deal further. What I want to know before I can form a correct mental picture of a given case of deafness, is for every pure note between the upper and lower limits of audition, the percentage of normal hearing that remains. Then if we can also estimate in percentages the degree to which the cochlear nerves and cerebral centres are functioning, we shall be able to say exactly to what extent the deafness is due to changes in the conducting apparatus, and to what extent to changes in the perceptive. Supposing, for example, we find that a patient has 25 per cent. or quarter normal hearing when tested by air conduction, and say 50 per cent. or half normal hearing when his perceptive function is tested, then we can say that he has only $\frac{1}{2}$ 50 per cent. of normal hearing by conduction, or in other words that, owing to impairment of the conduction of sound waves, only 50 per cent. of the waves that pass through the external and middle ear of a normal individual and reach the cochlea are able to pass through his.

I am convinced, therefore, that for a scientific study of deafness, we must give up arbitrary units of hearing and try to discover what percentage of the normal power of conduction and what percentage of the normal power of perception are present for pure sounds of the principal wave lengths. That to my mind is the problem that we have to solve, but it is not an easy one.

Perhaps it will be convenient to consider first the principles involved in the estimation of the perceptive function. Now it must be evident to all that the ordinary method of estimating bone conduction by means of a vibrating tuning-fork placed upon the mastoid is open to a thousand fallacies, since the more complete the interference with conduction through changes in the external or middle ear, the longer will the sound of the tuning-fork be heard. In November, 1917, a Committee of this Section [3], formed to organize otological research, issued its report. Its most useful recommendations were those concerning hearing tests, but, coming at a time of great national stress, the report did not receive the attention it deserved. One of the tests therein recommended was that described as *absolute bone conduction*. I do not know who is responsible for this test, but it certainly is not Bing's, as suggested in this report. To carry out the test the observer (who presumably has normal hearing) and the patient each blocks his external auditory meatus with his finger (preferably by pressure on the tragus). A vibrating tuning fork is then alternately pressed on to the mastoids of the patient and the observer until it is no longer heard. In this way the bone conduction of the two persons is compared. Ever since the publication of this report, I have made use of this test, and I think I am right in saying that I have practised it in every case of deafness that I have at all carefully investigated, but up to the present I have never yet found *absolute bone conduction* increased above the normal in any single case. The often repeated statement that in otosclerosis bone conduction is increased, is, therefore, when applied to absolute bone conduction, a misstatement. If, however, the comparison between the bone conduction of the patient and observer, with the meatus unblocked, is referred to, then admittedly in what is called otosclerosis, bone conduction is certainly increased. But so it is in all other forms of conductive deafness, be they caused by whatsoever injury or disease of the external or middle ears. Indeed the increase of bone conduction as thus tested in the most advanced case of otosclerosis that it is possible to conceive, cannot be greater than that present in a normal ear when thoroughly obstructed by a plug of wax.

If a person who, through injury or disease, has lost all perception for sound of any character in, say the right ear, be tested for absolute bone conduction, it will be found that he hears the fork for only a second or so longer on the left mastoid than on the right and, indeed, that the perception in the only functioning ear is approximately the same on whatever part of the vault of the skull the tuning fork is placed. This would appear to indicate that in the conduction of sound from a vibrating fork, the skull and its contents do not behave in the same way as the air, but that in all probability the cranium vibrates as a whole. If a person with normal hearing is made to close both meatuses with the fingers by a pressure on each tragus, and if a vibrating fork is then placed on different parts of his skull, it will be found that he sometimes states that he hears the tuning-fork better in one ear and sometimes in the other, and further, that the longer the tests are carried out the more inconstant the results become. As a matter of fact except when marked differences in hearing exist, it is exceedingly difficult to say in which ear a sound is best heard. I suggest that the reason is probably to be sought in the fact that in the ordinary way we make use of the different intensity of the perception of sound in the two ears solely for determining the direction of the sources of a given sound, and in determining this we usually turn our heads from side to side until the sound is heard equally in each ear. The perception therefore, of a difference of intensity of sound in the two ears is in the ordinary way only used to determine the direction of a source of sound,

and being never used *per se*, is only with very great difficulty estimated by the brain.

I have dealt with the relative intensity of the perception of sound in the two ears at some length, because it is an important consideration in assessing the accuracy of the results of the *absolute bone conduction* test. It will be realized that obstructing the meatus increases the perception by bone conduction very considerably. Therefore, if the degree of perceptive hearing (bone conduction) in the two ears is approximately equal however great the degree of conductive deafness in the ear not being examined, the test will not be vitiated by it. It is only where there is marked perceptive deafness on the side being tested, with, on the other side, good cochlear hearing and considerable conductive deafness—an unusual combination—that difficulties will arise. In such a case the patient will usually say at once that he is hearing the tuning fork in the opposite ear, and in this case it will be easy to exclude the ear not being tested, either by means of a noise machine (Bárány's box), or by rapid movement of the finger in the meatus.

Lowndes Yates [4], T. A. Clarke, and others, have shown that it is possible to estimate the total hearing (perceptive and conductive) power of a patient for C2 and other tones as follows: A tuning fork is held at any constant distance from the patient's ear. When he ceases to hear it, it is transferred to a similar distance from the ear of an observer who has normal hearing, and the time noted until it becomes inaudible. This gives in seconds the deficiency of the patient for the note in question. By means of a table or graph this can be converted into percentage retention of normal hearing distance, and seeing that the intensity of sound varies inversely as the square of the distance from its source, the square root of this figure gives the percentage of total hearing power present.

By means of the *absolute bone conduction* (A.B.C.) test above described, it is possible to estimate the percentage of normal perceptive (cochlear) hearing retained by a patient. A vibrating tuning fork is held on the patient's mastoid with the meatus closed, until perception of sound ceases. It is then transferred to a normal ear and the time noted until the perception of sound by air conduction ceases. By means of the table referred to above, this is translated into percentage retention of normal hearing distance. If not already known, a similar observation is made with regard to the absolute bone conduction of a person with normal hearing; then

$$\frac{\text{Percentage retention of normal hearing distance for patient's A.B.C.}}{\text{Do. do. do. for normal A.B.C.}} \times 100$$

gives us the percentage of perceptive hearing present. Knowing then the percentage of total hearing present in a given case and also the percentage of perceptive (cochlear) hearing it is easy to determine to what extent, if any, the deafness is due to changes in the conduction (external and middle ear) apparatus.

It may be objected that such methods as these are laborious and crude, and, moreover, uncertain, because the personal equation of two individuals is involved. I entirely agree, but up to the present I know of no other method that will tell me what I want to know. I am sure, however, that neither our investigations regarding otosclerosis, nor any similar researches, will really prosper until we have standardized our methods of testing ears and estimating hearing capacity. The last attempt made by this Section was by a Committee set up in 1917, at a period when few were able to give much time or attention to the subject. Is it not time that we, as a Section, made another attempt to solve this important problem?

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A Classification of Deafness Based on the Effect of Deafness on Efficiency in Life.

By J. Kerr Love, M.D.

Abstract.—The deaf classified from the point of view of incapacity as a result of deafness: (*a*) The true deaf-mute; developmental.—(*b*) The muted or semi-muted, totally deaf because of disease occurring shortly after birth.—(*c*) The speaking deaf, totally deaf from disease occurring after two years of age.—(*d*) Partially deaf, hard of hearing.

Hearing tests useless except in (*d*).—Treatment useless except in (*d*).

Syphilis: its association with congenital deafness; percentage due to this cause, in children.

Incapacity due to deafness.—Aids to hearing.—Prevention of deafness.

Deafness being only a symptom, a classification may begin from several points, e.g., its cause within the auditory apparatus, its degree, its effects on the deaf person, etc. Any classification starting from one point or base will cut across or overlap any other starting from any other point. I have chosen the incapacity or inefficiency resulting to the deaf person as my starting point, and although I may have to refer to diseased conditions and hearing tests incidentally, I ask you to keep in view, as I shall try to do, this base, or starting point.

By way of contrast let me ask you to look (*a*) at a hearing child aged 5 years, (*b*) a child born deaf of the same age. The former with his 300 or 400 words is full of life, chatter and response; you get on with him, talk to him and play with him to your mutual delight. Then take the deaf child of the same age. In spite of a public school and a university education, even if you know every language spoken under the sun, you are helpless before him. Unless you are a teacher of the deaf, you cannot even begin to make yourself understood.

It takes ten years of special education to fit the 5-year-old deaf child for any work in life, and at the end of that period his choice of a job or occupation is a much restricted one. Hardly any cause of inefficiency is more complete than that. In this classification these children have been put down under the heading A, "congenital defect in development," and this is roughly correct. They are nearly all cases of Mendelian deafness, though a few due to hereditary syphilis have to be included. It is not strictly true that these children have no hearing at all; they almost always have some island of hearing in the surrounding sea of deafness. The islands are nearly submerged at the best and not often placed in the speech area of the scale. These children are the true deaf-mutes, the true deaf and dumb, and have to be educated by specially trained teachers in special schools for the deaf.

No hearing tests, except very loud noises such as a bell struck near the ear or the shouted voice, are of any use in testing these children. But by these means you may demonstrate the presence of hearing, and even the hearing of vowels in a fair number of cases.

The second class (B) is described as "muted or semi-muted." The terminology here is not fixed—perhaps not quite satisfactory—and I will be glad to have suggestions and criticism. The result in the individual does not differ much from that in Class A—I mean the result on capacity or efficiency. They have a slight advantage over the deaf-born if the onset of the deafness occurs after the second year, because speech has operated and ideas about people and things have been formed before the total silence came down. Probably such ideas are never quite lost. But speech is nearly lost in the absence of sound, the stimulus on which its acquisition so largely depends, and for educational purposes these children have to go, with the deaf-born, to special schools for the deaf. The deafness in Class B is as a rule total—a good example of its cause is cerebro-spinal fever although, again, hereditary syphilis expressing itself in infant life rather than before birth, is not a very uncommon

cause. As a rule no hearing islands exist. Class C, the speaking deaf, differs from B in that hearing has been lost after the speech habit has been well established, say up to 5 or 6 years. Deafness, even if total, coming at 5 years, does not destroy speech. Speech becomes careless and slurred, especially in the consonants, but a good articulation teacher may, by training, resurrect the lost elements and the forgotten words. The causes of deafness in Class C are the same as in B, but occur later in child life and the deafness is usually total. Though there is no hearing, the remains of speech are a great asset to the child, whose ultimate speech is much better than that of the deaf-born child.

In all these classes, so far as I am aware, efforts to cure deafness or to develop hearing are useless. The basis of teaching is the substitution of one sense for another—the eye for the ear—similar to the substitution of touch for sight in the case of the blind. These children are not the victims of the electrical lie which is told to the hard-of-hearing adult. Class D, consisting of the partially deaf, is in many ways the most interesting of all. It is by far the largest class and, as the figures indicate, we come into the range of hearing tests. In Classes A, B, and C, the deafness was for practical purposes complete; hearing tests are valueless. Except for ranges of tuning forks, etc., for the mapping of hearing islands in a scientific inquiry the teacher does not call on the aurist for guidance. He will find out any useful hearing such as vowel hearing, during the early weeks of tuition. But in Class D, not only is hearing present, but it may be improved, and the diseased process on which the deafness depends may be checked. I am discussing classification, and only incidentally treatment and hearing tests, and yet it is a hearing test which enables us to classify these children at all, viz., whispered speech. Notice the figures at the extremes of the line, 20 and 6. We know that whispered speech may be heard by a healthy ear at far more than 20 ft. I do not think that any standard can be fixed without first of all getting a standard of quietness. The adoption here of 20 ft. as the normal standard is indicated by the fact that educational efficiency, and later on vocational efficiency, is not interfered with in a child or adult who can hear whispered speech at 20 ft. A child with this degree of hearing should not be in a school for the deaf, and an adult with this degree is not likely to lose his job. The capacity or efficiency is not threatened. At the other end of the line of figures, 6 and under, you have a degree of deafness which menaces both the child and the adult, and which I have tried to state in the 6 ft. rule. Thus, if a child does not hear whispered speech at least with one ear at 6 ft., he cannot profitably stay in the ordinary class of an elementary school, and in the case of an adult he is apt to lose his job. Do not take to these figures too rigidly. Bright children and bright adults, especially if they are good lip readers, appear to have better hearing then they actually possess, and good lip reading is often acquired by deaf people without any tuition at all. The 6 ft. rule is helpful in dealing with the average child or adult.

In adapting this classification to adult life addition, but no alteration, needs to be made. Class D must be made to include the cases of deafness which go by the names of chronic progressive deafness, chronic aural catarrh, etc., and which give us so much of the incapacity and inefficiency of adult life. In Glasgow we have a large unit of these cases in the "League for the Hard of Hearing," and it is significant that 75 per cent. of these are women. Many are cases of otosclerosis.

Just a word about syphilis as a cause of congenital deafness. That it is so I have no doubt, but that it is commonly so I have a good deal of doubt. In sporadic congenital deafness we have a sub-class which may contain some cases but, in my opinion, not more than 2 or 3 per cent. It can hardly occur to a greater extent than congenital syphilis as shown by the Wassermann reaction applied to children generally. The following figures from my friend Professor Leonard Findlay, of the University of Glasgow, is the nearest guide I can give you. "In the children's

hospital we found that only 2·6 per cent. of the patients gave a positive Wassermann reaction, and of the children suffering from scarlet fever in Ruchill only 0·8 per cent. gave a positive Wassermann reaction. The scarlet fever patients in Ruchill were chosen as it was considered they gave a better sample of the whole population. The other children were considerably younger, mainly under 3 years of age, while the children in Ruchill were chiefly over 6 years. In any case these figures show how rare congenital syphilis must be in the total child population of Glasgow. It is the rarest manifestation for us to find deafness as the clamant symptom. I can only recall one or two cases."

With the exception of the children under Class D, treatment is of no avail. The inefficiency is permanent. In spite of lip reading and aids to hearing, the sufferer is always handicapped. It is in the adults of this class that aids are often valuable. I cannot here say more on this subject than that it is in cases of middle-ear deafness that these electrical aids often succeed, but the success is not uniform and disappointment is common.

Any classification of the deaf worth talking about must have more than an academic value. This is not the place to discuss its value in the school room. But four practical measures are indicated:—

(1) An intensive campaign against diseases of the ear in children.

(2) An intensive study of the otosclerosis with a view to its prevention.

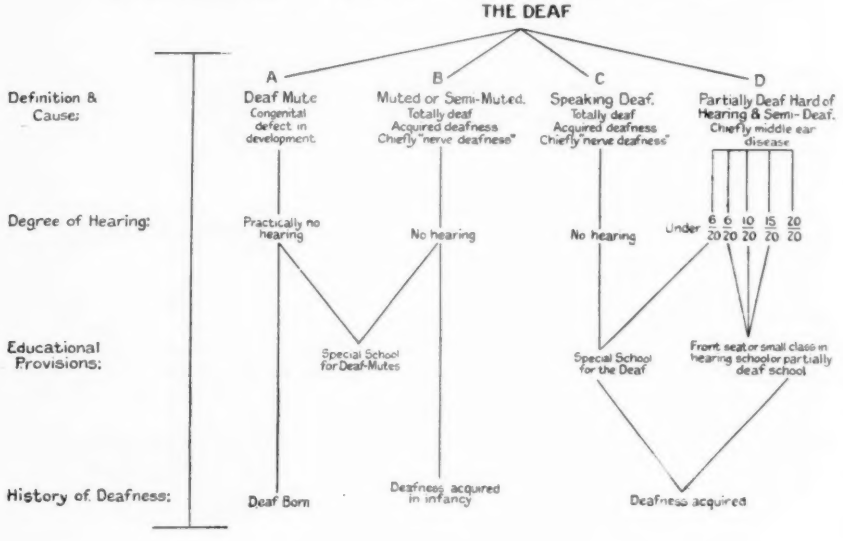
(3) The compulsory notification of syphilis or at least of congenital syphilis. This is the only sure way of reaching the whole family.

(4) The training of the public in the causation of congenital deafness. The congenitally deaf should not intermarry. The relatives of the congenitally deaf should not marry into similar families.

To the first two of these measures this Section is not only committed, but, I am glad to say, has taken a leading part in their initiation. And I am sure it will not rest till the problem raised in the last two has been solved—the prevention of congenital deafness.

EDUCATIONAL CLASSIFICATION OF THE DEAF BASED ON THE DEGREE AND HISTORY OR CAUSE.

For educational purposes, whispered speech at 20 feet is taken as ***normal****.*



On Hearing Tests.[1]

By T. A. Clarke, M.B.

Abstract.—Necessity for consideration of present hearing tests.—Testing of tonal limits considered; present tests accepted as satisfactory. Anomalous results of comparison of perception of the monochord by air and bone conduction.—The Rinne, Schwabach, Weber and Bing tests destructively criticized.—The Gellé and galvanic cochlear tests; Fraser's modification of Gellé's test.—Suggestions for the improvement of testing.—The "absolute bone conduction" test affords an absolute index of the perceptive component of hearing (nerve function).—Mechanism of transmission of sound waves through the skull (bone "conduction").—Quantitative determination of hearing power; results expressed on a distance basis are to be preferred; use of tuning forks in obtaining such results.—The mathematics underlying this use of tuning forks.—Accurate quantitative estimation of nerve function (the perceptive component of hearing).

Few specialities can have less reason than the otological to be satisfied with their functional tests, and I propose therefore to review the present methods, to emphasize their shortcomings, and to describe means of placing auditory tests on a rational, accurate and convenient basis.

Qualitative Tests in Present Use.

Tests concerned with Limitation of Tonal Range.—While curtailment of tonal range is a quantitative rather than a qualitative defect, it is best considered with the latter tests. There is never justification for a declaration, based on a functional test, that a patient has no hearing for any particular note, for there is always the possibility that with greater intensity of sound some perception would have been manifested. The term "limit of tonal range" is, however, useful in practice, but the instrument employed in its determination should be mentioned, as affording evidence of the intensity of vibration.

The lower limit is usually tested with tuning-forks, and with these 16 d.vs. per second is the figure most generally accepted for the normal lower limit. The testing of patients with low forks requires care; the vibrating prongs must not touch the hair or auricle, and overtones must be guarded against. Suitable construction of the fork, with adequate clamping, and striking the fork in the middle of its length against a soft object (e.g., the hypothenar eminence of the hand) minimize the production of overtones. If anomalous results are obtained, the patient should be asked to describe what he hears; only a small amount of intelligence is required to differentiate the low-pitched fundamental from the overtones.

The upper limit of audition has been very variously estimated, frequencies of from 15,000 to 41,000 d.vs. per second having been suggested. As with the lower limit, a precise figure is of small value unless the approximate intensity of vibration, evidenced by the instrument employed, is known. For testing the upper limit, audiometers, Koenig's cylinders, Galton whistles and the monochord may be employed.

For general purposes in testing tonal range limits, audiometers are not of great value. That described by Tucker [1] in 1922 conveniently exceeds the "normal" upper limit, while a further example with a high upper limit has recently been demonstrated by Popper [2]. Koenig cylinders [3] provide notes of constant pitch but with variable intensity. They are inconvenient and not of general utility. Galton whistles were introduced by Burkhardt-Merrian in 1878 [4]. Modifications have since been made, notably by Edelmann and Schafer. Using this last modification I have found the normal upper limit to be about 19,000 d.vs. per second.

[1] From the Ferens Institute of Otolaryngology.

The frequency is determined from the scale reading and a table supplied by the manufacturers. The formula which is the basis for such determination is $n = \frac{v}{4L}$, where n is the frequency, v the velocity of sound (in air 331 metres per second), and L is the length of the tube. Temperature affects the result; thus at 0° C. a tube length of 5 mm. corresponds to a frequency of 16,550 d.vs., but at 25° C. to one of 17,300. This is in accordance with the formula $V_t = V_0 (1 + \cdot 00183t)$.

The monochord is, I believe, the most convenient and accurate agent for testing the upper limit. While it may be made, by plucking the wire, to yield transverse vibrations, these are of a relatively low frequency which varies not only with the length but also with the mass and tension of the wire. The value of the monochord lies in the fact that, when rubbed with a spirit-moistened applicator, the wire is set into longitudinal vibration, and the pitch of the resultant sound depends only upon the length of the vibrating wire. The frequency is determined by the formula $v = 2Ln$ (where v is the velocity of sound in the wire, in the steel monochordal wire this is about 499,000 cm. per second; L is the length of the vibrating wire; and n is the frequency, in double vibrations per second). The length of the vibrating wire is given by the centimetric reading on the monochord. One may say then that

$$n = \frac{v}{2L} \text{ or } n = \frac{499{,}000}{2 \times \text{centimetric reading}}.$$

Though the frequency is independent of the mass and tension of the wire, these are not without influence on the result, since they affect the intensity of the resulting sound; decreased intensity may, of course, spuriously suggest a lowering of the auditory upper limit. It would be convenient to have the frequencies engraved on the monochord in place of the usual centimetres.

Few patients have difficulty in determining the upper limit by the monochord, the change from a squeak to a rub being easily recognized. I have found the normal limit by the monochord in young adults to be about 20,500 d. vs. per second, variations occurring even in the individual. The limit when tested with the whistle is rather lower than this, a finding which might be due to the whistle giving less intensity of sound at these high pitches, or perhaps to the masking effect of its associated blowing sound.

The monochord has the advantage that its sound may be transmitted by bone conduction. In so using it I found one thing for which I was not prepared, that in every case, even of perceptive deafness, the upper limit is lower by air than by bone conduction. In cases of obstructive deafness such a finding is not unexpected, but its occurrence, often to a marked degree, in cases of perceptive deafness is remarkable. A paper just published by Sonnenschein [5] records the same finding in health; he quotes Kalaehne for an explanation. It is there suggested that in using tuning forks one is comparing the incomparable; that in testing air conduction one is utilizing the transverse vibrations of the prongs, which are "proper" to the fork, and that one compares this with bone conduction excited by the incidental and weak longitudinal vibrations in the stem of the fork. With the monochord, equal longitudinal vibrations supply the stimulus for both air and bone conduction. This is, I think, the probable explanation.

Tests dependent upon Perception by Air and Bone Conduction and their Relative Proportions.—These tests form the backbone of our functional diagnosis, and I believe that the lack of otological progress in the last thirty years is largely due to their inherent inaccuracy.

Under this heading fall Rinne's test, Schwabach's test, Weber's test and Bing's test, all being closely related. *Rinne's test.*—If duration of perception of the sound of a tuning fork placed on the mastoid is greater than when the prongs are held close to the tragus, Rinne is said to be negative; if this is reversed, as normally, Rinne is

said to be positive. *Schwabach's test.*—The base of a tuning fork is placed on the patient's mastoid; when perception ceases it is transferred to the observer's mastoid. If the observer hears the fork, Schwabach is minus, and may be expressed as so many seconds deficiency. If, on the other hand, by reversing the order of the test, it is found that the patient's perception by bone conduction exceeds that of the normal person, Schwabach is plus so many seconds. *Weber's test.*—The base of a tuning fork is placed on the middle line of the skull; if perception is stronger (or longer) in one ear than the other, Weber is said to be lateralized. *Bing's test.*—The base of a tuning fork is placed on the mastoid; when hearing ceases the meatus is occluded; if the fork is again heard Bing is said to be positive.

Rinne suggested, as a rule for tuning forks, that normally, perception by air conduction is twice as long as by bone conduction. Such a statement, of course, does not stand the shortest scrutiny. In 1855 he described his test [6] which depended upon inversion of this air-conduction bone-conduction ratio and which differentiated conductive from perceptive deafness. It was soon recognized that the test was not free from fallacy, and Bezold [7] suggested that its value was limited to cases with more than a certain defined grade of deafness. This only implied recognition of the fact that the point of change from a positive to a negative Rinne is merely an arbitrary point on a sliding scale. Attempts have been made to assess the full number of possible results of the test, as many as eight having been suggested by some authors [8] [9].

The pitch of the tuning fork plays a great part in determining the result of the test, the lower the pitch the less severe the lesion required for a negative result. This is due to the fact that, in conditions giving rise to a negative Rinne, perception by air conduction for the lower notes is more affected than that for the higher notes.

Schwabach's test is based upon one of the factors of Rinne's test—the lengthening of perception by bone conduction in conductive deafness. It was promulgated by Schwabach in 1891 [10]

Weber's test rests upon an observation made by Weber in 1834 [11]. He noted that obstruction of the auditory meatus caused lateralization of perception of a tuning-fork, placed mesially on the skull, to the ear in question. This observation was then extended to cases of conductive disease, and the test established for cases of unilateral, or unequal bilateral disease. Attempts have been made (by G. W. McKenzie [12]) to assess the results of the test quantitatively by recording the distance from the middle line to which the fork can be moved without changing the lateralization of perception. While possibly attractive theoretically, such a suggestion can only be based upon lack of practical knowledge. In a normal person a tuning fork, placed upon one frontal or parietal eminence, is heard more strongly in the opposite ear; observations on such apparently anomalous perceptions of tuning forks placed in various positions on the skull have appeared in a paper by Frey [13]. These facts stultify any such mode of estimation of the test.

Bing's test [14] is closely associated with the preceding ones; it is only an expression of Weber's observation, since it relies upon the increased perception by bone conduction which occurs on closing the meatus in all cases in which such increase is not already present and maximal as a result of conduction disease.

The Schwabach, Weber and Bing tests agree with the Rinne test, in that, in cases of conductive disease, the lower the fork used the more readily is an appropriate response elicited.

These four tests fail to justify their prominent place in the routine testing of patients, because they are an unnecessarily complicated method of obtaining incomplete and frequently misleading information.

The Rinne test may give positive or negative results. Surely we are entitled to expect that the results of so elementary a test should establish unequivocal

conclusions; yet we have to consider full positive results, reduced positive results, positive-negative results, reduced negative results and full negative results, at the end of which we can really be said to have a very incomplete knowledge of the auditory pathology.

The Schwabach test may sometimes provide information, though it is always incomplete and sometimes misleading; it cannot serve as anything more than a conjecture as to the aural condition.

Bing's test may provide information as to the presence or absence of gross conductive disease; its value ends there.

Weber's test is an attempt at a short cut, inasmuch as it would provide information by the comparison of two unknowns (the two ears); as such, it fails lamentably. While in unilateral disease of pure conductive type it may yield information, in all other cases the results are indefinite or unreliable.

Complementary Tests.—Before leaving the subject of tests in present use, one other test requires consideration, being attractive in theory and disappointing in results—Gellé's test.

Gellé's test was put forward in 1881 [15], and has since met with much theoretical and practical criticism. Gellé supposed that the increased pressure in the external auditory meatus produced in performing the test caused inward movement of the drumhead and ossicular chain, with consequent raising of labyrinthine pressure by incursion of the stapedial footplate; the rise of labyrinthine pressure resulted in diminished sensibility of the cochlear organ, manifested by decreased perception of a tuning fork by bone conduction. Stapedial fixation would prevent such a sequence and there would be an absence of the normal response.

In an attempt to assess the value of the test I have carried it out in a number of cases. While deserving fuller consideration, the following figures, from the last eighty cases examined, may be interesting as affording information as to its occurrence and ground for criticism of the theory of its production.

TABLE I.—GELLÉ TEST.

160 ears examined:—
(A) 20 per cent. otosclerosis of the Bezold triad type.
(B) 29 per cent. with perforated drums.
(C) 51 per cent. with whole drums (this group includes normal ears, C.C.O.M., C.S.O.M. residua, and cases with perceptive deafness, but excludes the otosclerotic group).

	GELLÉ RESULT		
	Positive	Negative	Reversed[1]
All ears examined ...	79 per cent. ...	15 per cent. ...	6 per cent.
(A) Otosclerosis	66 ,, ...	15·5 ,, ...	18·5 ,,
(B) Perforated drums ...	78·5 ,, ..	21·5 ,, ...	0 ,,
(C) Whole drums	83 ,, ...	11 ,, ...	6 ,,

The grouping of cases and the results, expressed in Table I, in addition to calling attention to the existence of a reversed Gellé, lead to the following suggestions. Firstly, that otosclerosis has little or no influence on the result. Secondly, that the test is unsound in theory, for the test is as frequently positive when the drum is perforated as when the drum is whole; if the drum is perforated there can be no mass inward movement of the ossicles, and it is probable that the increased pressure is transmitted to the labyrinth at least partly by the round window; indeed one would expect more by the round window than the oval window, for one knows that the membrane of the former is normally considerably more lax than that of the latter. Thirdly, one would suggest that the significance of the Gellé phenomenon is still unknown.

Gellé's test consists essentially, as stated, in the variation of perception of a bone transmitted sound, and one might criticize the attachment of Gellé's name to a test

[1] By reversed Gellé I mean increased loudness of the sound occurring during increase of meatal pressure.

described in a recently issued pamphlet. In this test a nozzle attached by rubber tubing to a valveless Politzer bag is inserted into the meatus. A sounding tuning fork is placed on the bag, and pushed in to cause compression, variations in the subject's perception being noted. In this test the sound is transmitted by air conduction, and a factor entirely different from any in the true Gellé test is introduced, that is, variation in the tension of the ossicular chain while conveying the sound waves. This new test may possess considerable value, but to call it Gellé's test can only lead to confusion.

Suggested Improvement in Qualitative Testing.

Since auditory ability may be considered as being the total hearing which is the result of the coöperation of conductive and perceptive components, it is obvious that by determining the condition of any two of these we can deduce the condition of the third. The Rinne and associated tests fail in their purpose of locating an auditory defect because their results are influenced simultaneously by changes in both components of the auditory function, conductive and preceptive. The total hearing or auditory ability (that is to say the hearing as normally considered, by air conduction) is of course readily determined; its precise estimation will be considered in the third section of this paper. The primary determination of conductive function is obviously difficult, either by subjective or objective methods, though Bing's test may sometimes provide an approximate indication.

The perceptive component is readily determined and may be accurately assessed by a test which has been long described but which has never secured the universal appreciation it deserves—that is, by the comparison of the subject's absolute bone conduction (i.e., perception of a tuning fork placed on the mastoid, the meatus being closed by the finger) with a normal person's absolute bone conduction.

This test was described as long ago as 1883 by Pomeroy [19], and Hall Pyke [20] describing some cases recently stressed its value; Mr. Somerville Hastings has long used it as a routine.

If we were to accept the various theories involving the participation of the tympanum, drum-head and ossicular chain (Bezold [21], Gray [22], Kerrison [23]) in auditory perception by bone conduction, we might doubt the test as an index of purely perceptive function. But the test supplies the chief contradiction to the theories, for almost complete destruction of the drum, disruption of the ossicular chain or the occupation of the tympanum by fluid or organized contents causes no appreciable difference to perception by bone conduction so tested.

The performance of the test requires little notice, but there are one or two elementary points requiring attention. The meatus is best closed by pressure of the tragus inwards by the examiner's finger; certain wiry and inefficient tragi do not yield to such treatment, and in these cases the patient's moistened finger should be inserted into the meatus. Occlusion is aimed at, and increase of pressure is to be avoided, for gross increases of pressure may cause diminution of perception by bone conduction (Gellé's test—the result being undoubted, even if the suggested mechanism is uncertain). The pressure of application of the tuning fork should be the same when testing the patient as when comparing with the normal; this is considered in greater detail later in this paper; little practice is necessary to attain this equality. Any fork between 128 and 1,024 d.vs. per second may be employed. The 256, 512 and 1024 forks are most suitable and give readily defined end-points; the sound of the 128 fork is sometimes confused with a low-pitched noise which may be heard in the ear during occlusion of the meatus.

In the normal person perception by absolute bone conduction (T.F. stem) is slightly less than by air conduction (T.F. prongs) (*vide* later for this constant normal relationship).

Conductive disease results in loss of hearing power by air conduction (A.C.) with normal perception by absolute bone conduction (A.B.C.); if more than of the very

slightest degree, much less than that required to give a negative Rinne, it will result in reversal of the A.C.-A.B.C. relation. Perceptive deafness involves diminished hearing by both A.C. and A.B.C., the excess of A.C. over A.B.C. being if anything increased. A mixed deafness will cause loss by A.B.C. with greater and disproportionate loss by A.C.

It might be apposite here to compare the tests of absolute bone conduction and relative bone conduction (Schwabach).

The most important differences lie in the fact that absolute bone conduction affords an absolute index of the perceptive function, whereas relative bone conduction is influenced perhaps simultaneously by changes in both perceptive and conductive functions and is therefore indicative of neither.

There is a further difference which becomes evident during the test, in that the absolute bone conduction test requires the use of a Bárány noise machine in the opposite ear to that tested, less frequently than does the relative bone conduction test. Consideration of this question requires some examination of the mechanism of transmission of sound by bone conduction.

It is almost correct to say that bone conduction as such does not exist, i.e., that when a sounding tuning fork is placed on the skull, the sound vibrations are conveyed to the cochlea not by conduction as such but by sympathetic resonance of the cranial structure. This being the case, one would expect that the effective stimulus as the result of the application of a fork to the skull would be no greater in the region of application of the fork than at a distance, and it is indeed upon this fact that the theory is postulated. In this paper I use the term "bone conduction" without qualification, meaning not conduction as such but conduction by resonance. The existence of this conduction by resonance conveys important implications, affecting the performance of and deductions from the tests; the evidence for it is therefore important.

In a normal person place a sounding tuning fork on, say, the right mastoid, and occlude the left meatus; the fork is heard in the left ear. Immediately perception ceases, close the right meatus; the fork is heard either not at all or for a time so short that it might be explained by fatigue of the left ear.

Again, take a person with a unilateral nerve deafness; absolute bone conduction in the better ear is equal whether tested from the same or the opposite mastoid. Therefore, so far as absolute bone conduction is concerned, the stimulus is equal throughout the skull, and is not greater in the region of application of the fork; that is, there is not true conduction but conduction by resonance.

In regard to relative bone conduction one cannot be quite so definite, as it is possible that there is slight weakening of the stimulus at a distance from the point of application of the fork, but if this is so it is very much less than is commonly accepted. If one takes the effective stimulus to the cochlea of the same side to which the tuning fork is applied as 25, the effective stimulus to the opposite cochlea is certainly not less than 23. This is again readily and similarly demonstrated.

This cross stimulation dependent upon bone conduction by resonance is not sufficiently recognized, and as a result many tests are invalidated. Its importance appears in the accompanying table, in which are set forth the occasions on which the use of a Bárány box is necessary. Quantitative work has shown that for a fork of 512 d.vs. frequency, in a normal person, if the sensitivity by air conduction is 100, absolute bone conduction has a value of 50 and relative bone conduction a value of 25. These figures are utilized in the table; their precise estimation and significance will be discussed later.

TABLE II.

Condition of opposite ear	Ear tested: Relative bone-conduction test (Schwabach)	Ear tested: Absolute bone-conduction test
Normal	50 ↑	
23	25 ↓ 23	*50* ↓ 23
	Bárány necessary for all grades of N.D.	Bárány necessary for more severe grades of N.D.
Nerve deafness	50 ↑	
	25 ↓	*50* ↓
Less than 23	less than 23	less than 23
	Bárány necessary for grades of N.D. approximating to or exceeding that in opposite ear	Bárány necessary for highest grades of N.D.; exceeding, by a fair margin that in opposite ear
Conduction deafness 50	50	*50*
	Bárány necessary for all grades of N.D.	Bárány necessary for all grades of N.D.

N.D. = nerve or perceptive deafness.

The figures express sensitivity, the stimulus to the opposite ear being regarded as coming from a fork placed on the mastoid of the ear being tested.

The italic figures in the last two columns represent the normal findings; the arrows show the possible range of results of the respective tests.

Obviously it is impossible, without special precautions, to demonstrate any loss of sensitivity greater than that which requires a stimulus so loud as to affect the ear opposite to that tested. If this crossed sensation is anticipated one must exclude the other ear by the use of a Bárány noise machine. Now, in the Schwabach bone conduction test, the normal sensitivity of the two ears is equal (25), and therefore to demonstrate any degree of reduced sensitivity a Bárány box is essential. But in the absolute bone conduction test the normal sensitivity (50) of the ear tested is raised considerably above that of the opposite ear, where, the meatus not being closed, ordinary or relative bone conduction rules (25). It is thus possible to demonstrate a considerable degree of nerve deafness before a Bárány box becomes necessary.

Quantitative Estimation of Total Hearing.—Unsatisfactory though qualitative aural tests are, quantitative testing, where it exists at all, is still more unsatisfactory. Though otology is never likely to become an exact science, there is great need for a method of quantitative testing on an agreed basis that will yield accurate results which will be comparable wherever obtained.

Various modes of expression of hearing defect have been suggested—in arbitrary units, in physiological units (the sensation units of the American school (24)), in intensity units, and on a hearing distance basis. Of these the last is of most value, affording results which are intelligible even to the untrained mind, and which have a sound physical basis.

By this method we express hearing power as a fraction, i.e.:—

$$\frac{\text{The distance at which the subject hears a sound}}{\text{The distance at which a normal person hears the sound}}$$

or, better, we express this as a percentage. This we call the percentage retention of Normal Hearing Distance (percentage N.H.D.).

By this method we are at once presented with a defined mental picture of the degree of deafness; for example, it is obvious at once that a person with 10 per cent. N.H.D. can hear a sound at only one-tenth of the distance at which a normal person hears that sound. Moreover, it is a natural instinct to speak of deafness,

the loudness of sounds and hearing generally, in terms of distance. As a result the tests that have been most used, the ability to hear the voice, the watch, and Politzer's acoumeter, are on a distance basis.

Brief consideration may be afforded to the means of sound production employed in quantitative testing. The human *voice* has long been used for such tests, and inasmuch as the ability to hear and understand speech is the criterion *par excellence* of hearing ability, it is unfortunate that its disadvantages outweigh its advantages. Three steps of intensity are generally recognized, the shout, the conversational voice, and the "residual" whisper. The Normal Hearing Distance for each varies with the lung-power and mood of the observer, and the needs of the occasion. In using the test, note must be made of the test words employed, for the various consonants and vowels (according to their pitch) differ greatly in their carrying power, and are variously heard in the different types of deafness. The difficulties in the production of a standard voice are insuperable; attempts have been made by the use of phonographs and electrical apparatus to overcome these difficulties, but the success is not sufficient to lead one to expect their general application.

The ability to hear a watch is a most convenient test of hearing, and the results are on the rational hearing distance basis. The normal hearing distance for the watch under quiet conditions is usually under-estimated, but in the expression of results the Normal Hearing Distance should be that for the conditions at the time of testing, though this may lead to some over-estimation of the patient's hearing power, since a deaf person is less affected by extraneous noise than a normal person. An *Acoumeter* was devised in 1877 by Politzer [25], who hoped for the production of an instrument which would give a standard intensity of sound. The difficulties of production have apparently been too great, for acoumeters are encountered which vary widely in loudness. The normal hearing distance is some 40 ft.; the distance being so great, it is impossible to determine the N.H.D. under the conditions of each test. To express the patient's hearing power as

$$\frac{\text{Pt's H.D. for the acoumeter}}{\text{40 ft.}}$$

must usually lead to an exaggeration of the patient's hearing defect. As in the case of the watch, a high-pitched noise is produced, the tones averaging some 2,800 d.vs. per second. Care is necessary in testing, for with these high-pitched sounds, reflections from walls and other objects are particularly liable to vitiate the result.

Various electrical audiometers have been described. Probably the first was that of Urbantschitsch [26], who in 1890 described an audiometer which had frequency variable by a Neef's hammer, intensity variable by the separation of two coils, and a telephone for the production of sound for the patient. Some years later (1893), Gradenigo [27] had an audiometer, which was, however, limited to one frequency. In 1897, Seashore [28] produced an audiometer which has since been subject to considerable improvement; it is now known as the Iowa pitch range audiometer [29, 30]. Later audiometers have taken advantage of the wireless valve; examples are furnished by the Tucker audiometer [1], the Lowndes-Yates audiometer, and the models of the Western Electric Company of America. The Iowa pitch range audiometer and the Tucker audiometer at present give results merely in arbitrary units, i.e., of electrical current. The Western Electric Co. audiometers have been so adjusted as to read in terms of physiological units, the so-called sensation units. Although possessing advantages in the rapidity with which complete quantitative tests may be carried out, in the continuity of their tonal range and in the reduction of the personal factor to a minimum, I believe that, on account of their complications, lack of portability, and cost, they will never come into routine use.

Tuning Forks.—Tuning forks forming a part of the equipment of every otologist, essential for the performance of qualitative tests, it is natural that attempts

have been made to utilize them in quantitative testing. Since they can provide accurate information as to quantitative defect in hearing, and since their results can be expressed in terms both convenient and universally applicable, such use deserves considerable attention.

The value of tuning forks in quantitative testing was first suggested by Conta in 1864 [31]. He observed the duration of time the patient heard the fork, counting from the moment of striking the fork, and expressed the result as a fraction—this period over the normal period of perception of the fork. His results must have been completely lacking in accuracy and value. Some years later Magnus [32] also suggested their value, and expressed his results in a similar way. The expression of hearing defect in terms of duration of perception of tuning forks then became very widespread, and two sects became apparent; the one was content to express hearing defect as the number of seconds deficiency for a fork, i.e., the number of seconds a fork is audible to a normal ear after it had ceased to be audible to the deaf ear, the distance being the same; the other sect expressed the result as a fraction:—

$$\frac{\text{Duration (number of seconds) of perception of fork by patient}}{\text{Duration of perception of fork by normal person.}}$$

(Hartmann, etc., *vide* Dundas-Grant [33]).

The expression of hearing power merely as seconds deficiency for a fork has nothing to recommend it; it ignores the very wide differences in the rate of decrement of forks, which will frequently cause errors of 200 or 300 per cent. in such estimations. The fractional expression is only slightly more accurate. It takes no account of the initial amplitude of vibration of the fork, which varies widely even in the same fork, and much more so in different forks. Moreover, since decrement is logarithmic, such a time-fraction conveys a totally wrong picture of the hearing power.

Attempts have been made to overcome some of these defects by the production of forks giving universal and constant initial intensity (by mechanical strikers, etc.), and with universal similar decrement. In neither object has success been obtained, and it is extremely improbable that future attempts will meet with any more success.[1]

Gradenigo, in 1899 [34], devised some optical forks on which it was possible to observe the amplitude of vibration at any time during the fork's vibration; but the units were arbitrary, the gradations extremely rough, and the value confined to forks of low frequency.

Bezold and Edelmann in 1898 [35] published a paper in which they described an instrument for the graphic recording of the curve of decrement of vibration of tuning forks. Their results were obtained from forks covering the lower two and a half octaves (16 to 96 d.vs. per second), and they claimed that such a curve would be applicable to all tuning forks of the higher pitches. Schmiegelow [36], in the following year, criticized their results and conclusions; he suggested the construction of a curve on a distance basis for each fork, and expressed his results on a relative intensity basis. He constructed his curves by a number of experiments and rather disparaged the law of logarithmic decrement.

Harvey Fletcher [24] has recently suggested the expression of tuning fork results in terms of sensation units, calculated from the seconds deficiency.

Holding, as stated in the first part of this section, that the most rational and convenient way of expressing hearing power is in terms of percentage retention of normal hearing distance, and since tuning forks may readily provide information on such a basis, I shall outline this practice and the mathematical considerations underlying it.

1 Bezold [21] stated that such attempts at the production of forks with standard initial intensity and standard decrement could only result in the production of inferior forks.

The hearing is tested in the usual way, the duration of after-perception of a tuning fork by a normal ear after perception by the patient has ceased being noted in seconds (seconds of deficiency). By reference to a table this figure is then translated into percentage retention of normal hearing distance. Such a figure is of course open to comparison, and will be identical with the findings for the patient by other forks or instruments of the same pitch. The table referred to would be supplied by the makers of the fork at the time of purchase; if not it can be readily drawn up at the expense of a little time, with the aid of a few experiments and a little practice in mathematics. Or the standardization of existing forks might be determined by a central institute, for what need only be a small fee. Where one contents oneself with quantitative estimation for a single pitch, reference to such a small printed table will be most convenient: where one performs complete quantitative testing for a range of forks, a graph recorder, as suggested by Lowndes Yates, is conveniently employed.[1]

This use of tuning forks has been objected to on the ground that it introduces a second personal factor; this is so, but its importance can easily be exaggerated, as a very little practice enables one to speak with confidence as to the end point of perception of a fork. It cannot, of course, be denied that such tests are impracticable for examiners who suffer from deafness of varying degree. A constant degree of deafness can to some extent be compensated. Those who cannot thus circumvent their handicap must either rely upon costly and cumbersome audiometers, or employ their secretary as "corpus vile."

Selection of tuning forks for these tests, while by no means essential, can do much to facilitate their carrying out. The lower forks should be as free from overtones as possible and should have a fairly rapid rate of decrement. All low forks have a slow (distance) decrement, and if this is at all exaggerated, testing may be very tedious. The higher forks should be of good quality; overtones, if present, may not be perceived by the examiner and may rob any result of value. These higher forks (e.g., 512 d.vs. and over) should have as slow a decrement as possible, for the greater accuracy obtained more than compensates any extra time taken in testing.

While the translation of results from seconds deficiency for any fork to percentage N.H.D. is for the otologist merely a matter of referring to a table and requiring no mathematical knowledge, it is of interest to discuss the mathematical and physical basis underlying the obtaining of such results, and the circumstances producing them.

The Physical Basis of Tuning Fork Quantitative Tests.

(*A*) The fundamental basis of these tests rests upon the "law of logarithmic decrement," governing bodies in free vibration. Tuning forks, except when their vibrations are forced, or of unnaturally great amplitude, conform to it. It is expressed—

$$\frac{A1}{A2} = \frac{A2}{A3} \quad C$$

where A1, A2, A3, are successive amplitudes of vibration after equal time intervals.

(*B*) Since the distance at which a sound is audible varies directly with (but as the square root of) the amplitude of vibration of a body, this formula can be expressed in distance terms—

$$\frac{D1}{D2} = \frac{D2}{D3} \quad C$$

[1] I must gratefully acknowledge that I have been much helped by this author's paper [37], and stimulated by his conversation.

where D1, D2, D3 are successive distances at which the fork is audible after equal time intervals.

This may be clearer if expressed

$$D1 \div C = D2, \quad D2 \div C = D3, \text{ and obviously } D1 \div C^2 = D3.$$

Further, suppose that in this case the time interval was t seconds, the "decrement factor" (distance) for this period was C; for twice the time interval (audibility changing from D1 to D3) it is C^2.

now for t seconds the factor is C
„ 2t „ „ „ C^2
therefore „ 1 second „ „ $t\sqrt{C}$
and „ n seconds „ „ $t\sqrt{C^n}$

(*C*) Mention has been made of hearing distances, and this requires qualification. Hearing distance is obviously the distance from a sounding body to the receptive point of hearing; this is equal to the distance from the source of sound to the tragus plus the distance from the tragus to the receptive point of hearing. This latter distance is functionally decidedly less than the anatomical length of the meatus; one can explain this according to one's beliefs by saying that in the meatus one is dealing with conduction in a closed tube, or by saying that the function of the meatus is primarily by resonance and only secondarily by conduction.

The determination of this functional length of the meatus is easy from the formula $D1/D2 = D2/D3$; it was discussed in Lowndes Yates' recent paper [37], and his finding of $\frac{1}{2}$ in. agrees closely with mine. Schmiegelow [36] made the distance slightly more. Throughout this paper, as should be the usual practice, hearing distance therefore means the distance from the sounding object to the tragus, *plus* $\frac{1}{2}$ in.

(*D*) It is a simple matter to determine the "factor of decrement" (distance) for a fork, but accuracy will depend upon averaging a number of results.

Hold a fork at a distance, D 1 from the ear; when no longer perceptible approach it to D 2; it is audible for t seconds. Then the factor for an interval of t seconds = D 1/D 2, and is a constant for the fork. For one second the factor equals $t\sqrt{D1/D2}$.

We will in future call this constant factor of decrement for an interval of 1 second—K.

(*E*) Now $D1 \times K = D2$ (for an interval of 1 second, K being the factor for 1 second, and D 2 being the distance at which audibility ceased 1 second before ceasing at D 1).

$D1 \times K^2 = D3$ (interval of 2 seconds), etc. Therefore, having previously calculated K, and knowing D 1, we can determine the distance at which the fork was audible any number of seconds before it ceased to be audible at D 1, e.g., n seconds before ceasing to be audible at D 1 it was audible at $D1 \times K^n$.

We can therefore express the hearing power for any number of seconds deficiency (as compared with the normal) as the fraction

$$\frac{D1}{D1 \times K^n} \text{ or, cancelling out, } \frac{1}{K^n}, \text{ n being the number of seconds deficiency.}$$

(*F*) It has been said that hearing power is best expressed, not as a fraction, but as percentage retention of normal hearing distance. Thus, for n seconds deficiency it would be:—

$$\frac{D1 \times 100}{D1 \times K^n} \text{ or, cancelling out, } \frac{100}{K^n} \text{ per cent. N.H.D.}$$

I have nowhere met a formula so simple for this expression.

By giving n different values, we can readily draw up a table covering the whole period of vibration of the fork.

I have suggested in an earlier section that all tests of bone conduction should be of absolute bone conduction, and I believe that quantitative determination of bone conduction can afford valuable information as to perceptive function.

Quantitative Estimation of Perceptive Function.

One cannot obtain the same accuracy in testing absolute bone conduction as in testing by air conduction owing to the greater difficulty in determining the end point of perception. The value of quantitative testing of bone conduction is confined to pitches between 128 and 1,024 d.vs. per second; the 128 fork is on the border-line of reliability, as its end point is sometimes impossible to determine. Great accuracy being impossible, importance cannot be attached to small variations. In testing duration of perception of tuning forks by bone conduction, attention must be paid to auditory fatigue, which occurs readily in this form of testing, much more readily in some cases than in others. It is advisable to lift and replace the tuning fork, rather than to keep it in constant apposition to the mastoid.

The efficiency of contact between the tuning-fork base and the bony skull plays a large part in determining the amount of perception in this test. Doubtless the application of the tuning fork to the teeth, with the intermediation of some modified dental prop, would best overcome this source of error; but I believe that with very little practice one learns to use a fairly constant degree of pressure, reducing the error to negligible proportions. Lake and Peters [38], however, did not think so, and they devised a spring device for attachment to the stem of the fork.

In the normal ear, using tuning forks (i.e., prongs for testing air conduction, stem for bone conduction), perception by air conduction always exceeds perception by absolute bone conduction, and the two bear a constant ratio to each other. With the forks I have used I find, by calculation, that when perception by absolute bone conduction ceases, the fork is audible if held half an inch from the tragus, that is, at one inch hearing distance. This applies approximately to the 128, 256 and 512 forks. The 1,024 fork is audible similarly at one and a half inches hearing distance. Now in testing bone conduction the fork may be taken as being at the minimum distance; in testing air conduction the minimum distance is the length of the meatus, or $\frac{1}{2}$ in. In comparing bone conduction with air conduction, the minimum distance is therefore taken as $\frac{1}{2}$ in. Since the fork on ceasing to be audible by normal absolute bone conduction is actually audible by normal air conduction at 1 in., normal A.B.C. corresponds in terms of A.C. to a Hearing Distance of $\frac{1}{2}/1$ or 50 per cent. N.H.D. For the C3 fork, since it is audible at $1\frac{1}{2}$ in., the figures are $\frac{1}{2}/1\frac{1}{2}$, or 33 per cent. N.H.D. These figures are approximate; the actual figures are: C, 49 per cent.; C1, 54 per cent.; C2, 50 per cent.; C3, 32 per cent. N.H.D. While more work on this point is required, and while the figures will probably vary according to the construction of the forks, such figures should supply a fair average for the relationship.

It may at first seem unnecessarily complicated to express absolute bone conduction in terms of air conduction, but it is rational and has the advantage of making any alteration in the relation of the two immediately apparent.

In testing a patient, one observes the end point of his perception by absolute bone conduction, and then determines how many seconds longer it is perceptible by normal absolute bone conduction.

Such seconds deficiency absolute bone conduction are not comparable to seconds deficiency air conduction, for the application of the tuning fork base to the mastoid entails resonance of the cranial structures with consequent more rapid decrement of the fork's vibration. This increase in decrement is readily determined for each fork with sufficient accuracy for all ordinary purposes. The method of its determination is best explained by an example.

Example.—Suppose one takes a fork for which it has been determined that, after ceasing to be audible at 3 in. distance, under normal conditions of damping, it is audible at the tragus for a further 50 seconds. Hold the fork at 3 in. distance, and as soon as it ceases to be perceptible place the base on the mastoid and test absolute bone conduction; suppose it is audible by A.B.C. for 20 seconds; immediately perception by A.B.C. ceases, remove the fork and hold the prongs to the tragus, testing A.C.; suppose it is further audible here for a period of 20 seconds.

Normally, as stated, the fork after ceasing to be audible at 3 in. would have been audible at the tragus for a further 50 seconds; actually it was audible for 40 seconds (20 testing A.B.C. plus 20 testing A.C.); obviously the increased rate of loss must have occurred while A.B.C. was being tested; then it lost in 20 seconds testing of A.B.C. what it would have lost in 30 seconds of A.C. This may be expressed for the fork as A.B.C.: A.C. = 20 : 30 = 1 : 1·5. Then for this fork, if A.B.C. seconds deficiency is to be comparable to A.C. seconds deficiency, 5 seconds must be added to every 10 seconds A.B.C. deficiency.

It is of course not essential to compare the patient's absolute bone conduction with the normal absolute bone conduction; instead, its seconds deficiency as compared with the normal perception by air conduction may be determined, the interval between the normal air conduction and normal absolute bone conduction being known.

The results so obtained for absolute bone conduction are ready for direct comparison with the results of the tests of air conduction; and by reference to the same table are translated into percentages N.H.D. This is readily graphed, or the result may be expressed as the fraction :—

$$\frac{\text{Patient's A.B.C. as percentage N.H.D.}}{\text{Normal A.B.C. as percentage N.H.D.}}$$

This is rational, for it gives the normal A.B.C. a value of 1, and the minimum A.B.C. a value directly proportional to the maximum intensity of the fork—the maximum degree of deafness which we are justified in assuming as the result of any test.

In this paper I have outlined methods by which we can accurately assess the total hearing power (A.C.) and the perceptive function (A.B.C.); from which data the condition of the conductive apparatus can at once be deduced.

The results are expressed on a real and intelligible basis and are comparable, wherever and by whomever obtained. They can be expressed in graphic form and, if necessary, an accurate picture of the hearing power throughout the pitch range can be presented. While no necessary part of the method, I believe that such full testing, carried out as a routine, would result in some reorganization of ideas and the general improvement of otology.

The paper has attempted to cover rather a wide range and, in order to clarify its important parts, I shall conclude by describing my method of functional examination and by figuring a typical "decrement table."

TABLE III.

Fork. 512 d.vs. ... ... Mark A.

Factor 0·052

Seconds deficiency	A.C. per cent.	A.B.C.
0	100	50/50
1	95	—
3	86	41
5	77	38
7	70	34
10	60	28
15	46	20
20	36	16
25	28	11
30	22	8
35	17	6
40	13	5
45	10	4
50	7·8	2·5
60	4·7	1·3/50
70	2·8	
80	1·7	
90	1	

B.C. : A.C. = 1 : 1·2
A.B.C. = − 13 seconds, or 50 per cent. N.H.D.
R.B.C. = − 27 seconds, or 25 per cent. N.H.D.

Examination.

I test the hearing distance for the watch (N.H.D. according to conditions at time of testing). If this is not heard or the distance is small, I test with the acoumeter (N.H.D. = 40 feet, or c. 500 inches).

I then instruct the patient to occlude both meatus, and then place a vibrating tuning fork above the glabella; if there is any lateralization of perception I know that there is unilateral, or unequal bilateral, perceptive deafness. (This test, lateralization of absolute bone conduction, might be useful to the neurologist, whose chief anxiety is a unilateral nerve deafness.)

I then hold a fork (preferably 256) close to the tragus and, when perception ceases, transfer the base to the mastoid, occluding the meatus, preferably by pressure on the tragus with a finger of the other hand (if he hears it he has conductive disorder); on this perception ceasing I immediately see if I have any further perception by absolute bone conduction; if I have, the patient has some additional perceptive deafness.

Quantitative estimations for the two ears may be performed at the same test, but A.C. and A.B.C. are best tested separately. All the necessary particulars are in the table of reference. (Decrement Table.)

I then test with the C5 fork, and usually test the upper limit (monochord A.C. and B.C.) and the lower limit. Such testing only occupies four or five minutes. If complete investigation is required, I then proceed to do quantitative tests for all the C forks (C—1 to C5 by A.C., C—1 to C3 for A.B.C.).

Such complete examination of a case, with production of graphic records, occupies about twenty minutes.

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Discussion.—Mr. O. POPPER said it had been stated that the patient's ear was compared with a normal ear; was it always assumed that the examiner's ear was normal, or was a normally hearing person secured for the purpose?

Further, it was said that the percentage retention at normal distance was expressed in seconds, which meant that the factor of distance was used and was expressed in seconds, thus using two systems of units, time and distance. If a patient heard the tuning fork only at 2 ft. and the normal was 4 ft., was it assumed that such a patient was 25 per cent. deaf?

He disagreed with Dr. Clarke's tuning-fork methods. The Gellé, Schwabach and Rinne tests were not hearing tests *per se*. Hearing was used in those tests as an indicator, just as methyl orange was used to indicate the point at which a fluid became acid or alkaline.

They were designed to show that conduction was hyper- or hypo-functional. The trouble was that two absolutely identical tuning forks could not be produced, even if the maker was above suspicion. One held this fork at a constant distance, then compared with the so-called normal, then noted the difference of hearing in time, expressing this difference as a percentage of deafness. In a paper on the otometer, read at a previous meeting of the Section, he (the speaker) pointed out the enormous fallacies to which this technique was liable, even where the so-called "constant" of each tuning fork used had been accurately determined. Percentages of normal hearing distance were therefore also erroneous.

The electrical method (otometric, or audiometric) had none of these serious drawbacks.

Reverting to the question of arbitrary units, Dr. Clarke had used the figures 23 and 50, which he (the speaker) could not understand; presumably some form of arbitrary unit was used here, but it might be clearer when he read the paper at leisure.

He (Mr. Popper) did not agree that the audiometer was clumsy, not portable, complicated and expensive. It was portable and simple to use, and he believed its cost was not excessive.

Sir JAMES DUNDAS-GRANT said that tests for hearing were taken too seriously when reduced to figures in the way exemplified. The otologists were not prepared to abandon the tuning-fork methods, used for so many years. Those who had studied tuning forks felt that there were certain details which the speakers had not touched upon, but which every experienced practitioner must have in mind. Note had not been taken of the weight of the fork; too heavy a fork was not calculated to give a very satisfactory result for bone conduction, and Dr. Clarke had said that the fork (c^1) giving 256 double vibrations per second was the one to use. Some Continental testers preferred the A fork. But even forks of comparatively light weight varied very much. There was a certain quality in each fork which could be calculated, but by a rather complicated method. One should know how long a given tuning fork was heard when held opposite the meatus when it had ceased to be heard on the mastoid; the fork should be heard roughly about twice as long at the meatus as on the mastoid. Gardiner Brown's fork was, as nearly as possible, a typical one. Many were made so heavy that there was too prolonged hearing through the air after it was no longer heard on the mastoid. The real Rinne test, as it had been handed down, was, that when a fork ceased to be heard on the mastoid it ought again to be heard opposite the meatus. If it was not heard again the test was said to be negative. After it had ceased to be heard at the meatus it ought not to be heard on the mastoid, but if it were there would be an exaggerated Rinne. The dying down of the tuning fork note was very interesting. In connexion with a paper which he (the speaker) had read in Boston, U.S.A., in 1912, he had had the assistance of the late Dr. Womack. He showed one slide indicating the length of time that the patient heard the tuning fork, and another indicating the degree of hearing this denoted. The equation

$$a_t = a_0 e^{-\frac{kt}{2}}$$

expressed the diminution of the amplitude of vibration at the end of t seconds of a tuning fork whose decrement vibration from second to second was expressed by k. It was the inverse of the compound interest formula, in which the rate of interest was expressed by r instead of k.

One obtained the ratio of auditory perception as a percentage, the normal being taken as 100, of which the logarithm was 2. The difference in the number of seconds during which the observer heard the fork after the patient did, could be taken as a definite measurement of his hearing power. It was a really scientific means of testing.

Mr. F. J. CLEMINSON said that he had been much impressed by the last paragraph of Dr. Clarke's paper, describing his method of testing, which was extraordinarily efficient for all ordinary clinical purposes. It would probably enable one, in about three minutes, to arrive at a diagnosis and prognosis. The Rinne test had now been superseded.

Mr. ALEX. R. TWEEDIE said that the President's modification of the Schwabach test appealed to him and he proposed to adopt it.

The diagram suggested by Dr. Kerr Love for the classification of deaf children should be most helpful for educational purposes, and the only point he ventured to criticize was the second line, which rather indicated that the normal range for hearing a whisper was 20 ft., which of course Dr. Kerr Love did not intend to imply.

He (Mr. Tweedie) suggested that this should be amended to read, "Children with a range for whisper at 20 ft. or over do not require education in special schools."

What was Dr. Clarke's method of carrying out the Gellé test? Did he simply close the meatus with one finger? [Dr. CLARKE: Yes.]

He (Mr. Tweedie) suggested that Dr. Clarke should try the modification of these tests as described by Bárány. (See abstracts of original paper in the *Journal of Laryngology, Rhinology, and Otology*, 1909, 601, 612, 703.)

Mr. G. J. JENKINS said that ever since he had heard papers read by Dr. West, supported by Mr. Sidney Scott, he had used the Bing test. The papers just read indicated a real endeavour to standardize the tests, whether by the audiometer or by the tuning fork. Probably the best results would be arrived at by a combination of the two.

Mr. STORY (Secretary to the National Institute for the Deaf) said that the National College of Teachers had invited the National Institute to set up a properly classified system of education for the deaf children of this country. At present there was practically no classification, and one found children with all kinds of histories of deafness and all degrees of the defect planted in the same school, often in the same class. This was unscientific and obviously not suited to the best interests of the children. As helping towards classification, the Institute wished to devise a terminology of deafness based on history and degree, which would be intelligible to the ordinary teacher and the ordinary medical practitioner, for they had first of all to deal with these children. A test was required which was capable of being rapidly applied, even though the results might not be absolutely scientific. It was only required by them for educational purposes. In the hands of the local practitioner, the sheet which Dr. Kerr Love threw on the screen would be a very valuable one for dealing with deaf children. It might be that the general practitioner did not know a great deal about deafness, but he knew the subject in a general way, and such a sheet would be a good guide as to what it was best for the parents to do in the direction of having the child educated suitably with his physiological condition.

Mr. A. D. SHARP (Leeds) said he had something to do with a large deaf-and-dumb school, and he was much interested in the classification shown by Dr. Kerr Love; it was a sound and business-like classification.

His (the speaker's) estimate of the proportion of cases due to syphilis was from 3 per cent. to 5 per cent., figures closely approximating those of Dr. Love.

With regard to tuning-fork testing, there was a kind of patient—mostly of the toxic type—who, when the fork was held at hearing distance, said that it stopped during two or three seconds and then went on again. He (Mr. Sharp) did not understand the significance of that, and wondered whether any other Member had noted the same happening. It suggested a sort of "blind-spot deafness," comparable with the case of the eye in seeing.

In testing with the tuning fork, the fork should be held at the right angle, namely, at right angles with the bone surface. The result would differ as to whether it was held over the antrum, a small distance higher or lower, or posterior.

Dr. KERR LOVE (in reply) said he would be glad to meet Mr. Tweedie's suggestion by stating, parenthetically, that this classification had nothing to do with any except those in a school for the deaf. His meaning was that when a child could hear whispered speech at 20 or more feet there was no place for him in a school for the deaf.

Mr. CLARKE (in reply) said that Mr. Jenkins and Mr. Popper had mentioned the weakest point in the quantitative use of tuning forks, the keeping constant of the fork's position. He believed, however, that a little practice in the method enabled one to do so with a fair degree of accuracy.

He had to answer much criticism from Mr. Popper. Constant abnormality on the part of the observer could be compensated; personally he was quite content to regard himself as normal. Mr. Popper had questioned the amount of hearing possessed by a person who heard at 2 ft. what a normal person heard at 4 ft.; of course, such a person had *in terms of distance* 50 per cent. of hearing. It could not be said that percentages of the normal hearing distance were arbitrary units. Nor was it necessary to attempt to obtain identical sets of tuning forks. Mr. Popper had stated that the seconds of deficiency for a fork were merely arbitrary units to be used in an attempt at a more satisfactory method of recording; he (Mr. Clarke) claimed that the attempt had been successful. He regretted that he had not closely examined the audiometer demonstrated by Mr. Popper; it might be easily operated, reliable and cheap, but he was very far from agreeing that it was convenient.

CASES.

Protrusion of the Meninges in the External Auditory Meatus Simulating Aural Polypus.—H. NORMAN BARNETT, F.R.C.S.—This patient, a woman aged 42, was sent by her doctor for treatment of an aural polypus in the left ear. On examination she was found to have a growth in the external auditory meatus indistinguishable from an ordinary polypus on inspection. She complained of giddiness and gave a history of having had a "mastoid" operation performed by a general surgeon twelve years previously. There was a scar 2½ in. behind the left ear. In view of the history, and the patient being obviously ill, it was decided to admit her to hospital and not to proceed to any local operation on the polypus. The patient was admitted on July 21, 1928, and operated on next day. An incision was made close behind the left ear and superficial structures, which were matted together, were removed. It was then found that a large piece of bone had been resected at the former operation, presumably for some brain condition. The bone was now removed in a forward direction, the posterior bony wall of the meatus taken away and the tympanum opened. The neck of the polypus was followed upwards and was found to enter the skull and finally to spread out like a rosette; on gently pulling this it came away followed by a gush of foul pus. A cerebral trocar and cannula was passed along the channel of the pus and a temporo-sphenoidal abscess found and drained. The cannula was left in for 48 hours and then replaced by a Wick drain. A large amount of cerebro-spinal fluid came away, causing considerable anxiety, but ceased in about 24 hours. The patient made a slow convalescence, temperature varying from 96° to normal, and was finally discharged this month in good health with wound soundly healed and only a little mucoid discharge through the external auditory meatus.

This case is noticeable for the simulation of aural polypus constituting a grave danger if removed by a snare, and for the fact that a woman was able to go about and work for a long period in such a condition as that found at operation.

Deafness with Unusual Bone Condition of the Inner Ear.—H. NORMAN BARNETT, F.R.C.S., and H. GRAHAM HODGSON, M.B., D.M.R.E.—This patient, a woman aged 38, came to consult me on account of deafness, July, 1928. There was a history of deafness on the male side, but none on the female. She had suffered from pleurisy and pneumonia seven years ago, and had had an operation on tonsils and adenoids following this illness. On examination I found remnants of septic tonsils and distinct deflection of the nasal septum. The tympanic membranes were retracted and thickened. On testing the hearing function there was found to be very slight nerve deafness on the right side; tuning-fork note hearing was markedly decreased on both sides for high and low notes, as was that for watch sounds, while conversation was reduced to 12 in. on the right and 36 in. on the left, and whisper to 6 in. on the right and 12 in. on the left. Gellé test was positive. The Eustachian tubes were clear. No tinnitus. I concluded on the clinical evidence that this was a case of middle-ear deafness with well-developed adhesions between the structures in the tympanum, not one of otosclerosis, and sent the patient to Dr. Graham Hodgson for X-ray examination of the labyrinth.

I propose to remove the septic remains of tonsils and the nasal septum, and to give a course of iodine vapourization of the tympanum and ionization with iodide of potassium or silicon, and to carry out re-education of the centre. The patient has had to return to Australia for family reasons, but hopes to return later for treatment. It is of value to have the case put on record before anything is done, and later to report results; and also because of the very interesting and unusual condition found in the labyrinth on X-ray examination.

Mr. NORMAN BARNETT said that Mr. Graham Hodgson had reported that there was no definite conclusion in his mind about this case; he had never seen a similar one,

and was uncertain whether it was a normal ear with unusual bone development or whether there was a new growth of bone. He could detect no radiological evidence of the porosis of the cochlea which he had always seen in cases of otosclerosis. At present, however, negative X-ray evidence was not sufficient to rule out the possibility of otosclerosis.

Temporo-Sphenoidal Abscess. Hernia Cerebri. Recovery.—SOMERVILLE HASTINGS, M.S.—L. B. Discharging ear since 1926. *July*, 1926.—Acute mastoid (Schwartze). *August*, 1926.—Secondary suture. *May* 17, 1928.—Radical mastoid, extradural granulations. *May* 21, 1928.—Temporo-sphenoidal abscess opened. Half an ounce or more of pus. *May* 23, 1928.—Large hernia cerebri developed gradually, 4½ in. in diameter on June 13, 1928. *June* 13, 1928.—Hernia cerebri explored and about an ounce of pus removed. Gradual shrinking of hernia. *September* 30, 1928.—Completely healed. *Present Condition.*—Mentality good; slight mental depression. Acumeter heard 8 in.

Discussion.—Mr. SYDNEY SCOTT asked on which side of the brain was the abscess?—[The PRESIDENT: The right side.]—Also were the indications for the exploration, any special neurological or general surgical indications? It was difficult to diagnose brain abscess on the right side in right-handed people.

The PRESIDENT (in reply) said that exploration had been decided upon because extradural granulations had been found when the mastoid was opened up (May 17, 1928), but in spite offree mastoid drainage, the temperature remained high, the patient became drowsy, and slight weakness of the left side of the face developed.

Papilloma of External Auditory Meatus.—SOMERVILLE HASTINGS, M.S.—E. H., female. History of deafness and discharge from right ear six or eight months. *On Examination.*—Papilloma growing from posterior meatal wall.

Discussion.—Mr. T. B. LAYTON asked what the President proposed to do with the papilloma, if anything. Would he remove bone?

Sir JAMES DUNDAS-GRANT asked what was the result of probing in epithelioma simulating papilloma. He had himself found an enormous amount of denudation of bone beyond it.

Mr. SYDNEY SCOTT said that in case the papilloma proved to be malignant, he suggested applying surgical diathermy. He preferred to have a pathologist present at the time of operation, to cut a frozen section. If it were malignant, he would at once perform a more extensive operation than one originally planned.

The PRESIDENT (in reply) said that he proposed to carry out a modified mastoid operation, removing the posterior wall of the meatus, cartilage included, after removing some bone.

[The report of the specimens shown by Mr. E. D. D. DAVIS, and of the case shown by Mr. J. A. GIBB will appear in the next issue of the *Proceedings* of the Section.]

War Section.

[*December* 10, 1928.]

Some Common Problems in Naval Hygiene.

By P. M. Rivaz, M.B., D.P.H.

(*Surgeon Commander, R.N.*)

In this paper I shall try to discuss everyday problems with which I, as a naval health officer, have been confronted in different parts of the world during the last three and a half years. The problem of naval hygiene resolves itself into how to keep men fit, living, as they do, in a steel ship subjected to sudden and often considerable variations of climatic conditions. The men have to live and work in a very confined space, and altogether the conditions on board necessitate constant vigilance and co-operation between various departments to prevent an undue amount of sickness and disease. Also a ship is a fighting machine and although the authorities are aware of the importance of keeping men fit, yet in certain matters hygiene has to give place to fighting efficiency.

A considerable amount of literature has accumulated about ventilation of warships, and many committees have dealt with the subject. However perfect a system of artificial ventilation may be, it cannot compete with a natural supply of air. There may be adequate means to regulate the temperature, to render the air bacteria-free and to ensure its chemical purity, but, all the same, such air always appears devitalized and stuffy. Unfortunately in a ship, exhaust ventilation can only be utilized to a very limited extent. It has been found impracticable to exhaust cabins and mess decks, as the exhausted air would usually be replaced by foul air from the deeper parts of the ship. Exhaust ventilation is only applicable to spaces such as engine and boiler rooms, where very high temperatures obtain, or to compartments such as wash-places, lavatories, cooking galleys and bakeries having their own peculiar atmosphere and odours which have to be dissipated as soon as possible to obviate their permeating the ship.

Constructors say that the original plans of a ship always provide a perfectly adequate system of ventilation. In course of construction, however, structural alterations have to be made, which often leave the ventilation system unsatisfactory.

The best position for the air intakes appears to be a matter on which there is considerable divergence of opinion. The ideal system is warm feet and a cool head with a floor temperature of about 60° F., and a head temperature a few degrees lower; the air should be changed sufficiently often to keep it pure but undue draughts must be avoided.

Now, it appears to me impossible to evolve a system of ventilation which will be equally satisfactory in the North Sea in winter and in the tropics; yet our ships are liable to serve in any part of the world. It is comparatively easy to warm a ship but difficult to keep it cool. I take it that warm air should be delivered low down in a compartment and in most of the older destroyers which I have been over the air inlets are about a foot from the deck, but often tucked away in a corner of the compartment. Apart from the fact that air delivered at such low level gets contaminated by dirt from the deck, this low delivery apparently proves satisfactory in cold climates but is most unsuitable for hot weather. In the majority of ships the inlets are rather above head level, probably the best level for delivery of cold air, but unsuitable if a warm air supply is desired.

The punkah louvre system by which the inlets work on a ball-and-socket joint which can be adjusted to deflect the air to any required angle is, I am told, highly successful. I recently went on board one of our newest destroyers fitted with this system which had just returned from a cruise in a hot climate and the officers and crew were very pleased with the general air supply.

The ventilation committee in ships perform their duties with varying degrees of energy. It is surprising how often old-standing defects in the ventilating system are detected; trunks get blocked, supply-fans are found acting as exhausts and *vice versa*, and constant vigilance is needed. Unfortunately it often happens that the individuals who suffer from unrecognized defects are those who work in inaccessible spaces and who are too diffident to complain. Fans have to be rested for a certain period each day and in a well-run system of ventilation it should be possible to rest individual fans at a time which gives the minimum discomfort to the crew. For instance, fans supplying cabins and mess-decks should be rested during the forenoon, when the occupants of these spaces are mostly working elsewhere, while fans supplying offices can be rested at night. In some ships it appears still to be the custom to rest all fans at the same time, thereby causing unnecessary inconvenience to a large proportion of the ship's company.

During a recent tropical cruise in which two battleships were employed, the ventilation system was very carefully inquired into. Weak spots were quickly detected directly the ships got into really hot weather, defects which were not apparent in a cool climate. In certain cases exhausts were found to be in unpleasant proximity to intakes, so that a vicious circle was established and hot foul air was sucked down again to living spaces. Many of these defects were easily remedied by the ship's staff.

Various recommendations were made to render those types of ships more habitable in hot climates. The desirability of abolishing as far as possible the multiplicity of small offices was emphasized. Each department naturally wants its own office for reasons of quiet and privacy, but this renders the ventilation much more difficult as it leads to pocketing of air.

A much more liberal supply of table fans was recommended. The numbers allowed for officers' quarters is usually fairly adequate but the supply allocated to the ship's company should be largely increased. A great number of kata-thermometer readings were taken and the conclusion was arrived at that table fans in suitable positions on the bulkheads were preferable to overhead fans. This is not the general opinion but it was our experience.

Hot, insufficiently ventilated compartments raise the question of a suitable tropical uniform to be worn on board. In small ships on isolated service, officers and ratings, during the heat of the day, wear the sensible rig of a short-sleeved tennis shirt or singlet, shorts and canvas shoes. Even in big ships the ratings wear no jumper or coat but a flannel which leaves the neck exposed. The officer, however, wears either a tunic which buttons closely round the neck, or a jacket with a roll collar which necessitates a collar and tie. He also wears trousers. A more unsuitable rig could hardly be imagined or one more calculated to prevent dispersion of bodily heat. If battleships will shortly be based in Far Eastern waters perhaps the whole question of tropical rig will be considered and a more suitable one evolved.

I will not enlarge on the vexed question of heat stroke. I recently attended an interesting discussion on this subject at Malta, and the conclusion arrived at was that although symptoms vary greatly, it is unwise in the present state of our knowledge to differentiate too closely between clinical varieties. Whether the underlying cause is toxic or physical, the heat-regulating centre in the medulla is upset, and the chief indication is to enable the individual to get rid of his surplus bodily heat. In coal-burning ships cases of heat exhaustion, usually fairly mild, were common among the younger members of the engine room department. In the present oil-burning ships, where stokers do not perform such hard manual work, this condition does not arise nearly so frequently although the temperature in the boiler rooms may rise as high as in the older type of vessel. Better ventilation with a freer movement of air has undoubtedly contributed materially to this decrease, but probably a most important factor is that in coal-burning ships, stokers have to wear

heavy "fearnought" trousers to prevent themselves getting scorched, while in oil-burning ships thinner clothing is possible, and consequently bodily heat is got rid of more easily.

The coloured races are said to be able to work without ill-effects in the direct rays of the tropical sun, although they have no head covering. This immunity may be partly due to their pigmented skin which is said to absorb certain deleterious rays. But it must be remembered that the coloured coolie is practically a vegetarian and works in an ideal rig—practically no clothing. He sweats profusely and his skin is always cool.

During the war I am informed that in certain tropical areas when raw coloured levies were suddenly put into uniform, they suffered from heat stroke just as much as the white sailor, when on landing parties and duties ashore. He would be a bold man who recommended that the tropical helmet should be discarded, but it is open to question whether less and looser clothes are not equally important. I should appreciate views as to the most suitable material for tropical rig and as to the value of coloured spine pads.

In certain hot stations native labour is utilized during trying times to assist the stokers below. It is a common experience that these natives cannot stand hot conditions between decks as well as the European, although these same individuals work cheerfully on the upper deck. May not the explanation be that the native, who is usually very lightly clad, cannot tolerate the additional clothing which work in the stokehold necessitates? Training and acclimatization are important factors. Many big passenger lines who sign on the same native firemen for long periods have no difficulty in this respect; apparently the trained acclimatized Lascar can work in an engine room as well as anybody.

The food problem is receiving more and more attention in the Navy. The system of general messing, which now obtains in shore establishments and in many ships, enables men to have a varied and well balanced diet. Although the messing arrangements are under the supervision of the accountant officer, the weekly menus are sent to the medical officer for scrutiny, and he can make suggestions. General messing will probably never be introduced into small ships on account of the extra staff required and men will still to a great extent be able to choose their own diet. The average Britisher is conservative and if left to himself feeds very much the same in whatever part of the world he happens to be. Obvious deficiency diseases are now a rarity in the Service. Only one case of scurvy has been reported during recent years, and that, curiously enough, was in an individual serving in a large ship in which the diet was ample and varied. Presumably the man was a faddist who did not take advantage of the food offered him. Incidentally, the Service lime juice, made from West India limes, has no antiscorbutic properties, and the Admiralty have under consideration the issue of orange juice made from Californian oranges.

Cases of beri-beri still occasionally occur, especially among native races in the Persian Gulf. Beri-beri is not the only result of vitamin B deficiency, and indeed it is now suggested that this disease is of microbial origin acting on an individual who is rendered particularly susceptible by lack of the vitamin. Lack of vitamin B is said to make an individual more liable to pyogenic infections, leads to dilatation of the stomach and atony of the gut, with consequent constipation and intestinal toxæmia. These are just the conditions common in men serving in hot climates. I submit that this question of possible vitamin B deficiency should be borne in mind by medical officers. The food question concerns all three Services. We do not want to become food cranks, but the tastes of the individual should be studied as much as possible, and those responsible should not be content merely to provide a strictly scientific diet, for this may be monotonous or even unpalatable.

A short time ago I was sent to the Red Sea to investigate local conditions and to report on the health of ships' companies in the Red Sea sloops. The nosological

tables of these sloops over a number of years showed a surprisingly small incidence of disease which could clearly be attributed to climatic conditions. But the medical officers informed me that there was really a considerable amount of complaints of an indefinite nature. Constipation, insomnia, anorexia, irritability and general lassitude, are worrying to the individual, though he may not run to the sick bay for advice. The Admiralty realize the trying conditions in a small ship in hot climates and have ordered that men are not to serve in such situations for more than two years at a time. These small ships usually refit twice a year, and opportunity is taken to send men to recuperate in rest camps in a more healthy climate.

Numbers of excellent papers on pulmonary tuberculosis have been written in recent years. It is often asked why a carefully selected body of men, well fed, well clothed and generally well looked after, should have a relatively high incidence of this disease, especially as there has been a progressive fall in the civilian rate for many years.

I do not wish to trouble you with elaborate statistics, but the following figures are instructive.

Naval.				Civilian.			
Population	Year	Cases	Rate per 1,000	Population	Year	Cases	Rate per 1,000
112,700	1909	320	2·82	35,424,000	1909	Not known	
126,830	1913	245	1·93	36,514,000	1913	81,159	2·20
110,700	1921	264	2·38	37,887,000	1921	59,299	1·57
89,950	1925	240	2·76	38,830,000	1925	58,545	1·51
90,650	1926	187	2·06	39,067,000	1926	56,212	1·45

The naval figures are not really comparable with the civilian rates, which include all ages and both sexes; again, all proved cases of pulmonary tuberculosis in the Navy are eventually notified, while in civil life a very appreciable proportion escape notification.

The annual report for 1927 of the Chief Medical Officer to the Ministry of Health states that: "Notification is, on the whole, more effectively carried out year by year, but the reports of medical officers of health indicate that in some areas there is still considerable room for improvement since their figures show that many deaths from tuberculosis had occurred amongst cases not previously notified." The report gives concrete examples—e.g., in one urban district 41 per cent. of cases of pulmonary tuberculosis have not been notified during life and in a rural district the rate was as high as 56 per cent. Such a failure of notification, even if exceptional, must, to a great extent, detract from the value of these statistics. A comparison of the naval and civilian mortality rates is of no value, as naval cases are invalided out of the service at an early stage of the disease.

Various naval medical officers have endeavoured to prove that the complaint is most common in one class of ship, in one particular age group, or in one particular branch. The actual number of cases in the Navy is relatively small (in 1926 it was 187) and so statistics are not of much value, and up to now I do not think any useful information has been gained along these lines.

One of the most important preventive measures is early diagnosis, and I think that the majority of naval cases are diagnosed, or at any rate regarded with suspicion, in a fairly early stage. But early diagnosis needs adequate machinery for keeping in touch with the men. In the Navy, especially in the big home establishments and in the Atlantic Fleet, the personnel is constantly changing, and it is difficult for the medical officers to keep track of everyone. This is more difficult ashore than afloat, although a big ship may have a complement of over a thousand men, some of whom work in isolated positions and are never seen by the medical officer, even at Sunday divisions. These men often sleep in badly ventilated compartments, keep very much to themselves and escape observation.

It is just the early doubtful case with which we want to get into touch, the man who is progressively losing weight for no obvious reason, who is off his food and gets easily tired, who has a poor chest expansion, and who may even be found to run an occasional evening temperature. Unfortunately such types often do not voluntarily come to the sick bay for advice. They may have no cough and suffer no pain, but just feel run down and think that the next leave period will put them right.

Every man in a ship is now required to have his chest examined at periodical intervals. A ship is a noisy place and abnormal early physical signs are difficult to detect, and, anyhow, suspicious cases should be brought under observation before definite signs are apparent. I think in practice that most medical officers take the opportunity of stripping newcomers to the sick bay, whatever is the matter with them, and assessing their fitness more by questioning and general inspection than by auscultatory signs.

Another valuable help in early diagnosis is the quarterly weighing of all ratings under 36 years of age, if it is carried out conscientiously. This weighing is done by the physical training instructor, the men being dressed in a light vest and pants. The results are sent to the sick bay and if a man has lost weight for no obvious cause he can be kept under observation. When such a suspicious case is detected, he is usually sent to a well-equipped naval hospital at an early date. The medical officer of his ship may label him "Observation T.B." or he may be sent in with an indefinite diagnosis which is obviously provisional. In hospital, he is carefully overhauled and watched. An X-ray examination may clinch the diagnosis but I am informed by an experienced radiologist that in very early or suspicious cases a skiagram or screening is often unsatisfactory. Under appropriate hospital routine such a man often makes marked improvement and is returned to duty. He may relapse sooner or later and end his service career as an undoubted case of pulmonary tuberculosis or he may survive to draw his pension.

The ultimate disposal of this type of suspicious case must be equally difficult in the Army and R.A.F.; in the Navy, if a case is definitely diagnosed the patient can be retained in a naval hospital until sanatorium treatment is arranged. The nature and duration of such treatment depends on the resources of the local authority who receives the case, but at any rate the Navy has done what it can do to give the affected man as fair a start as possible under civilian control.

We naval doctors would like to give every suspicious case as indicated above, the benefit of sanatorium treatment, for we know that it is just this type of man who would have a good chance of being put on his feet and be enabled to become a useful member of the community.

Financial considerations and lack of beds preclude most local authorities from receiving such doubtful cases into their sanatoria and unfortunately we have no naval sanatorium in which we can treat them ourselves. We are between the devil and the deep sea. If we return a suspicious case to duty, although he may have no sputum, he is undoubtedly a potential danger to his messmates and has to be kept under very careful supervision, which is difficult for the reasons indicated above. If he is invalided with some such diagnosis as pulmonary fibrosis, it usually entails great hardship: he is cast on the labour market in a poor state of health, frequently without pension and with the prospect of finding it difficult to obtain suitable employment. These cases give the medical officer considerable anxiety and it is inevitable that errors of judgment should occur. I will conclude by quoting two instances from my experience in which the subsequent histories show the initial difficulty of decision.

(1) An officer was invalided, much against his will, for early pulmonary tuberculosis. His case was very carefully gone into and, though T.B. was never demonstrated, all the members of the survey board agreed that his retention in the

Service was undesirable. He had several months' sanatorium treatment and, as was to be expected, his condition improved greatly. On discharge from the sanatorium he consulted a specialist who assured him that he had had no signs of pulmonary tuberculosis. Pressure was brought to bear and he was readmitted into the Service labouring under the firm conviction that the members of his board were ignorant and had treated him with scant consideration. He is still serving and as far as I know fit and well four years later. Now, if this individual had been a rating, I do not think it would have been possible for him to be re-entered.

(2) I myself sent a warrant officer into hospital suffering from an attack of pleurisy, the second within nine months. He was returned to his ship a few weeks later. Another warrant-officer lodged a complaint because he was required to share a cabin with this individual, and I think there was a great deal of justification in the objection.

Fortunately for the dispute, the pleuritic patient in a few days' time, sustained a slight injury so that I was able to send him into hospital once more and his relief in the ship was arranged for. Very shortly after admission to hospital pleurisy again developed but this time accompanied by purulent sputum in which tubercle bacilli were demonstrated. He was, of course, invalided.

Discussion.—Major-General H. P. W. BARROW (after responding to the request of Surgeon Commander Rivaz for details of the army system of victualling in the tropics) said that it was quite possible that certain conditions constantly met with (e.g., neurasthenia, indigestion) might have as a basis a vitamin B deficiency. He would also draw attention to the necessity for the adequate ventilation of troopships and the importance of suitable clothing.

Fleet-Surgeon W. E. HOME said that too little had been written on the subject of naval hygiene. Overcrowding was defined by Sir Thomas Fraser in his "Report on Plague," as a "function of the number of persons occupying a space and the efficiency of the ventilation of that space, and when he (the speaker) had gone to Hong Kong in 1900, in thirty-one days he had found that illness increased among those men who had only 100 cubic feet of space, while those who had 150 cubic feet escaped. It was difficult to get cases of tuberculosis in an early stage. For example: A man had joined the "Excellent" from a destroyer, in which he had suffered for two months from a cold. He had a hæmorrhage, and then complained for the first time; tubercle bacilli were found that day in his sputum. He was at once sent to Haslar, thence, by the kindness of the King, to Ventnor, and afterwards to Brompton; but, though taken in hand so quickly, he never made a complete recovery.

Section for the Study of Disease in Children.

[*November* 23, 1928.]

Nutritional Anæmia in Infancy: Some Observations on a Common Deficiency Disease.

By Helen M. M. Mackay, M.D.

Abstract.—The prevalence of anæmia among London infants is shown by an investigation extending over the last three years and comprising about 770 cases with 3,100 hæmoglobin estimations. This anæmia is largely nutritional in origin and is due to a deficiency of iron, possibly associated with other factors. It is known that an infant obtains its necessary iron not only from its milk, which is poor in iron, but also from a store in the liver. Our results lead us to suppose that this store may be unduly low in many London infants. The average hæmoglobin percentage in the blood of fairly healthy artificially-fed London infants shows a sharp fall from its high level at birth to about 65 per cent. at 2 to 3 months of age, a rise to about 70 per cent. by 5 to 6 months, and then a steady fall reaching about 65 per cent. at 12 months. The continuous administration of iron, beginning when the infant is under 2 months old, raised the average hæmoglobin percentage to 80 per cent. at 4 months of age and onwards. Infants whose birth-weight is under 6 lb., twins and premature infants are specially in need of iron treatment. The selection of the iron salt is of importance, and iron and ammonium citrate proved effective. It was satisfactorily administered on a large scale by giving it incorporated in a dried milk.

No evidence was obtained that want of light is an etiological factor in anæmia in infancy, and artificial light therapy did not cure it. No vitamin deficiency appeared to be involved. The value of liver treatment is still under investigation, but the results so far obtained can be explained on the assumption that liver treatment is efficacious only because it provides a source of iron. The routine administration of an iron salt to artificially-fed infants is advocated on the ground that, by preventing the anæmia from which most of them would otherwise suffer, the level of health of our infant population would be raised.

Anæmia in babies is so common in London that I hope an investigation into this condition may be of interest. Judging from our work extending over the last three years and comprising about 770 cases with 3,100 hæmoglobin estimations, this anæmia is chiefly nutritional in origin and is mainly due to a deficiency of available iron [1].

The Iron Requirements of an Infant.—Webb Hill [2], in an interesting paper, has attempted to estimate the daily iron requirements of a growing baby. He estimates it under two heads: (*a*) *Growth.*—For increase in blood volume—and consequent increase in hæmoglobin—with growth, he calculates that an infant half to six months old requires 0·8 mgm., an infant six to twelve months old, 0·4 mgm. of iron daily. If this iron is derived from the diet, another 40 per cent. must be added to allow for incomplete retention.

(*b*) *Metabolism.*—The amount required for metabolism apart from growth he places at 1·5 mgm. daily for a 20-lb. infant.

Taking these figures as our basis, let us suppose that a baby weighing 10 lb. requires for metabolism say 0·7 mgm. (i.e., about half the requirement of a 20-lb. baby), and for increase in blood volume 0·8 mgm., then the total daily requirement would be 1·5 mgm. If such an infant consumed 1¼ pints of human milk per diem, the diet, taking Webb Hill's figures, would supply him with perhaps 0·4 mgm., or about one quarter of his daily needs, even if there were 100 per cent. retention. The same quantity of cow's milk, which could seldom be given to so young an infant, would provide, say, 0·12 mgm. The difference between the child's iron requirements and its iron intake in the food should normally be made good by the iron store in the liver at birth. I hope to show that in a large proportion of infants in London, this difference is for some reason not made good, and the infants become anæmic for want of iron.

The Percentage of Hæmoglobin in the Blood of London Infants.—First, I propose to consider the average hæmoglobin percentage in the blood of fairly healthy, artificially fed infants in London—infants of the type met with in welfare centres. Chart IA shows the hæmoglobin level at each month of age for the first twelve months of life. As will be seen, there is a sharp fall from its high level at birth to about 65 per cent. at two to three months of age, a rise to about 70 per cent. at five to six months of age, and then a steady fall through the rest of the first year, reaching about 65 per cent. at twelve months. So far as I have been able to discover, this double drop during the first twelve months of life has not previously been pointed out. These data have, for the most part, been collected in the Out-Patient Department of the Queen's Hospital for Children, the blood estimations being made by Miss Lorel Goodfellow. We are now engaged in collecting similar figures for healthy *breast-fed* infants attending the Mothers' Hospital, Clapton. The figures we have so far obtained for breast-fed infants may need modification when larger numbers have been collected, but those now available indicate that their hæmoglobin level is very near that of the artificially fed group.

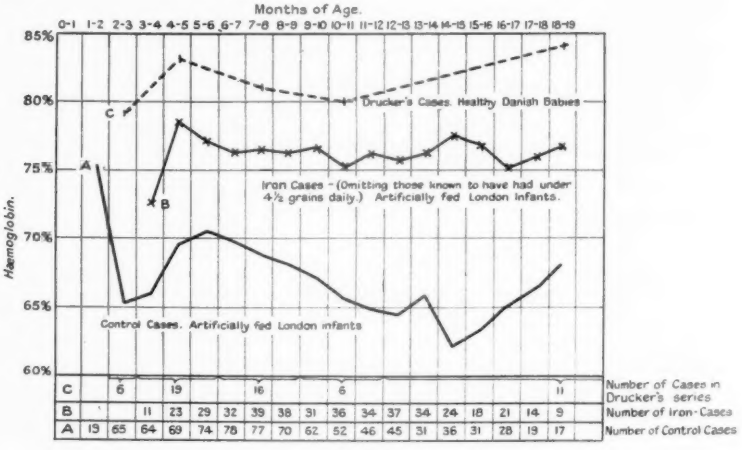
Months of Age.
0-1 1-2 2-3 3-4 4-5 5-6 6-7 7-8 8-9 9-10 10-11 11-12 12-13 13-14 14-15 15-16 16-17 17-18 18-19
85%
80%
75%
70%
65%
60%
Hæmoglobin.
Drucker's Cases. Healthy Danish Babies
Iron Cases - (Omitting those known to have had under 4½ grains daily.) Artificially fed London Infants.
Control Cases. Artificially fed London infants
C 6 19 16 6 11 Number of Cases in Drucker's series
B 11 23 29 32 39 38 31 36 34 37 34 24 18 21 14 9 Number of Iron-Cases
A 19 65 64 69 74 78 77 70 62 52 46 45 31 36 31 28 19 17 Number of Control Cases

CHART I.

The hæmoglobin level in the blood at different ages.

Evidence of Iron Deficiency and its Prevalence in London.—The same chart (1B) demonstrates the effect of iron treatment on the hæmoglobin level of artificially fed infants, and shows that the average hæmoglobin percentage from four months of age and upwards was raised by this means to over 75 per cent. It should be noted that treatment was begun at all ages of infancy, so that this chart does not represent the result of continuous iron administration from the first months of life. The great prevalence of anæmia among London infants is indicated by the large proportion of babies, among an unselected group, who were benefited by iron treatment. Eighty-one per cent. of all artificially fed infants to whom iron was given showed some increase in their hæmoglobin percentage, whereas only 19 per cent. showed no increase. The same facts are also brought out by comparing the number of infants whose hæmoglobin reached 80 per cent. in the control group and in the iron group—after two months of age only 7 per cent. of the untreated cases reached this level, whereas 81 per cent. of those known to be receiving an adequate amount of iron did so. I am personally of the opinion that iron therapy is of value for anæmia in children only where there is an actual deficiency of iron for blood regeneration,

either because the diet relative to the need is deficient in this respect, or because the tissues are depleted of their normal store. If this be so, then our results show that such a deficiency is extremely prevalent at any rate in London, and judging by their appearance, I do not suppose that babies in other parts of the British Isles are any better off in this respect.

The Hæmoglobin Level of Babies in Some Other Countries.—Comparison of our figures with those of investigators in other countries, however, leads one to conclude that certain other countries may have an advantage over us in this connexion. For instance, it would seem that babies in some Danish cities have a much better hæmoglobin level. Drucker [3] has published figures for healthy infants in Copenhagen showing that their hæmoglobin percentage remained about 80 per cent. from 2 to 12 months old. A curve drawn from his figures is also shown on Chart 1c. His estimations were made from a prick in the heel, as was done in our case, so that the higher readings usually obtained from a prick in the ear [1 and 3] do not account for the difference between our figures. It is true that his infants were specially

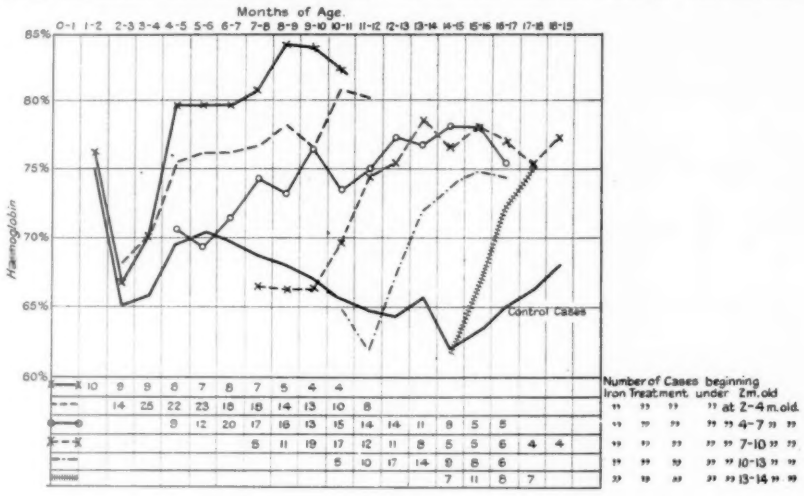

CHART II.

The effect of iron treatment. Infants grouped by age at which treatment was begun.

selected as healthy. Again Appleton [4], in America, found averages of 88 to 87 per cent. for selected healthy infants aged 3 to 11 months. The difference in our figures may possibly be due to a larger iron store in the livers of Danish or American babies resulting from a more satisfactory ante-natal maternal diet. I hope to obtain some information on this point by treating mothers during pregnancy. Meanwhile, animal experiments appear to support such an hypothesis. For example, young rats [5], or mice [6], or pigs [7], develop anæmia on a milk diet, if the mother has previously been deprived of a sufficiency of iron in her diet, and the liver store of the fœtus is thus rendered inadequate. If the mother has had an adequate diet, the iron store proves sufficient to protect the young from anæmia even if their diet is very poor in iron.

Age of Onset of Symptoms of Iron Deficiency and Predisposing Factors.—If it be accepted that our figures demonstrate that iron deficiency is a prevalent cause of anæmia in infancy in London, it is desirable to know at what age this deficiency is manifested. Chart II shows that fairly healthy, bottle-fed babies were anæmic

by 3 to 5 months old, and that this anæmia could be prevented by giving iron. Infants given iron from under 2 months old averaged about 80 per cent. hæmoglobin from 4 months onwards, whereas the controls averaged 70 to 65 per cent. So far we have not been able to reduce the drop in the first two months of life, and our results throw no light on the cause of this drop.

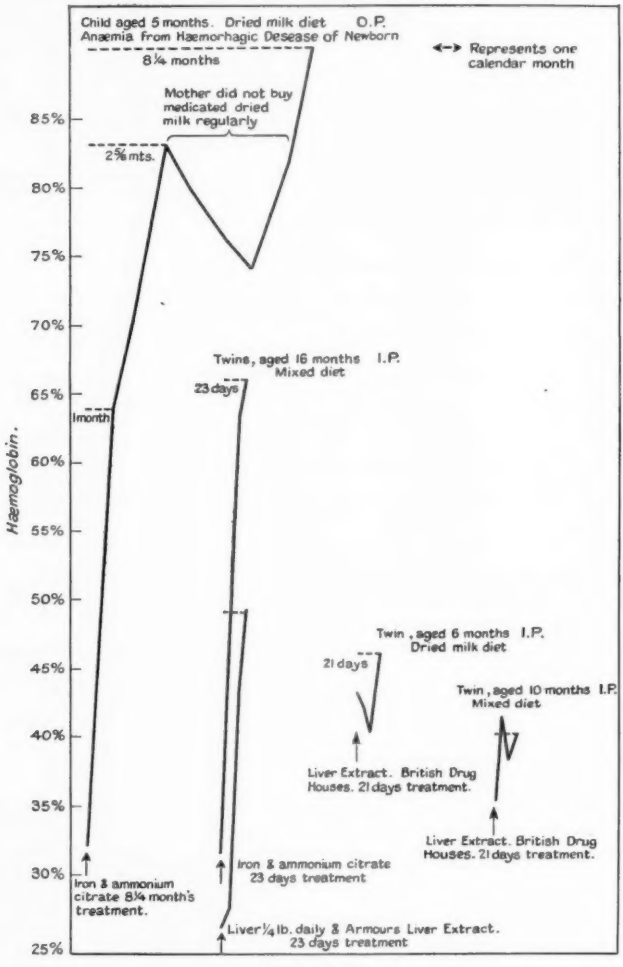

CHART III.

Individual case charts, showing hæmoglobin level after varying periods of treatment.

Our figures are derived from infants who, for the most part, were of normal birth weight. Most babies, other than those suffering from a pyogenic or blood infection, who are found to be profoundly anæmic, are either premature infants or small twins. Such infants, as a result of a very small ante-natal store of iron in the liver, invariably suffer from marked iron deficiency. Of course, there are

many other causes of profound anæmia, but they are relatively uncommon. In all, twenty-seven infants in our series had a hæmoglobin percentage of less than 50 per cent; of these nineteen were twins, or were under 6 lb. at birth, four were over 6 lb., and of the remaining four the birth weight was unknown.

Any young child, of course, can be rendered profoundly anæmic by continuing a milk and polished cereal diet for long after infancy, and although this important fact has been known for many years, it is too frequently forgotten by those giving directions to mothers regarding feeding. I usually aim at getting an infant on to a mixed diet by 8 to 9 months of age.

May I also draw attention to the need of providing a liberal supply of iron for blood regeneration to any infant who, for one reason or another, either as a result of bleeding or infection, has suffered hæmoglobin loss and consequent rapid depletion of his iron store. Chart III shows the response to iron treatment obtained in an infant aged 5 months who had lost blood in the first week of life as a result of hæmorrhagic disease.

The Technique of Administration of an Iron Salt.—The iron salt used in our work was iron and ammonium citrate, and the best way to administer it on a large scale proved to be the use of a dried milk in which the salt was incorporated before drying. The West Surrey Central Dairy Co. ("Cow & Gate" firm) prepared such a milk at my request, and have now placed it on the market. By this means much more regular administration was ensured than when the salt was ordered as a medicine. Iron and ammonium citrate is well tolerated and has no ill effects on a baby of fair digestive powers if it is carefully used. The infants in our series have usually had 4½ to 9 gr. daily. This quantity, given to an infant unaccustomed to it, may cause loose stools, and it is, therefore, always best begun gradually. When using dried milk containing iron, my usual method has been to ask the mother to give equal parts of ordinary and medicated dried milk for some days before putting the infant entirely on to the medicated milk. If the iron was given as a mixture, the mother was advised to give it in the feeds, beginning with a small dose. I have occasionally given iron to a baby as young as one week old. One need not fear constipation as a result of this treatment—constipation was just as common in our control group as in our iron group.

The particular iron salt selected appears to be a matter of considerable importance, and interesting work has been done on this subject by Mitchell and Vaughn [8]. For example, iron and ammonium citrate, ferric citrate and ferric chloride are placed by them among the salts giving good results in nutritional anæmia; ferrous carbonate, ferrous sulphate, as well as ferric oxide and reduced iron, on the other hand, gave very poor results.

Factors other than Iron which may influence the Anæmia described.—Whether this nutritional anæmia in infants is due to iron deficiency alone, or whether iron deficiency is only part of the cause, is, I think, open to question. The remarkable results obtained by Hart and his associates [9], as well as by McHargue [10] and his fellow workers, prove that both iron and copper are necessary to cure and prevent anæmia in their rats on a milk diet, and interesting figures are available showing that just as there is a store of iron in the liver at birth, greatly in excess of the percentage present in the adult's liver, so there is a similar store of copper in the liver of calves [11]. Young rats at birth contain nearly double as much copper as adult rats, the copper being estimated per kilo of dried weight. Young guinea pigs, which begin mixed feeding from birth, have no iron store, and, presumably also, from McHargue's results, no copper store [11]. The iron salt we used was found by Professor Hart to contain a very small quantity of copper (1·7 mgm. per 100 grm.). The dried milk containing iron and ammonium citrate contained per kilo about 0·4 mgm. more copper than the untreated dried milk. We have no evidence to show whether this had any bearing on the results obtained.

Comparison of the hæmoglobin levels in summer and winter, or of cases treated with artificial light and a control series, gave no evidence that light, or the want of it, played a part in the production of this anæmia [1]. Both iron cases and controls received a similar diet, with similar quantities of vitamins, so that there is no evidence that any vitamin deficiency was a material factor in the anæmia with which we are dealing. Moreover, I had previously found that anæmia was no more marked in a group of infants in an institution abroad who were on a diet deficient in vitamin D and developed rickets than in a similar group protected from rickets [12], and in my experience the cure of rickets with cod-liver oil or light treatment does not cure anæmia.

The reason for the efficacy of liver treatment in pernicious anæmia has not yet been elucidated, and its value in other forms of anæmia is still under investigation. The work of Hart and his associates [9], as well as the recent paper by Whipple and Robscheit Robbins, indicates that the inorganic fraction of liver is potent in some forms of experimental anæmia. As regards the alimentary anæmia now under consideration, liver treatment proved effective in the few cases in which I used it, as shown in Chart III. My numbers, however, are not sufficient to enable me to offer an opinion as to whether the rapidity of response is usually greater with liver treatment or with iron and ammonium citrate, but it seems not improbable that liver treatment is efficacious in this form of anæmia only because it provides the necessary iron. A liver extract, potent in pernicious anæmia, gave a negative or doubtful response in two cases after three weeks' administration (Chart III), but such an extract is almost free from iron. Liver, or liver extract, coupled with iron medication did not appear to hasten the cure in the few cases in which we tried it, but this requires further investigation.

The response to iron treatment varies greatly according to whether one is dealing with a simple nutritional deficiency or whether there are complicating factors at work. For example, the presence of an otitis media or of some septic infection introduces another factor, and such cases, as one would expect, do not respond quickly to iron treatment. If such an infection supervene in a baby already suffering from nutritional anæmia, the administration of iron is certainly an important element in treatment, for there is little doubt that resistance to infection is lowered by iron deficiency.

Summary of Conclusions.—To sum up the practical results of this investigation, I think you will agree with me that they indicate the wide prevalence of nutritional anæmia, the efficacy of iron treatment and the desirability of its prophylactic administration on a large scale to artificially fed infants. I consider iron therapy should be begun in the earliest months of life and continued throughout the first year, and possibly longer. Such treatment is particularly desirable in the case of twins and all premature babies and those whose birth weight is below 6 lb., as all such infants are likely to suffer from iron deficiency in a severe form. Iron and ammonium citrate is an efficient salt, and can be most conveniently given to bottle-fed infants incorporated in dried milk. It should be begun gradually. If the administration of iron to infants in this way becomes as much a routine measure as giving cod-liver oil or fruit juice, our results go to show that it will definitely improve the health of London infants and increase their resistance to disease.

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Discussion.—Dr. F. J. POYNTON (President) said that the paper raised an even wider question than that dealt with by its reader, and that was whether the use of dried foods, now so prevalent, would eventually have some effect on growing children, at present not foreseen. It seemed clear that these foods did not produce gross effects such as scurvy, or rickets, but they might reduce the power of resistance to disease later on.

Dr. KINGSTON BARTON said that often iron and ammonium citrate failed, though it was one of the most successful combinations for the treatment of anæmia in children. When a child in this condition did not respond to iron and ammonium citrate, the late Dr. Gee (a recognized authority on children's diseases) used to insist on the great value of reduced iron.

Dr. LESLIE WITTS said that at the London Hospital a study was being carried out on the anæmias which occurred in women. Sir George Newman had stated that anæmia in women was disappearing. That statement might apply to chlorosis, but he (the speaker) did not think it applied to the anæmia of pregnant women. At the London Hospital there had been collected a number of cases of profound anæmia in women at or about childbirth and during lactation. He would be much interested to hear whether Dr. Mackay knew anything of the frequency of anæmia occurring in the mothers of the anæmic children she had described. Dr. Mackay had quoted the investigation of anæmia occurring among sucking-pigs. Pigs were often fed on cotton-seed meal, a food which contained scarcely any iron, and the progeny of sows so fed became very ill three weeks after birth. They could be restored to health by the administration of iron. But by giving iron to the sows, disease in the offspring could be prevented. Some of the mothers studied at the London Hospital were found to have been living on extraordinarily poor diets during their pregnancy; in many cases they ate meat only once a week, and in the interim most of their food consisted of bread-and-jam, tea and some cereals. On such food it was impossible that the mothers should be supplied with a store and a reserve of iron, to enable the infant to embark well on a normal life. Already so many treatments—including the taking of iodine and more vitamins—were recommended to mothers during their pregnancy that he hesitated to add to them; but, whenever an adequate diet could not be afforded, it seemed reasonable to prescribe iron.

Dr. MACKAY (in reply) said that some figures concerning breast-fed babies had also been collected, and though as yet the number of these did not justify any dogmatic statement they gave a curve very similar to that of artificially-fed children. If an extended series should confirm this, then one could not attribute this form of anæmia to the use of dried milk; some other factor must be at work, and this she believed would very probably prove to be lack of iron in the diet of the mother, as suggested by Dr. Witts. Efforts were being made to get a group of mothers for investigation, so that definite evidence on that point could be obtained. That, however, was far from easy to accomplish. [Dr. COCKAYNE: Were the children referred to in the paper those attending a welfare centre, or children who were attending some hospital on account of illness?] The children were, for the most part, attending the Queen's Hospital for Children, to which they were brought in the first instance generally for minor complaints, such as constipation, bronchitis, etc., or for failing lactation in the mother. She kept them under observation in the Out-patient Department for the purpose of making the investigations detailed in the paper. The type of case was therefore that seen at welfare centres.

Two Cases of Suprapituitary Tumour (Adamantinoma).—W. G. WYLLIE, M.D.—(I) R. S., girl, aged 10 years, has had headaches—chiefly frontal—and shaking of the hands for twelve months; has also become increasingly fat. Present weight, 75½ lb. (normal for age, 64 lb.). No visual complaint, or increase of thirst or of micturition. Circumference of head, 22½ in. Obesity is obvious. Fingers tapered, hair thick and silky. Both discs show papillœdema. No evidence of bitemporal hemianopia by rough tests (perimeter charts not done). Child is tremulous in walking and in use of hands. Skiagram of skull shows advanced erosion of sella turcica.

(II) F. H., girl, aged 21 years, has always been fat; was nicknamed "Fatty" at school, but excessive obesity developed at age of 15 years. Present weight is 12 st. 11 lb.

Menstruation began at age of 13; was absent between 15 and 17, has been regular since.

Headaches, at first associated with nausea, vomiting and giddiness, commenced at age of 15, but lately have been only slight. There has been no polydipsia, or polyuria; no complaint of impaired vision.

Patient is very obese but shows no sexual dystrophy, and distribution of axillary and pubic hair is normal. Both discs show secondary optic atrophy with a slight degree of swelling. There is some restriction of the visual fields, most marked in the temporal halves. Knee- and ankle-jerks are normal, but the plantars tend to give an extensor response. Fasting blood sugar is 0·102 grm., and after 50 grm. glucose there is a delayed return to the fasting level, but without any glycosuria.

Skiagram of skull shows marked erosion of sella turcica, with an irregular shadow above (calcareous portion of the tumour).

Hemiplegia Noticed after Diphtheria.—D. W. WINNICOTT, M.R.C.P.—B. R., girl, aged 2 years and 5 months, healthy except for left hemiplegia.

At age of eight months she caught nasal diphtheria from her brother. She was in a fever hospital for six weeks, but no serum was administered. When the parents took her away they noticed that her face was weak on the left side and that she avoided using the left hand. The left hand movements were very clumsy.

At about 2 years she walked, but with left leg weakness. The degree of paresis has diminished steadily since the onset.

Present condition.—*Face:* The left weakness is now only made out with difficulty.

Upper Limbs.—No spasticity, no wasting, no atonicity of the affected side; limbs equal in length. The left hand is used less than the right and the movements of the hand and fingers are clumsy. Reflexes symmetrical.

Lower Limbs.—No spasticity, no wasting, no atonicity of the affected side; limbs equal in length. Reflexes symmetrical and normal. Left foot drop.

Gait.—Is that of a hemiplegic, with circular movement of the point of the weak foot (left).

Dr. J. D. ROLLESTON said that cases of hemiplegia occurring after diphtheria were decidedly rare. During the first four years of his connexion with fever hospitals he had never seen a case of the kind, nor, indeed, had he heard of one, and when he wrote his thesis for the M.D. degree in 1904—on diphtheric paralysis[1]—he had made no allusion to it. But in the year 1905, he had a case of hæmorrhage occurring after a very severe faucial attack[2], and he then found that the first case had been described by Sir William Gull, who was one of the first in this country to deal with the nervous complications of diphtheria. Since 1905 he (the speaker) had seen about a dozen cases of the kind, all of them after severe faucial dipththeria[3].

Various explanations had been submitted as to the pathogenesis of hemiplegia. The most satisfactory was that it was an embolic change, secondary to cardiac thrombosis. In the cases which came to autopsy one found evidences of embolism elsewhere: renal infarction, or splenic infarction, or occlusion of arteries in the limbs. What had puzzled him was, why one did not meet with hemiplegia more frequently, when the heart, especially in the severe cases, was so often attacked.

In the present case he doubted whether the diphtheria was in any way responsible for the hemiplegia. In his experience, purely nasal diphtheria did not tend to affect the heart in the same way that faucial diphtheria did. He would regard the hemiplegia in this case as *post* rather than *propter* the attack of diphtheria.

Mongolism with Enlarged Liver and Jaundice.—RONALD CARTER, M.D.—E. R., aged 2 years, brought to hospital suffering from jaundice.

Family History.—Parents young and healthy. Father when two years old suffered from jaundice, which lasted some time and was nearly fatal. No other history of jaundice in the family. Patient is only child; mother had no miscarriages.

1 *Practitioner*, 1904, lxxiii, 614.
2 *Rev. Neur. and Psych.*, 1905, iii, 722.
3 *Clin. Journ.*, 1913, xlii, 12; *Rev. Neur. and Psych.*, 1916, xiv, 145.

Previous History.—Patient said to have had jaundice for first 3 weeks of life.

Condition on Admission.—October 25, 1928.—General aspect of mongol, but tongue not enlarged; finger-tips broad, and little-finger does not curve toward ring finger; well-nourished; cheeks high-coloured; 13 teeth; marked jaundice. Stools normal in colour and no bile present in urine. Liver enlarged to 3 finger-breadths below costal margin; spleen not enlarged. Wassermann reaction negative. Fragility of blood-corpuscles normal. *Blood-count:* red cells 5,168,000; leucocytes 5,600; polymorphonuclears, 8 per cent.; lymphocytes, 87 per cent.; transitionals, 1 per cent.; large mononuclears, 4 per cent.; platelets scanty; slight anisocytosis.

November 5, 1928.—Jaundice completely cleared, but liver remains the same size; child is otherwise quite well; takes an interest in surroundings.

The history does not suggest ordinary catarrhal jaundice; there was no fever or vomiting at any time, and I do not know whether any other cause can be given for the liver enlargement.

Discussion.—Dr. COCKAYNE said that the age of this child was unusual for catarrhal jaundice, but this was the appropriate season for that disease, and it was very common this year. From the clinical standpoint the fact that the stools were normal in colour, and that there was no bile in the urine, was very much against a diagnosis of catarrhal jaundice.

Dr. B. SCHLESINGER said that recently, during an epidemic of catarrhal jaundice, he had seen several cases in which the jaundice was not very well marked, being often evident only for a day or so, and in which the liver, and in many cases the spleen, were distinctly palpable. Though in a number of these the jaundice had subsided within forty-eight hours, the liver remained enlarged for several weeks. He thought that in these epidemics the whole reticulo-endothelial system was involved; and that this was the reason the liver and spleen remained enlarged for some time.

Unilateral Hypertelorism.—R. C. LIGHTWOOD, M.D. (for Dr. W. P. H. SHELDON). *Family History.*—W. B., a girl, aged 8 years, is the youngest of five children, the others are of normal appearance. The first is a girl of 23; the next a boy of 21, said to have renal trouble; the third (a girl) died from pneumonia; the fourth (a girl of 15) is of small stature, but otherwise normal. This child was born when her mother was aged 42; although labour was prolonged no instruments were employed; there have been no miscarriages. Mother's and father's families are both normal.

W. B. was brought to hospital because she held her head on one side. Torticollis, probably of ocular origin, was discovered.

Facial Appearance.—Upper part of face on right side is flattened and right supra-orbital margin is under-developed; right eyebrow somewhat higher than left; and right palpebral fissure wider than its fellow. Right orbit displaced outwards and a little upwards; consequently the nasal bridge is wide; distance between inner canthi 34 mm.

Description and Measurements of Head.—Maximum horizontal circumference, 514 mm.; greatest length, 197 mm.: greatest parietal breadth, 168 mm.; cephalic index, 85.

The skull is therefore brachycephalic. Well-marked flattening of occipital region on right side, but none on left.

Ophthalmologist's Report (Dr. T. H. WHITTINGTON).—Compound hypermetropic astigmatism, for which patient wears glasses. Alternating concomitant external strabismus, and she does not fix a near object with both eyes. The right eye tends to diverge the more with the screen test, and turns a little upwards as well as outwards, but is the fixing eye of choice under ordinary circumstances. Vision of left eye is worse, and tendency to turn face to left and tilt the head to left is probably associated with faulty vision and defective muscle balance. Discs normal.

General.—Nervous system and visceral organs normal. Slight webbing of second and third digits on each foot. Suggestion of incurving of the little fingers, but neither the hands nor the feet are mongolian in character. Slight backwardness is reported by her school teachers, but the Binet-Simon tests do not confirm the report.

Discussion.—Dr. F. PARKES WEBER said he would like to suggest that if the diagnosis was correct, the heading should be altered to "Asymmetrical Hypertelorism." Or it could be called "Hypertelorism with asymmetry of the parts involved." He assumed that every care had been taken to avoid any mistake in diagnosis, namely, to exclude some form of local hyperostosis in the orbital cavity at that site. He presumed there was no evidence of congenital syphilis, which might cause asymmetrical ossifying periostitis.

Dr. W. P. H. SHELDON in reply to Dr. Weber, said that the difficulty about calling the condition "asymmetrical hypertelorism" was that in hypertelorism there were various abnormalities in the skull, in addition to those of the eye. "Unilateral hypertelorism" was intended to signify that all the changes were confined to one side of the skull. As far as possible, the other conditions suggested by Dr. Parkes Weber had been excluded.

Cystic Hygroma in a Mongol.—R. S. CORBETT, F.R.C.S.—Girl, aged 11 months, first seen at St. Bartholomew's Hospital on February 13, 1928.

Brought on account of swelling on left side of neck. Full term child; pregnancy was normal, labour normal; breast-fed every four hours, sleeps and takes her feeds well.

Family History.—The mother is a healthy woman, aged 39. She has had five children in fourteen years and they are stated to be normal and in good health.

Swelling was noticed at birth as a fullness below the jaw on the left side.

The child was a healthy looking baby, weight 7½ lb.; mongolian type.

Swelling diffuse and ill-defined; skin over it pale, surface smooth; it is of an elastic consistency, but not tense.

In the submental region is another swelling, oval in shape and well-defined, which suggests a lymph-node. No other abnormality.

A diagnosis of cystic hygroma was made and it was decided to wait until the child was weaned before any treatment was undertaken.

The mother thinks that during this time the swelling has varied in size but has never shown any inflammatory change.

There has recently been a marked increase in the size of the cystic swellings on the left side of the neck. The condition was originally confined to the submaxillary region, and showed well the multilocular type of lymphangioma. Recently extension has taken place to the posterior triangle of the neck, and illustrates the unilocular type.

The PRESIDENT said that operations on mongols were seldom satisfactory; he knew of no type of child who was more likely to die unexpectedly after an operation—even a simple adenoid operation—than a mongol.

Porencephaly with Myoclonus and Congenital Heart Lesion.—I. M. ALLEN, M.D., and C. WORSTER-DROUGHT, M.D.—D. G. H., male, aged 4 years, admitted to hospital September 27, 1928.

Present Complaints.—Unable to walk, to talk properly, or to feed himself; is not clean in his habits; is constipated.

Previous History.—Full-term child, weighing 7 lb. at birth; confinement was normal. When 10 months old he could not sit up; cut first tooth at age of twelve months; tried to crawl at age of 3½ years; has never been clean in habits and has always been constipated. Tonsils and adenoids were removed at the age of 3 years.

Family History.—Three other children, older than the patient, are healthy.

Condition on Examination.—Lies in his cot or sits up in a crouched position, leaning towards the right side. *Cranial nerves:* Can see people in the distance. Nystagmoid jerkings on looking to the left. Hears the sound of a tuning fork and responds by smiling. Speech: In reply to questions, uses a limited vocabulary

of simple words which are spoken indistinctly. Other cranial nerves normal. *Sensation:* Impossible to investigate in detail; pain sensation apparently normal. *Upper limbs*: Moves both arms freely and apparently aimlessly: tends to hold both arms in position of flexion; motor power and muscle tone poor; deep reflexes active on left side and not elicited on the right side. *Lower limbs:* Motor power and muscle tone poor; knee-jerks present and equal; ankle-jerks present and sluggish; plantar reflexes flexor. Abdominal reflexes present. *Gait:* Cannot walk. When child is held up, knees are pulled together, but when feet are in contact with floor they are placed wide apart.

Spontaneous Movements.—Sudden spasmodic movements of left side of face and of left upper limb, especially in region of shoulder, associated with sudden contractions of groups of muscle fibres. These contractions occur frequently without producing any movement of the limb.

Heart.—Apex beat in fourth left intercostal space inside nipple line; no thrill; soft systolic murmur replacing the first sound, heard best over the third left intercostal space, but audible over the whole precordium. Other systems normal.

Cerebro-spinal Fluid.—Normal: Wassermann reaction negative.

Mental.—Vacant appearance of face. Apparently understands some things which are said to him. Responds to questions by saying "No"; and, with encouragement, uses other words. On hearing sound of a tuning-fork responds by smiling. Cannot feed himself. Shows little desire for food. If not at once attended to, is incontinent. Is not able to make known his wants.

The interesting feature of this case is the association of mental deficiency and myoclonus confined to the left side of the face and the left shoulder with a minor congenital heart defect. The lesion giving rise to the nervous symptoms must be congenital and is probably more or less localized to the frontal lobe and the motor cortex on the right side. It may be of the nature of a local cortical aplasia or of cystic degeneration. The latter condition is suggested because of the unilateral nature of the defect.

Clinical Section.

[*December* 14, 1928.]

CASES.

Rifle Bullet in Brain for Thirteen Years.—C. WORSTER-DROUGHT, M.D.

—S. W. T., aged 32, complains of generalized headaches, occurring in frequent attacks and often lasting three or four days, weakness of the left arm and leg with occasional twitching of these limbs.

History.—While carrying ammunition at Suvla Bay, Dardanelles, he was shot by a Turkish sniper during the night of August 20, 1915. At the time he remembers feeling faint and dizzy, and has a vague recollection of being carried to the base. He has no memory for any events that occurred after this until he reached England some time in September, 1915. During this period, however, it appears that a superficial operation was performed on the left side of the head, in the region of the entry wound. He has no recollection of this operation. An X-ray report dated October 1, 1915, states, " Bullet lying transversely in middle cranial fossa."

He remembers that, on reaching hospital in England, his left arm and leg were partially paralysed, and that at times he relapsed into a stuporose condition—as he does not recollect certain visitors known to have visited him. The power of the left arm and leg gradually improved, and by April, 1916, he was walking about and using the left hand. By 1921 there was little sign of paresis in either limb, apart from changes in the deep reflexes. Towards the end of 1923, however, weakness of the left arm began to return, together with some unsteadiness on attempting to use the hand. During the last three years this deficiency of control over the left arm has gradually become worse and the left leg is also affected. The headaches have persisted throughout.

Condition on Examination.—Small non-adherent scar of entry wound in left temporal region; small gutter palpable in bone beneath, but no pulsation is felt and there is no impulse on coughing. Encircling this above is a 4-in. semicircular and linear operation scar.

Pupils, left larger than right, both somewhat dilated; left pupil reacts sluggishly to light and consensually, right normally; both react to accommodation and dilate to shade. Fundi normal. Visual fields full. Fine nystagmus on lateral deviation of eyes, more to left than right. No ocular palsy. (He complains of occasional diplopia on looking to the left and that things in this direction appear " uphill.") Other cranial nerves normal. Sensation: no abnormality. Left arm: general muscular weakness with increased tone and pronounced incoördination. Tremor of " cerebellar " type. Right arm normal. Left arm-jerks brisker than right. Abdominal reflexes: left absent, right normal. Left leg: decreased muscular power as compared with right leg, increased tone and slight incoördination. Knee-jerks and ankle-jerks: left increased and greater than right. No ankle clonus. Plantar reflexes: left unobtainable, right flexor. Romberg negative. Other systems normal.

X-ray Examinations.—October, 1928: Rifle bullet seen within cranial cavity, lying obliquely in mid-line about 1 in. above the basilar process of occipital bone and above and behind the transmeatal line; almost certainly within the brain. Calcification present anterior and proximal to the bullet. Small gap apparent in the bone of the left parieto-occipital region (entrance wound). February, 1925: Bullet seen lying in the same position as described above, and calcification present in the same area but rather less dense. April, 1922: Bullet in approximately the same position, but

the nose directed downwards more obliquely, about 60° from the horizontal as compared with the skiagrams of 1925 and 1928 in which it lies pointing backwards and downwards at about an angle of 30° from the horizontal.

Commentary.—It is remarkable, considering the depth to which the bullet penetrated, that death did not ensue from shock or hæmorrhage at the time of the wound. As far as it is possible to localize the position of the bullet in the brain, it appears to be lying above the anterior limit of the tentorium, above and in front of the roof of the fourth ventricle in its anterior part, and behind the pineal body and the anterior and posterior colliculi. In such a situation it would be liable to interfere with the cerebellar peduncles and possibly with the crura cerebri.

FIG. 1.
Skiagram (1928) lateral view of skull, showing bullet

According to the history of initial left hemiplegia, it seems that the right crus cerebri was damaged at the time of the original wound, that is, the crus on the side opposite to that of the bullet's entry. This condition gradually improved, but the signs of such a pyramidal lesion are still apparent in the muscular hypertonia and to some extent in the increased arm reflexes, knee- and ankle-jerks of the left side. I have watched this case for seven years, and it is only during the last four years that the patient has complained of a return of "weakness" and some unsteadiness of the left arm and, to a slighter extent, of the left leg. These symptoms result from gradually increasing incoördination of these limbs, more particularly of the left arm, which was not definite previous to 1925. I suggest that this

incoördination—especially of the left arm—is due to gradual involvement of the superior cerebellar peduncle by the calcified area above the bullet seen in the skiagram. This would likewise explain the nystagmus which has also appeared only recently.

FIG. 2.
(1928) Antero-posterior view.

Finally, is the calcification the end-result of local inflammatory process set up by the presence of a foreign body, or is it due to osteophytic proliferation of a few bony fragments of the skull that were carried into the brain with the bullet?

Severe Rickets with ? Dyschondroplasia.—BERNARD E. MYERS, C.M.G., M.D., and St. J. D. BUXTON, F.R.C.S.—G. B., male, aged 4, admitted to the Royal Waterloo Hospital, September, 1928, for rickets.

The mother states that up to 2 years of age, when he had pneumonia, he had been healthy and apparently nothing abnormal was observed about him. However, as he was not seen by a doctor during this time, and as there are seven other children in the family, with poor house conditions, in a general state of dirt, symptoms, if present, may not have been noticed by the mother. Nothing abnormal about pregnancy or labour. Patient was fed on boiled cow's milk; he had no orange juice or cod-liver oil, but later on, Scott's emulsion is said to have been given. The child has had measles, whooping-cough and pneumonia. There is no history of fractures. Head sweating, nocturnal restlessness and head rolling were not noticed by the mother.

On admission, he was seen to be suffering severely from rickets. There was marked thickening at the wrists, ankles and lower ends of femora. The lower parts of the tibiæ were noted to be distinctly curved, and the femora show very obvious forward and outward curving. The child has a habit of fitting the curved parts of the tibiæ into each other when lying down, and when sitting the femora are abducted. Rachitic rosaries are present on the ribs. There is some prominence of

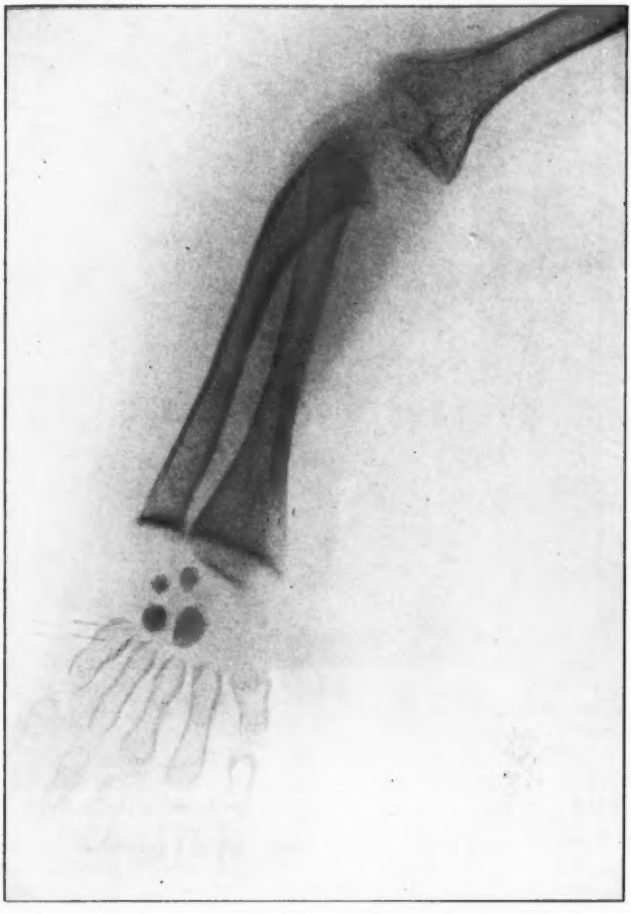


FIG. 1.—Skiagram of right forearm showing rachitic changes at lower end of radius.

the frontal and parietal eminences, but the head is not absolutely typical of rickets. The abdomen is distinctly protuberant. Harrison's sulcus is present, and the lower part of the ribs shows a rickety flare. There is a certain amount of kyphosis and also some scoliosis, but the spine straightens out when the child is lifted by hands placed beneath the shoulders. So far he has not walked, but he can stand with assistance. The tonsils are enlarged and the cervical glands easily felt.

X-ray examination (September, 1928) of the lower end of the femora and the radii showed typical rickets. This appearance has altered after three months' treatment, so that rachitic changes shown in the recent skiagram are slight. The upper end of the shafts of the femora show an unusual condition possibly suggestive of osteitis fibrosa. The pelvis has become triangular by lateral compression. Professor Knaggs, who kindly saw the case, is of opinion that the pelvis, femora, etc., are typical of severe rickets. There is stated to be no other case of bone deformity in the family.

Osteogenesis imperfecta, fragilitas ossium and achondroplasia are excluded by the history and the X-ray appearances, as well as by the clinical signs.

When first admitted, the child had a swelling just above the right lateral incisor and canine tooth, and there was swelling of the gums and the teeth were carious.

29.11.28

FIG. 2.—Skiagram of pelvis and upper ends of femora. Note the triradiate pelvis and the changes at the upper end of the diaphysis of the femur, suggested to be dyschondroplasia.

Mr. Cairns Forsyth removed the bony swelling. The microscopic appearance of this suggests the metaplasia of connective or fibrous tissue into bone.

Dr. Mackenzie Wallis found the blood calcium to be 5·47 mgm. per 100 c.c. The urine total acidity equals 60 c.c. of N/10 NaOH, ammonia 0·197 grm. per cent., and the calcium output in the urine for twenty-four hours equals 3·89 mgm., the P_2O_5 output in same time equalled 0·34 grm. He concluded that the urine shows a high acidity and ammonia content. In spite of the high acidity there is a much diminished output of phosphorus, as well as of calcium, in the urine. The calcium content of the blood is very low. From these results it would appear that the child is absorbing very little calcium and phosphorus from the alimentary tract.

Treatment has consisted of rest in bed, plenty of milk, butter, eggs, etc. He has also had malt and cod-liver oil, parathyroid, $\frac{1}{30}$ gr. daily, and calcium citrate. Since being in hospital his general appearance has distinctly improved. He is quite intelligent and happy.

It will be necessary to treat the deformity due to the changes at the upper end of the femur, but nothing will be carried out until the ossification is more complete in that region.

In this case there may be something superadded to the rickets.

Discussion.—Mr. ST. J. D. BUXTON said that the child was interesting owing to: (1) The condition of the upper jaw; (2) the abnormal skeletal condition.

(1) The gums and alveolar margins were enlarged, carious teeth were present and a bony swelling existed on the right side of the upper jaw, which microscopically showed a metaplasia from connective tissue into bone. The teeth had been removed and gradually the alveolus was getting smaller. The child had an infective periostitis—in fact a mild degree of one form of leontiasis ossea.

(2) The skiagrams after three months' interval showed that the child had rickets and was recovering. However, the upper ends of the femoral diaphyses were not like those in rickets. The neck was thick, and was ossifying irregularly, and there were translucent areas. The epiphysis for the head, which had only begun to ossify three months ago, had advanced considerably lately, but the cartilage between epiphysis and diaphysis is broader than normal.

It was suggested that this child had rickets and one type of dyschondroplasia. This condition was also called Ollier's disease, but Ollier's description in 1898 does not appear to coincide with the changes we now recognize.

It is common in males; there is dwarfing of the limbs and it may be uni- or bi-lateral. There are irregularities of ossification of the diaphyses of the long bones; the carpus and tarsus are normal, but the hands and feet may be affected. One case is quoted (*Brit. Journal of Surg.*, xv, 1927, p. 130) of combined rickets and dyschondroplasia, but that differs from the case shown by irregular protuberances from the ends of the diaphyses.

It is not intended to correct deformities at present.

Dr. J. W. CARR (President) asked what was the cause of dyschondroplasia. When the child first came under observation was there much sweating about the head, or about the body generally?

Dr. KINGSTON BARTON said that Dr. Myers' remarks suggested that this was not so much acute present rickets as past rickets. The skiagrams of the pelvis suggested pressure upwards of the heads of the femora, or pressure downwards of the trunk, which had been operative probably for a year, as there was great deformity of the pelvis, and there was a deformity of the tibia just above the ankle. As the child was supposed to have been normal at the age of 2 years, and as its age was now 4, there had probably been a gradual and late development of rickets which had been unnoticed and therefore untreated. Surely this was a case for testing whether ultra-violet rays could cure rickets, independently of dieting. In experimental rats the development of rickets was a matter of three or four weeks, in human beings of from three to fifteen months. If there was an excess of calcium in the diet, in some extraordinary way it inhibited the absorption of phosphorus; or one could reverse it. Hence, accepting the views of the chemical pathologists, it was not easy to know what one should do. If one almost starved the child, and gave him nothing extra, he would recover more quickly, because he would then make use of his normal quantity in a better way than if the intestine was overloaded with an excess of either calcium or phosphate.

Dr. E. STOLKIND said his experience of phosphorus without the addition of cod-liver oil was that it exerted no influence on the rickets at all.

Dr. MYERS (in reply) said that no definite history could be obtained as to head sweating and head rolling, and general irritability. When the X-rays showed the bones to be properly hardened the child could be allowed to stand. Osteotomy might be required later on for the tibiæ.

Severe Anæmia and Bronzing.—R. C. LIGHTWOOD, M.D.—D. G., male, aged 7½, brought to out-patient department at Hospital for Sick Children (Dr. Pearson) on May 29, 1928.

Complaint.—Not well since about December, 1927; small since birth; short of breath; easily tired; mother had noticed small clots of blood in patient's gums five times in the previous three months; bruises easily; pains in limbs. He has always had a dark-brown skin; has attended a special school for "debility"; has had artificial sunlight (four times; pigmentation preceded).

Past Illnesses.—Chicken-pox, measles, mumps, and whooping-cough.

Family History.—Three other children, all healthy; parents healthy; no tuberculosis. *Pregnancy.*—Normal, except that mother was in poor health on account of feeding another baby.

Admitted May 29, 1928, under Dr. Poynton. Weight, 32½ lb. *Diagnosis.*—Anæmia and bronzing.

Examination (May 29, 1928.)—Small wasted child with dark-brown skin and brittle hair. Three or four small cutaneous bruises. Hæmic bruits. Blood-pressure: systolic 110, diastolic 65 mm. of Hg. No enlargement of glands, liver, or spleen. *Central Nervous System.*—Normal; *Fundi*: hæmorrhage left disc. *Wassermann* negative.

Fragility Test.—There was increased resistance to laking by hypotonic saline. Trace of hæmolysis at 0·36 per cent. NaCl. Incomplete hæmolysis at 0·33 per cent. NaCl.

Treatment.—Liver extract 5 ij, b.d., Ferri et ammonii citras, stovarsol ½ tablet b.d. Blood-transfusion (400 c.c.) August 17, 1928. Discharged on October 26, 1928.

Course.—After admission, occasional bruises appeared for a time. Small warts on face were noted in June; these still persist. Irregular fever occurred at the end of July and beginning of August. At this time there was enlargement of the cervical glands on both sides.

The blood-picture is shown in the table:—

	29.5.28	30.5.28	30.5.28	8.6.28	15.6.28	9.7.28	31.7.28
R.B.C.	700,000 per c.m.		1,000,000	800,000	925,000	1,050,000	2,750,000
W.B.C.	4,800 per c.m.		—	3,600	3,800	7,800	6,000
Hgb.	Less than 20 per cent.		20 per cent.	16 per cent.	16 per cent.	15 per cent.	15 per cent.
C.I.	?		1	1	0·7	0·75	0·26
Differential:							
Polymorph. ...	20 per cent.		—	24 per cent.	52 per cent.	26 per cent.	44 per cent.
Sm. lymph. ...	71 per cent.		—	70 per cent.	36 per cent.	8 per cent.	52 per cent.
L. lymph. ...	9 per cent.		—	6 per cent.	10 per cent.	54 per cent.	3 per cent.
L. Mono. ...	—		—	—	1 per cent.	10 per cent.	—
Eos.	—		—	—	—	—	—
Bas.	—		—	—	—	—	—
Trans.	—		—	—	—	2 per cent.	1 per cent.
Abnormal cells ...	nil		—	—	nil	nil	nil
Remarks	—		Platelets: 10,000 per c.mm.	—	Polychromatophilia	Increased resistance to laking	Poikilocytosis

Admitted 29.5.28

Liver extract, iron and stovarsol commenced

[Continued]

	15.8.28		22.8.28	4.9.28	21.9.28	17.10.28		7.12.28
R.B.C.	500,000		1,762,500	1,625,000	1,700,000	2,412,500		1,736,000
W.B.C.	6,400		6,200	14,000	4,400	7,800		8,000
Hgb.	10 per cent.		30 per cent.	35 per cent.	25 per cent.	35 per cent.		35 per cent.
C.I.	1		0·9	1	0·75	0·7		1
Differential :								
Polymorph. ...	33 per cent.		28 per cent.	21 per cent.	43 per cent.	40 per cent.		25 per cent.
Sm. lymph. ...	57 per cent.		63 per cent.	66 per cent.	39 per cent.	48 per cent.		61 per cent.
L. lymph. ...	10 per cent.		5 per cent.	7 per cent.	11 per cent.	8 per cent.		4 per cent.
L. Mono. ...	—		2 per cent.	—	—	—		7 per cent.
Eos.	—		1 per cent.	3 per cent.	4 per cent.	—		1 per cent.
Bas.	—		—	2 per cent.	—	1 per cent.		0 per cent.
Trans.	—		1 per cent.	1 per cent.	2 per cent.	3 per cent.		1 per cent.
Abnormal cells ...	nil		nil	1 nucleated red cell per 100 white cells	2 nucleated red cells per 100 white cells.	nil		1 per cent. of Neutrophil myelocytes. 1 nucleated red cell per 100 white cells.
Remarks	Aniso-cytosis and poikilo-cytosis		—	—	—	Anisocytosis and poikilo-cytosis marked		Anisocytosis and poikilo-cytosis slight.

Transfusion 400 c.c. August 17, 1928.

Discharged 26.10.28

After discharge he attended as an out-patient.

November 23, 1928.—A few bruises. Hæmic bruits persist. No enlargement of liver or spleen.

December 7, 1928.—Wassermann test repeated; reaction negative. Weight 34½ lb.

Blood-pressure: systolic 106, diastolic 70, silent zone 100 to 90 mm. of Hg.

This is a case of severe anæmia which shows some evidence of blood regeneration. It has lasted about a year, and is accompanied by a pigmentation similar to that of Addison's disease, but without the characteristic coloration of mucous membranes or the low blood-pressure. An intradermic tuberculin test (Mantoux) with 0·1 c.c. of a dilution of 1 in 1,000 O.T. has been negative, and there is no clinical evidence of tuberculosis. Treatment by both liver and iron therapy has failed, but a whole-blood transfusion in August was beneficial. Since then the blood-picture has again deteriorated, and further transfusions will probably be necessary.

Pigmentation occurs in such a variety of conditions that it is sometimes difficult to ascertain the cause in particular cases. Arsenic and exposure to ultra-violet light have been excluded. Perhaps this child should be regarded as an example of "anæmia gravis of childhood" (Thursfield),[1] a disease which may correspond to the pernicious anæmia of adults. In pernicious anæmia, pigmentation (melanodermia) is a rare occurrence,[2] and its presence in this case may have a similar origin.

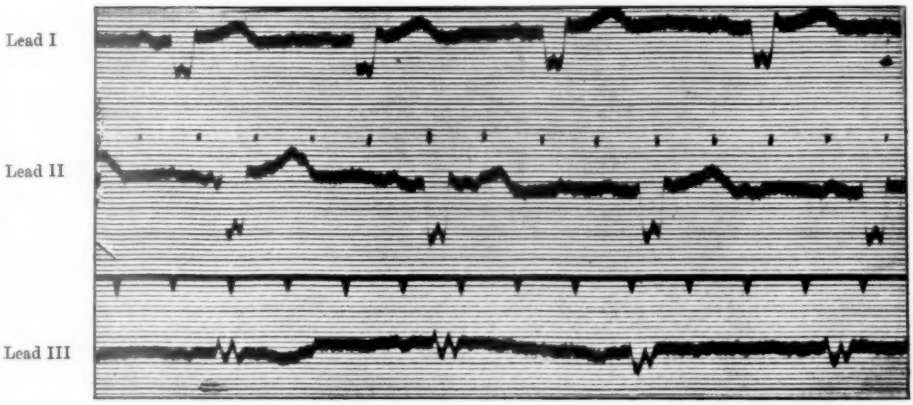
1 F. W. Price (*Ed.*), "Practice of Medicine," Second Ed., 1926, 697.

2 J. H. Sequeira, "Melanodermia associated with Pernicious Anæmia," *Proc. Roy. Soc. Med.*, 1910, iv (Sect. Derm.), 26.

(3) **Congenital Heart Disease associated with Arborization Block of the Left Branch Bundle.**—BERTRAM S. NISSÉ, M.D.—E. W., girl, aged 7. Father and mother healthy. One other child, quite healthy. No miscarriages.

History.—When 5 weeks old, presence of congenital heart disease noted. Has always been very easily tired, and after slight exertion becomes blue and breathless. Is very backward, did not begin to talk until 2 years of age, and did not learn to walk until 3½. Measles and whooping-cough, three years ago.

Clinical Findings.—The child is under-developed for her age, and is of subnormal intelligence. She has an old, rather wizened-looking face and the bridge of the nose is noticeably depressed. There is cyanosis of the lips and cheeks; but, though they are livid, there is no clubbing of the fingers and toes. The tongue is cyanosed and its tip presents a rather bifid appearance. Tonsils enlarged. Pulse-rate 84; rhythm regular. Blood-pressure: 90 mm. systolic, 55 mm. diastolic. Right- and left-sided enlargement of heart confirmed by orthodiagram; apex beat external to midclavicular line in fifth intercostal space. A systolic thrill is easily felt all over the præcordium but is most marked in third interspace to left of sternum. A harsh systolic murmur is heard all over the præcordium; maximal over third left interspace; conducted through



to the back. Second sound inaudible at base. Lungs clear. Liver and spleen are palpable. Wassermann reaction negative. Blood-count: Red cells, 5,500,000; white cells, 18,040; hæmoglobin 110 per cent.; colour index = 1.

Electrocardiogram shows a definite arborization block; leads I and III are suggestive of damage to the left branch bundle; lead II was, however, atypical.

The case is apparently one of a congenital arborization block of the left branch bundle associated with a patent interventricular septum and probably hypoplasia of the pulmonary artery.

Congenital heart block in association with malformation of the heart practically always affects the conductivity along the main bundle, the block generally being complete, although cases of 2 : 1 block have been noted. There is apparently only one instance in which a diagnosis of congenital malformation of the heart associated with a right branch bundle lesion has been confirmed by graphic record [1]. Maude Abbott, however, refers to a case [2] which was about to be published by Baumgartner, in which there was an abnormality in the ventricular complex of the electrocardiogram. In a careful histological study of a case of congenital heart disease

with a defective septum, Morison [3, 4] found that the main A.V. bundle divided into an abortive left branch, which coursed in the left auriculo-ventricular groove but failed to reach the left ventricle, and into a larger right branch, which divided lower in its course to supply the left as well as the right ventricle. Such a distribution might conceivably explain the changes shown in the ventricular complex of the electrocardiogram in the case here described. The possibility, however, that these changes may be the sequel to a fœtal carditis must also be borne in mind.

I am indebted to Dr. B. T. Parsons-Smith for permission to show this case.

REFERENCES.

[1] SCHLESINGER, B., *Proc. Roy. Soc. Med.*, 1927, xxi, Part 2. [2] ABBOTT, MAUDE E., Osler and McCrae's "System of Medicine," 1927, iv, 612. [3] MORISON, *Proc. Roy. Soc. Med.*, 1912, vi, Part 1. [4] MORISON, *Journ. Anat. and Physiol.*, 1913, xlvii, 459.

Discussion.—Dr. S. H. COOKSON suggested that the electrocardiogram in this case could be entirely explained by defective conduction in the left branch of the bundle of His. There was no clear proof that one could diagnose a lesion of the Purkinje network from an electrocardiogram. Experiments in America conducted by Smith showed that extensive incision into the myocardium of dogs would not of itself produce the curves said to represent arborization. In this country, recently, Drury had published a case with normal electrocardiogram in which there was a lesion in the myocardium similar to that described by Rothberger and Winterburg as producing arborization block. Last year he (the speaker) saw a case in which there was a very extensive syphilitic lesion of the left ventricle, so that the posterior and left lateral walls formed three large aneurysmal dilatations. In that case, therefore, there must have been extensive damage to the network, yet the electrocardiograms were normal except that the complexes were small. If there was delay in conduction along one branch, one could get the exact type of complex which had just been demonstrated and which had been shown experimentally by Herrmann and Wilson.

Dr. EVAN BEDFORD said that the term "arborization block" was best avoided, as it had not been proved to represent a pathological entity, and it could be explained by a partial defect of one branch. A definite widening of the complexes always meant a lesion of the main branch. The fact that complexes were small indicated that there was some further feature in the case.

The PRESIDENT asked as to the significance of the white cell increase.

Dr. NISSÉ (in reply (said that he thought the electrocardiographic picture was due to an abortive left branch, the fibres of which failed to reach the left ventricle. He did not think the leucocytosis had any significance.

Central Osteomyelitis following Puerperal Sepsis, after Three and a Half Months' Abortion.—WM. E. TANNER, M.S.—Woman, aged 34, married, admitted to the North-Western Fever Hospital, under the care of Dr. Breen, on July 10, 1928, certified "puerperal septicæmia"; then gravely ill. There was a severe infected laceration of the perineum with a good deal of local uterine infection; blood-culture was negative and there was no sign of extra-uterine spread.

The uterine sepsis cleared up completely in ten days. July 21, 1928: Patient complained of pain in the right arm, and three days later the arm was swollen. Within a week there was a similar condition in the left arm. The arms were treated with rest and fomentations. There was an evening pyrexia until September 17, 1928.

October 2, 1928: Still pain in arms, with swelling of upper parts of the humeri, but no œdema or pyrexia. The patient was anæmic, and a blood-count showed 4,000,000 red cells with a colour index of 0·8. Wassermann reaction, negative.

Skiagrams taken at this time show an osteomyelitis of both humeral shafts, with numerous cavities and several sequestra.

Iron has been administered by the mouth, and massage and movements have

been carried out. The general condition has improved; there is now very little pain in the arms, and movements are good.

The interest of the case lies in the symmetrical involvement of the nutrient arteries by an embolic inflammatory process which has attacked the upper branches. Resolution has taken place spontaneously without operation. Twice this year I have seen an acute central infection of bone following tonsillectomy, and on many occasions as a complication of scarlet fever, but in these cases the septicæmia was much more severe and the bone lesions were much more acute.

Chronic Osteomyelitis in a Girl, aged 5.—CECIL P. G. WAKELEY, F.R.C.S.—I. W., female, when aged 2 was admitted to hospital February 28, 1925, with a large boil on the left nostril. There was œdema of the face and marked chemosis; the temperature was 105·2° F., pulse 148, respirations 40.

Skiagram of right femur showing cavities in the upper part of the shaft.

An incision was made over the inner canthus and a considerable quantity of pus was liberated. The nasal bones were found to be necrosed. The discharge

diminished, and an autogenous vaccine was given (staphylococci). The patient left hospital March 24, 1925, only to be readmitted ten days later with a swollen left foot. The swelling was thought to be tuberculous and a plaster was applied to the foot. This swelling entirely disappeared and the plaster was removed after three months.

One year later (March 6, 1926) the patient was readmitted with swelling on medial side of left arm, about $\frac{3}{4}$ in. in diameter, not painful; no limitation of elbow movements. Discharging sore over lower end of right fibula, with thickening of the malleolus, but no limitation of ankle movements.

Swelling on left arm was explored March 11, 1926, and blood-stained pus was obtained which yielded *Staphylococcus aureus*; bone opened and packed with flavine and paraffin. Ankle sinus was excised, bone abscess gouged out, rubbed with Bipp and sewn up without drainage. Patient discharged April 28, 1926, with both wounds healed.

Readmitted to hospital December 8, 1928, with pain in right thigh. On examination: There is a thickening on the outer aspect of the right femur below the great trochanter. Flexion of the right hip is painful and is limited by 15 degrees. Abduction is slightly limited. Other movements normal. There is $\frac{1}{2}$ in. of wastin in the thigh muscles.

A skiagram shows chronic osteomyelitis of the upper end of the right femoral shaft, with a large cavity.

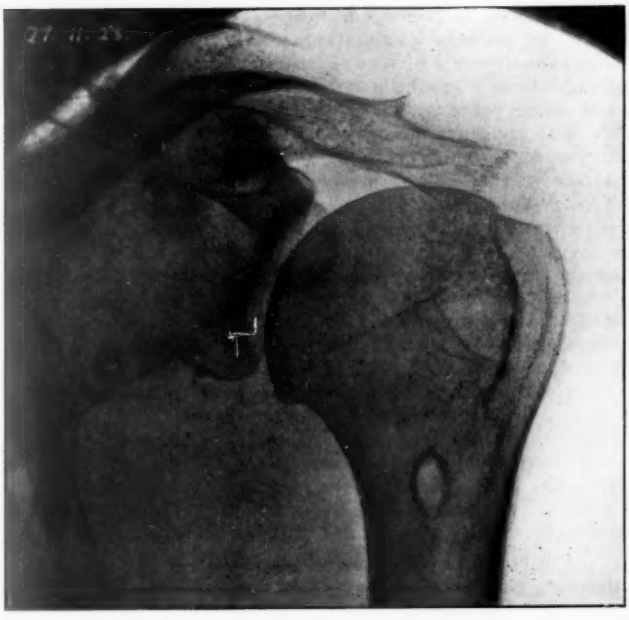
Osteomyelitis causing Elongation of Tibia and Fibula.—A. E. MORTIMER WOOLF, F.R.C.S. (shown by Mr. I. PRICE). Patient, a boy, aged $5\frac{1}{2}$, has elongation of one tibia as a result of osteomyelitis three years ago. Three years ago the patient had the usual gutter operation, and thirteen months later a sequestrum separated. Three months after that another sequestrum was removed by operation, and since then the case has been treated by artificial sunlight. Three months ago genu valgum developed, and the affected tibia was found to be two inches longer than the other; the fibula also was elongated.

Mr. B. WHITCHURCH HOWELL (discussing the last three cases) said that in the case of the woman shown by Mr. Tanner, he would not operate. Sequestra of that size were almost invariably absorbed without further incision being necessary.

In Mr. Wakeley's case of the child with the swelling in the upper third of the right femur, he thought there should be an exploration. It would be wise to do a radical sequestrectomy with primary suture of the wound.

With regard to Mr. Woolf's case, it was common to observe an increase in the length as well as in the width of a bone, as was seen during the war, in cases of gunshot wound. Concerning the removal of the sequestrum in the tibia in the boy, he thought that should be done on the same lines as he had advised in the preceding case—radical sequestrectomy, putting spirit and bipp into the wound in only a small quantity. Artificial sunlight treatment was in these cases a sheer waste of time, money and energy.

A Case of Chronic Abscess in the Shaft of the Humerus.—CECIL P. G. WAKELEY, F.R.C.S.—Dr. W. F. S., aged 46, was operated upon in May, 1922, in Germany for osteomyelitis of the greater tuberosity of the left humerus. The cavity was curetted and packed with gauze and the wound was allowed to heal from the bottom. From 1922 until 1928 the patient was quite free from pain. In the spring of 1928 he suffered from vague pains in the left shoulder. The pains became worse and in November, 1928, the patient came under observation. On examination it was found that there was some deep tenderness on pressure over the upper part of the shaft of the left humerus. Movements of the shoulder-joint were quite free. In the skiagram a definite circumscribed abscess could be seen in the upper part of the shaft of the humerus.



Skiagram of the upper end of the left humerus showing isolated chronic abscess.

Congenital Deformity of the Forearm and Hand.—CECIL P. G. WAKELEY, F.R.C.S.—J. C., female, aged 4 months, was brought up to hospital because of two small nævi on her back. On examination the child was found to have a deformed left forearm and hand. The forearm is curved inwards, owing to the fact that the radius is bent and distorted, while the ulna appears to be shorter than the bone on the opposite side and is considerably thicker than normal. There are only two digits connected to the carpus and these are united by a very definite web. The external digit is the thicker of the two and is probably the thumb. It contains a metacarpal and two phalanges. The inner digit contains a metacarpal and three phalanges.

There is no family history of any deformities. The nævi on the back have been treated with carbon-dioxide snow.

Mr. B. WHITCHURCH HOWELL said that these deformities had to be splinted, otherwise they became worse as the patient grew older. Splinting was tedious and because it had to be done over such a long time the mother was liable to lose her enthusiasm. He had now under his care a case of congenital absence of radius, and with only four digits, with fair function, without webbing. Splinting had been done for four years, and the forearm was now straight, waiting for a bone operation to replace the radius. In the present case he would be prepared to treat with a movable splint, at first of metal, followed later by a laced leather support. He did not believe in operations on webbed fingers in children under 6 years of age; it was wise to wait as long as possible, so as to get plenty of tissue for the operation, afterwards keeping the fingers apart by some form of splint or traction. He used a Limerick hook on the finger, and a cock-up splint with a bridge.

Oleothorax.—E. G. CHANDLER, M.D., F.R.C.P.—By oleothorax is meant oil in the pleural cavity. The oil I have used is olive oil containing 5 per cent. oil of gomenol. It is a method of maintaining lung collapse. It is specially indicated: (1)

When a continuous pressure is desired. (2) When obliterative pleurisy is occurring in the course of an artificial pneumothorax. (3) In certain cases of tuberculous empyema. The case shown is of special interest because the patient was desperately ill with the combination of phthisis and severe diabetes, and he has been doing full work for nearly three years with a complete oleothorax. T. McC., male, in 1925, when aged 25, came under my observation with diabetes and pulmonary tuberculosis. The diabetes was so severe that dieting and 60 units of insulin hardly controlled it. There was extensive tuberculosis in the left lung of a rapidly-spreading broncho-pneumonic type. Tubercle bacilli were present in the sputum in large numbers. The temperature was high and the patient was emaciated. Artificial pneumothorax was induced by Dr. F. H. Young. The temperature fell to normal, the general condition rapidly improved; cough, expectoration and tubercle bacilli disappeared. The insulin was reduced from 60 to 15 units. After a time a large effusion appeared on the left side. This was replaced twice, once by air and once by oxygen, but again recurred. Fearing the onset of obliterative pleurisy I decided to replace the fluid by oil. This was done in March, 1926; 600 c.c. of 5 per cent. gomenolized olive oil were left in the pleural cavity. The patient has since remained in good health. He has gained over 2 st. in weight, and is doing full work. At first refills of air were given above the oil. In March, 1927, more oil, 250 c.c. was injected. Since that date refills of oil have been given as follows: On February 7, 1928, 360 c.c., and on July 21, 1928, 390 c.c. The air of the pneumothorax was gradually absorbed and the oil was pushed, by the pressure of the contracting pleural space, right up to the dome of the pleural cavity. The last two fills had to be given under great pressure.

Discussion.—The PRESIDENT said that a few years ago the case of a patient suffering from severe diabetes, with active and extensive pulmonary tuberculosis, would have been regarded as hopeless. What was the future of this patient? He took it that the oil would be slowly absorbed, and there would come a time when it could not be replaced. Would the lung process then remain quiescent, or would some further operative procedure be required?

Dr. PARKES WEBER asked what happened to the oil when it was absorbed, and whether it appeared in the urine.

Dr. L. S. T. BURRELL said that his own difficulty about putting oil in the chest was that it stayed there. He had seen, in Switzerland some years ago, that when oil was placed in the chest it remained there for years. Four or five years ago a patient in South Africa had pneumothorax, and the doctors there, feeling that the man could not have refills when travelling to England, put in some gomenolized olive oil. The patient duly arrived in London, but he (the speaker) had never before seen a man in such a state as he was in on arrival. The pleural cavity had filled up with fluid, so that there was great displacement of the heart.

In Dr. Chandler's case the lung was adherent, and the mediastinum for that reason was prevented from being pushed over. When oil was put in, with a free mediastinum, it would require so much to fill the pleural cavity that it would cause displacement of the heart and mediastinum. He had found that the oil did not prevent much of the obliterative type of pneumothorax; the oil got higher up in the chest, and the adhesive process at the bottom of the lung continued to progress. Whether it did so as quickly as by the other method he did not know. The chest could not be completely filled with oil, there must be an air space above. He asked how quickly the oil was absorbed. He thought the forced fibrosis was so strong that it would still go on. Did it lead to the fibrotic and obliterative type of pneumothorax, or did it keep the lung collapsed?

Dr. EVAN BEDFORD asked whether the oil could be removed again if one desired to do so.

Dr. F. G. CHANDLER, in reply to the President, said that the oil in this case would probably not be completely absorbed for another year or two, by which time he hoped that the lung would have healed. He would try another oil refill in six months' time, but he was afraid that the intrapleural pressure would be too great to admit any more oil.

In reply to Dr. Parkes Weber: the oil was taken up apparently by the cells lining the pleura; a lipolytic action took place and the oil, he imagined, was used as food. If an oil

such as lipiodol was injected into the pleural cavity, the iodine fragment was liberated and could be detected in the urine.

In reply to Dr. Burrell: The rate of absorption was roughly known; olive oil might be completely absorbed in six months, and paraffin oil in from twelve to eighteen months; he did not employ liquid paraffin, however, as in one case it had appeared to irritate and to thicken the pleura.

The accident that Dr. Burrell described should never occur. He could not understand anyone producing an oleothorax and straightway sending the patient upon a voyage. After the first two or three injections of oil, the patient must be carefully watched, lest irritation of the pleura should occur, with consequent exudation of fluid and a serious displacement of the mediastinum. In certain cases oil would produce adhesions, but in obliterative pleurisy, sufficient oil must be put in to prevent the pleural surfaces coming together. As the pleural cavity further contracted the oil was driven to the upper part of the chest and it was usually just in this part that it is wanted.

In reply to Dr. Evan Bedford: It was perfectly easy to remove the oil. He had taken oil out six months, twelve months, and eighteen months after it had been put in and it was still as sweet and clean as when it was injected.

Aortic Stenosis with Mitral Stenosis.—B. T. PARSONS-SMITH, M.D.—A.V., male, aged 42. No subjective symptoms. Past history negative, except for influenza in 1918.

Examination.—Normal area of heart dullness; apex impulse in 5th space, 3¼ in. from mid-line; regular rhythm; systolic thrill and harsh systolic murmur at aortic region, conducted upwards to the cervical vessels; second heart sound inaudible in the aortic region; systolic and mid-diastolic murmurs at the apex region; pericardial friction sounds over aortic area; pulse 88, regular; blood-pressure 135-65; electrocardiogram: T waves inverted in all the leads; right waves slurred in Lead I; R.S. complexes split in Lead III.

X-ray Examination.—Hypertrophy of left ventricle and enlargement of left auricle. (Orthodiagraph exhibited.)

This case probably belongs to that somewhat rare group in which the site of the stenosis is subaortic, i.e., below the valves; six cases of this type of aortic stenosis are mentioned by Osler, and he refers to twenty other cases recorded by Goldweizer; the lesion may be congenital—Keith explains it as an arrest of development of the conus—or it may be acquired, the inflammatory process having spread from the mitral valve to the subaortic region. In certain respects, subaortic stenosis differs in its symptomatology from aortic stenosis of the valvular type; the condition is, as a rule, discovered accidentally and symptoms of diminished circulatory output may be largely absent as in the case at present under discussion in which there is no suggestion of pallor, no history of giddiness or fainting and no tendency to bradycardia.

Discussion.—Dr. EVAN BEDFORD said that he did not agree with Dr. Parsons-Smith about the ætiology; he thought that this was a straightforward case of rheumatic aortic and mitral stenosis. The combination of aortic and mitral stenosis was quite common in rheumatic cases. In cases of combined aortic and mitral disease of rheumatic origin in adults, an aortic systolic thrill could often be detected if carefully sought for. Cabot had stated that some degree of stenosis was the rule in cases of aortic incompetence in adults, examined post mortem. He (the speaker) had followed up a series of cases of rheumatic aortic stenosis and had found that there was always some degree of regurgitation to be detected clinically. He had only found one case of rheumatic aortic stenosis without clinical incompetence and even in that an aortic diastolic murmur was audible occasionally.

In Dr. Parsons-Smith's case there was a definite aortic diastolic murmur which was in favour of a rheumatic and against a congenital lesion, in which the stenosis was usually "pure." He (the speaker) believed that these "rheumatic" cases started as aortic incompetence and that, in the course of time, gradual stenosis occurred and the degree of incompetence diminished. He had proved this in one case of clinically almost pure stenosis,

as reference to previous hospital records had shown that aortic incompetence had been diagnosed years before. In Dr. Parsons-Smith's case the presence of mitral stenosis proved the rheumatic origin of the lesion.

Dr. PARSONS-SMITH (in reply) emphasized the accidental discovery of the condition and the absence of rheumatic history; he thought that the absolute freedom from subjective symptoms of any kind made it more likely that the valve lesion was of congenital origin rather than that it was an acquired endocarditis.

Paroxysmal Tachycardia of Forty-eight years' Duration and right Branch Bundle Block.—FRANCIS BACH, B.M. (introduced by Dr. PARSONS-SMITH).—H. H., male, aged 57; musician; admitted 1928 to the National Heart Hospital under Dr. Strickland Goodall on account of attacks of "palpitations" which recently had prevented him from working. Since age of nine has suffered from paroxysmal tachycardia; he says that his heart suddenly begins to beat very rapidly; has a suffocating feeling in his throat and breathing becomes difficult. Attack has sudden onset and sudden cessation; immediately after he feels quite well again. Duration of attack varies from two minutes to fourteen days. He is

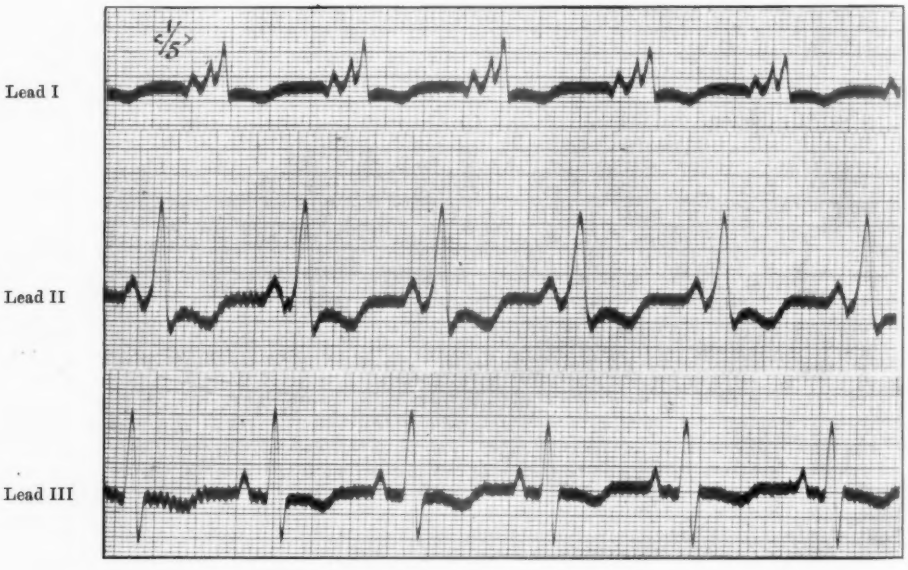


FIG. 1.—Taken between attacks, 1914.

frequently able to stop the attacks by holding his breath, and putting his head between his knees. Between the ages of 47 and 57 he had no attacks. Three months previous to admission there was a recurrence of attacks, of increasing frequency and duration, but no alteration in character. Fourteen years ago he was admitted to the London Hospital under the late Sir James Mackenzie and Dr. Parkinson. An electrocardiogram of paroxysmal tachycardia was obtained and also one between attacks (fig. 1).

Résumé of Clinical Examination.—Cardiovascular system: heart enlarged; apex beat, sixth space, 6½ in. in anterior axillary line; first and second sounds heard in all areas. Aortic second accentuated. No murmurs. Rate 80, regular. Liver just

palpable. Slight œdema of the ankles. A few crepitations left lung base. *Skiagram :* Heart rather enlarged and apex displaced. Lungs clear. Electrocardiograph. Liver and radial tracings were taken. *Electrocardiogram* (shown) taken between the attacks suggests evidence of an early right bundle branch lesion, although the right wave in Lead II is quite atypical. *Past History.*—No rheumatic fever. Diphtheria, scarlet fever thirty years ago. Septic right arm, followed by distension of veins in right upper chest, which has persisted.

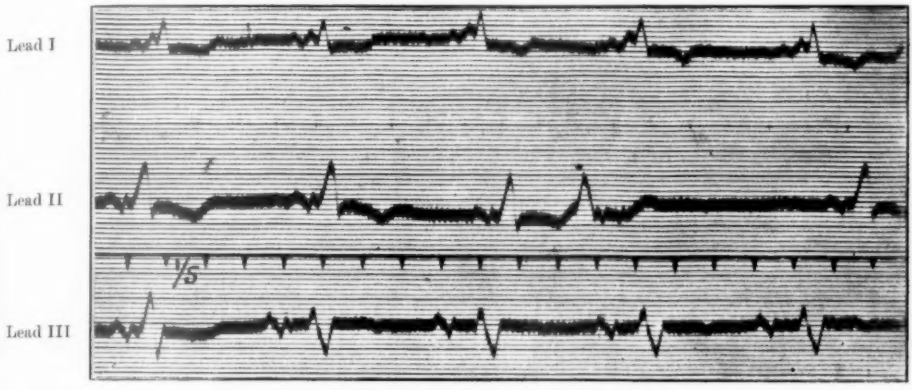


FIG. 2.—Taken between attacks, 1928.

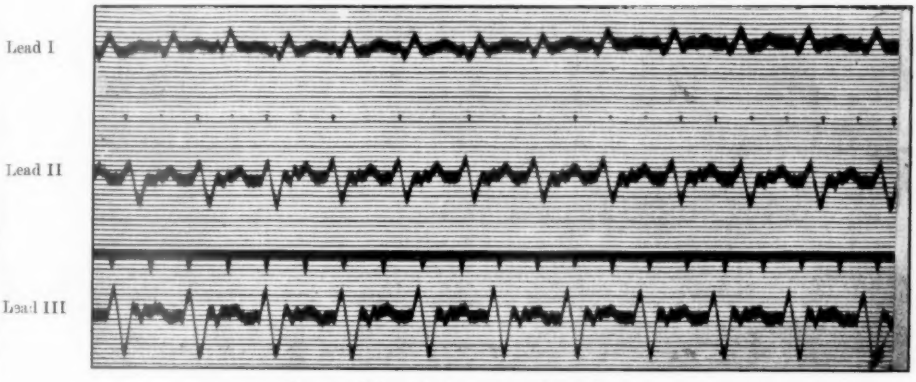


FIG. 3.—Taken during an attack, 1928.

Present Condition.—Well-built man. *Cardiovascular system :* heart apex beat outside nipple line in sixth space. Left: local heaving. Rate 80, regular. Occasional premature beat. Apical first and second sounds modified. Basal, aortic and pulmonary second sounds modified. Blood-pressure: 120-90 mm. Hg (right arm). *Respiratory and Nervous System :* Normal. *Abdomen.*—Liver just palpable. *Urine.*—Normal. Wassermann reaction (blood), negative.

Orthodiagram.—Heart shadow definitely enlarged to left. Aortic shadow normal limits.

Electrocardiogram.—Taken between attacks (fig. 2) shows: regular rhythm, right branch bundle block, increased P.R. interval, prolonged and notched Q.R.S. complex; delayed and inverted T waves in all leads. An occasional extra systole.

Considered in conjunction with the first record this shows an essentially similar type of curve, but there is a progressive change in that the ventricular complex is widened (delayed), notched, and the voltage less. Taken during an attack (fig. 3) shows paroxysmal tachycardia. Ventricular rate: 170 per minute.

The chief points of interest are: Firstly, the attacks of paroxysmal tachycardia of forty-eight years' duration. Secondly, the clinical evidence of cardiovascular disease associated with electrocardiographic records suggesting right branch bundle block, extending over a period of fourteen years. The question raised is as to whether there is any association between these two conditions.

Dr. COOKSON said the case was important in showing that a considerable degree of cardiac enlargement and a bundle branch lesion were not inconsistent with a very good prognosis. For the last fifteen years, despite both these lesions, the patient had been going about in good health except for the times when he had paroxysms of tachycardia.

Disseminated Sclerosis with Multiple Carcinomata of the Skin.—CECIL P. G. WAKELEY, F.R.C.S.—D. B. M., male, aged 57. Complains of paralysis of legs and sore on chest. Forty years ago complained of seeing double.

Treatment.—Drops applied and condition improved. Twenty years ago patient fell on several occasions because his legs gave way. These attacks would come on suddenly and after a few minutes would disappear and patient would continue walking. No pain.

Examination of the Central Nervous System.—Pupils, equal in size; react tc light. No nystagmus. Internal recti paralysed. Double vision at certain angles. Other cranial nerves, normal. ? Slight muscular atrophy of lower limbs.

The only voluntary movement patient can perform is to move the toes of his left foot. Knee-jerks present, ankle-jerks not present, plantars extensor. He complains of "pins and needles" sometimes, but sensation of pain, heat and cold, normal. Upper reflexes normal; motor and sensory powers normal. Sphincters normal, never incontinent. *Cerebral-spinal fluid:* normal. Wassermann reaction, negative. *Blood:* Wassermann reaction, negative. *Examination.*—On the anterior surface of the chest there is a growth situated between the upper border of the sixth rib to the eighth rib in the left anterior axillary line. Edges raised, indurated. Surface red, rough, ulcerated in three places. Consistency, firm and indurated. Slightly tender on firm pressure. Freely movable over ribs. There are several smaller indurated raised patches varying in size situated irregularly around the big mass. About eight years ago patient first noticed the red patch on his chest. It was not painful. He does not carry his pipe in his waistcoat pocket. Three years ago the red patch became raised. Eight months ago he noticed that the place was tender and swollen and it became gradually covered with black spots. Iodex was applied and condition became worse, a definite ulcer resulting. The surrounding red patches appeared a month ago and were painful until a week ago, even the pressure of clothes caused him pain. In the middle of the back between the scapulæ there are several irregularly shaped, raised, red, indurated patches, not ulcerated. The mass extends more on the right side than on the left. Is it of use to try X-ray treatment for this case? Radium is out of the question; it would require too much to cover the secondary, as well as the primary growths.

Dr. F. PARKES WEBER suggested that, as yet, in the absence of microscopical examination and of X-ray treatment, the possibility of mycosis fungoides could not be excluded.

Postscript (January 24, 1929).—A portion of the growth was excised in December, 1928, and on microscopical examination it was found to be a nævo-carcinoma. X-ray treatment has been given and the results so far have been excellent; the tumour has almost disappeared.—[C. P. G. W.]

The report of other cases shown at this Meeting will appear in the next issue.

Section of Medicine.

[*January* 15, 1929.]

Endocrine Tumours.

By F. Parkes Weber, M.D., F.R.C.P.

Abstract.—The paper opens with a short historical survey of the evidence that the cells of tumours, including cancers, may manifest functional, secretory or other activity (œcology) of the same nature as that of the normal glandular or other cells from which they have arisen. This is followed by a short consideration of the classification and the acknowledged and doubtful endocrine or hormonic effects of primary tumours and tumour-like hyperplasias of the various endocrine glands of the body.

Perls, in his "Manual of General Pathology," in 1877 [1], seems to have been the first to record that bile or a bile-like substance can be secreted by the tumour-cells of a primary adenoma or a adeno-carcinoma of the liver. In one case he discovered evidence of this not only in a primary cancer in the liver but in a metastatic growth from it at the base of the skull. This observation by Perls was confirmed by those of F. T. Paul (1885) [2], of Liverpool, who apparently was ignorant of Perls's work, and was the first in England to draw attention to the point.

It has since been universally recognized. Now it is acknowledged that the cells of tumours, including cancers, may give evidence of functional, secretory or other activity (œcology) of the same nature as that of the normal glandular or other cells from which they have arisen [3]. Thus according to Waring [4], the cell elements of the primary and secondary growths in cases of pancreatic carcinoma can produce ferments identical with or similar to those furnished by the normal cells of the gland. Mammary adenomata within or outside the female breast may enlarge during menstruation, pregnancy and lactation, together with the normal mammary gland [5]. Mucin is observed in the alveoli of rare cases of primary carcinoma of the lung [6] and of the cervix uteri [7]. Certain tumours of the lung showing accumulation of true mucin are perhaps primary adeno-carcinomata of the bronchial mucous glands [8]. "Colloid cancer of the omentum" owes its peculiar consistency to the mucous-secreting cells of the primary cancer of the stomach or intestine from which it is derived [9]. The mucin-containing "signet-ring" cells of Krukenberg tumours of the ovaries owe their characteristic appearance to the mucous cells of the stomach, from which the primary carcinoma in such cases originates [10].

The transient waves of hardening and apparent contraction occasionally noted in uterine leiomyomata during menstrual periods may be due to contraction of the myomatous tissue of the tumour together with the normal muscular tissue of the uterine wall. Some of the most striking features of chorion-carcinoma and certain meningeal tumours may be explained by comparison with the erosive action of the normal chorionic villi in the pregnant uterus and to that of the normal arachnoid villi of the Pacchionian bodies [11]. One may also call to mind that in some instances primary so-called "multiple myeloma" of the bone-marrow has been found to consist of cells whose cytoplasm contains granules more or less resembling those in normal myelocytes [12]. I need hardly refer to the formation of cell-nests ("epithelial pearls") in some of the metastases from squamous-celled carcinoma of the skin and tongue as well as in the primary growths.

Endocrine Tumours—General Considerations.

In this paper I shall confine myself to a short consideration of the endocrine or hormonic effects of primary tumours and tumour-like hyperplasias of the various endocrine glands (so-called ductless or blood glands), as far as such effects have hitherto been definitely recognized or suggested.

Excessive bodily growth (macrosomia) and precocious or abnormal sexual development[1] had from time to time been recorded in association with suprarenal or other tumours before attention in England was concentrated on the subject by Bulloch and Sequeira's paper, in 1905, on suprarenal cortical tumours in children giving rise to precocious general growth and abnormal sexual development [13]. This was soon (1907) followed by Guthrie and Emery's writing on the same subject [14].

An important point of distinction between endocrine tumours (tumours of ductless glands) and adenomata or adeno-carcinomata of externally secreting glands is that in the former the secretions can be more or less freely absorbed into and distributed by the general circulation, just as the secretions of the endocrine glands (from which the tumours arise) are absorbed and distributed in the case of normal individuals.

It should not be forgotten that mere hyperplasia of glands (endocrine as well as other glands) due to functional over-activity and chronic irritation tends secondarily to be associated with true tumour formation, as is illustrated by primary carcinomata of the liver, arising in cases of chronic hepatic cirrhosis and by multiple papillary adenomata arising in the cortex of contracted kidneys, and by adenomata arising in hyperplastic thyroid glands. Tumours of this kind and their possible functional results (in the case of endocrine glands) should be remembered in considering the subject of endocrine tumours, and some tumours of endocrine glands may represent an adenomatous form of compensatory, or attempted compensatory, hyperplasia.

Before proceeding further it will be well to draw attention to certain other fundamental points concerning the whole subject: (1) In primary tumours and hyperplasias of endocrine glands the proliferating cells are more likely to preserve their original endocrine or hormonic powers during the early stages of the growth before they have become much altered and degenerated by rapid and malignant proliferation. (2) It must be remembered, however, that even when rapid and excessive proliferation is in progress, and though the hormonic activity of the individual tumour cells has been greatly diminished or even almost lost, the immense increase in the total number of cells, owing to malignant proliferation, may be more than sufficient to counterbalance the diminished hormonic activity of the individual cells. It is obvious that, in such cases, the total hormonic effect of the endocrine tumour may be far in excess of the corresponding normal endocrine glands. (3) Although the hormonic effect of an endocrine tumour may, perhaps, be qualitatively not exactly identical with that of the corresponding endocrine gland of a normal individual, nevertheless, on the whole, it may be regarded as the "magnified" effect and may serve to shed light on the normal hormonic functions of the endocrine gland in question. (4) The hormonic activities of certain endocrine tumours need a youthful body (one still capable of further growth and bodily development) fully to manifest themselves, and cannot give rise to the same striking manifestations of their presence when they arise in older subjects in whom normal growth and general bodily development have ceased.

Here also I would make the following note. In cases of precocious and excessive bodily growth there seems generally to be a corresponding lack of development on the mental side, and precocious excessive mental powers tend to be associated with relatively poor physical development. This is the general rule, though striking exceptions have been recorded. I believe, moreover, that there is some truth in the popular belief that in normal persons excessive continual cultivation on the physical side (athletics, etc.) is harmful for thorough intellectual development, while excessive prolonged mental study is necessarily associated with a certain lack of physical development.

1 "Macro-genito-somia" is the syndrome of abnormally large body and genitalia or precocious sexual and general bodily development.

Endocrine Suprarenal Cortical Tumours.

These tumours give rise to so-called "virilism." In females—and most cases are in females—they produce masculine features or "female virilism" [15] with hypertrichosis (hirsuties) of male type; in males they cause precocious bodily (sometimes especially muscular) and sexual development (virilism in boys or "male virilism"). In the female a tumour of the suprarenal cortex acting in early embryonic life may produce a condition of female pseudo-hermaphroditism and in adults may induce premature menopause and an abnormal condition of hypertrichosis. For references to individual cases and discussions on the subject I must refer to the writings of Guthrie and Emery [16], E. E. Glynn [17], K. H. Krabbe [18], Rolleston [19], Helmut Schmidt [20], E. Mathias [21], C. M. Kennedy and W. A. Lister [22], H. Strauss [23], and others.

For the female, according to Apert (1910) [24], a scheme of five types of the syndrome or symptom-complex may be drawn up: (*A*) Female pseudo-hermaphroditism, when the endocrine tumour produces its effect in early fœtal life; (*B*) large clitoris and hypertrichosis with hypoplastic uterus and ovaries, when the endocrine action is in later fœtal life; (*C*) abnormal development of the body and precocious puberty (adiposity and hypertrichosis), when the endocrine effect occurs before the onset of puberty: (*D*) the endocrine effect during the menstruating period is to produce amenorrhœa with adiposity and hypertrichosis: and (*E*) after the menopause the clinical picture is indistinct (adiposity, possibly metrorrhagia). An excellent picture of chronic suprarenal virilism in an adult woman with suprarenal cortical adenoma (post-mortem examination), combined with signs of functional over-activity of the suprarenal medulla (chromaffinic system), has been given by Margarete Winkel recently (1928) [25]. On the subject of the exceedingly rare cases of "true glandular hermaphroditism" and the existence of an "ovo-testis," I would refer to the writings of W. Blair Bell [26], F. von Neugebauer [27], and Reverdin [28], etc. In Reverdin's case the patient, 45 years old, had an ovo-testis on the left side and a seminoma on the right side. In a case reported by Morand and Hirchberg (Paris, 1927), a tumour of the left ovary in a girl, aged 18, was found on operation to be a typical seminoma [29].

Operative removal of an endocrine suprarenal cortical tumour has been followed by disappearance of the virillism and other symptoms in some cases, such as those of Gordon Holmes [30], A. Collett [31] (a girl, 1½ years old), and G. G. Murray and G. S. Simpson [32]. In K. V. Trubshaw's recent case the results of operation were satisfactory up to the time when the case was described [33]. At the Meeting of the Association of Physicians of Great Britain and Ireland, at Liverpool, May 25, 1928, Drs. A. D. Fordyce, W. H. Evans, and A. G. W. Owen showed a remarkable example of infantile female virilism successfully treated by operative removal of a suprarenal tumour [34].

In two young adult men, in contrast to the increased virilism noted in other cases, active mammary enlargement occurred. These were the Bittorf-Mathias case, as finally described by Professor E. Mathias, who made the post-mortem examination in 1919 [35], and the case of a man, aged 27, under the care of J. P. zum Busch, to which I, with his permission, referred in 1926 [36]. Moreover, P. Brutschy [37] described a case of male pseudo-hermaphroditism with suprarenal hyperplasia which has some bearing on the subject and is referred to by F. Spehlmann (1924) in his article on the relation of the suprarenal cortex to development of sex [38].

The suprarenal cortex is represented by a separate *interrenal organ* in selachians and rays and, therefore, the endocrine symptoms of suprarenal hyperplasia and tumours have likewise been termed "interrenalism" by E. Mathias [39], and "interrenal dystrophy" by G. Petenyi [40] and L. Puhr [41]. It seems possible (cf. Petenyi) that symptoms such as obesity and slight hypertrichosis in elderly women may be connected (almost physiologically) with the "interrenal" system.

Some degree of the clinical syndrome of interrenalism or interrenal dystrophy may occur apparently from hyperplasia and excessive functional (endocrinic) activity of the suprarenal cortical tissue without any actual tumour formation. Indeed, this may, I believe, occur in more than one member of the same family, and one might be tempted to suppose that when signs suggestive of slight "interrenalism" occur in brothers or sisters, the syndrome is not due to actual tumour formation.

Paraganglioma Sympatheticum.

The symptoms—sex characters, hirsuties, macrosomia, precocious obesity, etc.—in suprarenal cortical tumours are so different in degree in different cases that it has been suggested that there are really more than one kind, and that that is the reason why the tumours vary in their endocrine or hormonic effects on the affected subject. But probably the variety in symptoms is largely due to the individual "soil" of the patient as influenced by the age and potentiality for growth, the sex, and constitutional (possibly familial) factors [42]. Remarkably high blood-pressure was noted in certain cases [43], but in some of them this may have been due to associated arteriosclerosis or contracted kidney [44]. Moreover, there can be no doubt that with suprarenal cortical tumours there may sometimes be an associated functional over-activity of the suprarenal medulla (chromaffinic system)—as there seems to have been in the case of chronic "suprarenal virilism" in an adult married woman, aged 36 years, recently recorded by Margarete Winkel [45].

There are, however, certain tumours of adrenal medullary substance, *chromaffinic medullary hypernephromata* or suprarenal *paragangliomata* [46], which are clinically characterized by high blood-pressure, as if due to excessive endocrine secretion of adrenalin or of an adrenalin-like hormone.

It may be true that the originally suggested explanation of abnormally high blood-pressure as due to the excessive production of adrenalin by the chromaffin tissue in the body is for ordinary cases an only superficially plausible theory without any basis in facts—a "beautiful dream," as the late Dr. T. C. Janeway [47], of New York, called it. [Moreover, in elderly persons, with chronic cachexia of uncertain origin, when hidden visceral carcinoma has been thought of as the cause of the cachexia, the presence of high blood-pressure suggests that the cachexia is not due to carcinoma but to some other cause, possibly cardio-vascular disease (I hear that Professor Seyderhelm points this out to students).] Yet, it must now be acknowledged that endocrine, adrenalin-secreting tumours of the chromaffin cells of the body, do, though rarely, occur, which can give rise to excessively high blood-pressure, as one of their chief symptoms, sometimes even in the absence of any disease of the kidneys or arteries or arterioles which might be adduced to explain it. It was formerly not realized that by tumour-like hyperplasia the adrenalin-producing chromaffin cells might be so increased in number that the total production of adrenalin in the body might be enormously increased.

Edmund Neusser (Nothnagel's System) in 1898 recorded two cases, characterized clinically by high blood-pressure (systolic brachial blood-pressure reached 220 mm. Hg), in which at the post-mortem examination, instead of disease of the kidneys and arteries, a suprarenal tumour was found. In an apparently similar case Orth (1914) was able to demonstrate in the suprarenal tumour (paraganglioma) removed at the post-mortem examination the presence of a large amount of adrenalin (epinephrin). In 1909 remarkable cases of suprarenal paraganglioma were published by Wiesel (in a child, aged $2\frac{1}{2}$ years) and Bergstrand. The case of a woman, aged 28 years, with medullary hypernephroma of the suprarenal gland and paroxysmal high arterial blood-pressure, was described with post-mortem examination by Marcel Labbé, J. Tinel, and Doumer, in 1922 [48]; B. S. Oppenheimer and A. M. Fishberg (1924) refer to several cases (including the above) in a paper on the "association of suprarenal tumours with high blood-pressure" [49]; and the whole subject was

recently (1928) discussed by P. Wichels and M. Biebl in their paper on the diagnosis of paraganglioma of the suprarenal gland [50]. According to the last authors paraganglioma is a rare adrenalin-producing tumour of the suprarenal medulla, and 15 cases have been described, mostly first discovered at post-mortem examinations.

They describe three cases of their own in men between 33 and 47 years of age, with high blood-pressure (in their first case the systolic brachial blood-pressure was 230 mm. Hg) and clinical signs suggestive of contracted kidneys together with diabetes mellitus. There may also be abnormal pigmentation of the skin and mucous membranes. Quite recently the various cases, notably the French ones, bearing on the subject have been collected by Dr. Paul Lohéac (Lille) in an elaborate thesis on tumours of the suprarenal glands and their relation to high arterial blood-pressure [51].

It is possible that the abdominal tumour in the woman, aged 40 years, whose systolic brachial blood-pressure reached 220 mm. Hg, and whose case I shortly described in the *Practitioner* in 1920 [52], was a suprarenal paraganglioma, but there was no necropsy. It is likewise possible that a boy, aged 19 years, shown by Dr. E. Bellingham Smith in 1922, was a case of paraganglioma in accessory suprarenal tissue. The patient was a stunted, obese youth, with a bloated, plethoric face, a brachial systolic blood-pressure of 218 mm. Hg, and with sugar in his urine [53].

Endocrine Hyperplasias and Tumours of the Pituitary Gland.

The best-known are the eosinophil-cell hyperplasias and tumours of the anterior lobe, which produce the various features of acromegaly. But the symptoms vary in different cases, partly according to the degree and acuteness of the endocrine disturbance, and partly according to the age, sex and constitutional and local proclivities of the affected individual. Thus, in early life, before growth in length of the skeleton is completed, the specific endocrine cause of acromegaly gives rise to excessive stature (acromegalic gigantism). In some individuals, perhaps only in males, the excessive growth of the skin of the scalp causes the scalp to become too large for the calvarium which it covers, so that in order to remain fairly well adjusted the scalp has to become "corrugated" into folds and ridges, constituting one kind of the local condition known as "bull-dog scalp" or "cutis verticis gyrata" [54].

Tumours of the pituitary gland have sometimes been present in cases of precocious and abnormal sexual development, precocious obesity with coarsely coloured red plethoric-looking type of face, and, I think, in some cases with abnormal hirsuties, but into the question of the pituitary ætiological factor in such cases I cannot enter here. Other tumours of the pituitary region may of course, by their position and pressure, give rise to endocrine disorders of growth and development. Tumours and cysts derived from remnants of Rathke's pouch are probably the most frequent cause of so-called pituitary infantilism (Gilford's ateleiosis, Lorain's type of infantilism).[1]

Tumours of the Pineal Body.

According to Horrax and Bailey the presence of a pineal tumour had been verified, up to the date of their first paper (1925), in only fourteen children with precocious sexual and bodily development or "macro-genito-somia"—the "pineal syndrome" elucidated by the early work of Marburg, Frankl-Hochwart and Askanazy [55].

1 It seems that the eosinophil-cell adenomata of the pituitary anterior lobe may occasionally cause an acute instead of chronic onset of the symptoms of acromegaly, but that in cases of so-called "acute acromegaly" the eosinophil-cell tumour is more likely to be malignant, and that the term "malignant acromegaly" in such cases might be substituted for "acute acromegaly." It seems also that the basophil-cell adenomata of the anterior lobe of the pituitary gland may likewise be regarded as endocrine tumours, the eosinophil-cell tumours exercising the remarkable hormonic effect on bodily growth manifested in cases of acromegaly and the basophil-cell tumours exercising an effect on sexual development. See the writings of H. M. Evans and M. E. Simpson, etc., referred to by Professor Arthur W. M. Ellis in the discussion on this paper.

Practically all the patients were males and died early. According to Askanazy, in most such cases the pineal tumour is a teratoma. Y. Izawa [56] (1923), mainly from his observations on pineal-ectomized young fowls, concluded that the chief function of the normal pineal body is to repress premature development of the male and female sexual organs. Izawa also referred to two published human cases, in which excessive and incomplete development of the pineal body were associated respectively with retarded (Kosaka's case of a man aged 21 years) and increased (case of an idiot woman, aged 23 years, recorded by Askanazy and Brock in 1921) growth and functional activity of the sexual organs. According to this view the "pineal syndrome" might be due not to hyper-pinealism, but to hypo-pinealism, owing to destruction of the pineal body by a tumour. It seems, however, that there are certain very rare primary tumours of the pineal body which are to be regarded as true "pinealomata." Yet, the position at present is that there is no certain proof that either the normal pineal body or true "pinealomata" produce any endocrine secretion which has any influence on sexual or general bodily development. Moreover, precocious sexual development or precocious macro-genito-somia has occasionally been observed as a sequel of epidemic encephalitis (F. Stern [57], A. Wimmer, E. John [58]), or associated with chronic internal hydrocephalus [59], or other cerebral lesions. But it should be noted of course that epidemic encephalitis might damage the pineal body and its neighbourhood, and internal hydrocephalus may be secondary to a pineal tumour, or may damage the pineal body.

The part played by tumours of the pineal body in the production of macro-genito-somia has not been cleared up by Horrax and Bailey's new work [55] on "Pineal Pathology" (1928). They describe two cases of macro-genito-somia in boys, aged 9 years and 7 years respectively. In the first boy there was a pineal tumour present, but in the other boy (in whom precocious puberty and adiposity had commenced at the age of 3 years) the pineal gland was found to be microscopically normal, but there was a ganglio-neuroma, originating from the infundibular region and occupying the third ventricle.

A very remarkable case of pineal tumour in a boy, aged 4 years, was related by Oestreich and Slawyk in 1899 [60]. The boy, in addition to his precocious male sexual and general development, was remarkable for a heterologous secondary sexual character, namely, mammary enlargement. This reminds one of the mammary enlargement which has been noted in some cases of testicular teratomata (see further on) and in the two men with suprarenal tumours to which I have referred in a previous section. It likewise reminds one of a pituitary case, namely that of an acromegalic male in whom there was a mammary secretion of true milk (L. M. Davidoff) [61].

Endocrine Tumours of the Testis.

It is well known that precocious sexual and bodily development (macro-genito-somia) in boys has been sometimes associated with the presence of a tumour of the testis, and that in a certain number of these cases successful operative removal of the tumour has been followed by disappearance of the abnormal symptoms. One of the most remarkable early successful cases of the kind was that of Ercole Sacchi [62], of Genoa (1895).[1] The patient was a boy, aged 9½ years, in whom excessive general bodily growth (macrosomia) and sexual development had commenced at the age of 5½ years. The right testis was atrophic. Excision of the large left testicle, which contained a carcinoma-like tumour, was gradually followed by disappearance of the physical signs of precocity, and mentally the boy became more child-like. This seems to be the case which Dr. Serge Voronoff at the commencement of his recent lecture on "Rejuvenation" before the Cambridge University Medical Society, referred to as follows: "Another striking proof of the power of this gland on the body was that

[1] Unfortunately the subsequent history of the case is not known, and both Dr. Sacchi and Dr. Giulio Vassale, who worked with Dr. Sacchi, are dead.

of a boy born with one hypertrophied testicle and one normal one, and consequently possessing an increased internal secretion. This boy was quite abnormal, and at the age of 9 his body was that of a man of 20, while his thoughts were those of a man of 24, and he showed no interest in children of his own age. In this particular case, the parents asked a surgeon to remove the hypertrophied gland when the boy was 10.

FIG. 1.

Six months after the operation his beard dropped off, his body became that of a normal boy, and also his mind changed and he played quite naturally with children of his age" [63].

It would be interesting to know whether any of the testicular tumours in such cases could be justifiably regarded as endocrine primary tumours of the interstitial cells of the testis. Tumours, supposed to be primary tumours of the interstitial cells

of the testis, have been described by pathologists, and several are referred to in the last German edition of Eduard Kaufmann's well-known "Text-book of Pathology," but all these had occurred in adults in whom normal somatic growth had already ceased [64]. Recently, however, R. P. Rowlands, at a clinical-pathological meeting of the Section of Surgery of the Royal Society of Medicine, on April 27, 1927, showed a testicular tumour which he had removed from a boy (F. P.), aged 9 years, with precocious macro-genito-somia. The case was not described in the Society's *Proceedings*, but Mr. Rowlands kindly gave me permission to refer to it here, with illustrations (figs. 1 and 2). Up to the age of 6 years nothing abnormal had been noticed, but then the boy began to grow rapidly; his voice "broke" at 7 years, and at 8 years he had attained puberty. On admission (when 9 years old) in April, 1927,

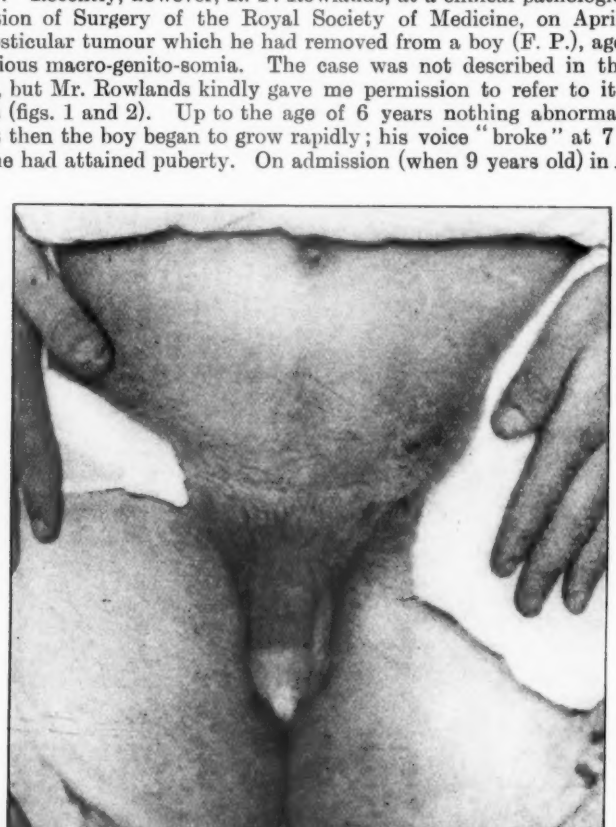


FIG. 2.

for a swelling of the left testicle and premature puberty, the patient in bodily development appeared to be a full-grown man (see figs. 1 and 2). Unlike many other subjects of premature puberty he was of average intelligence for his age, and not vicious. The left testis was removed entire with its coverings and sent to Professor Adrian Stokes and Dr. G. W. Nicholson, who examined the growth microscopically and reported that it was a growth arising in the interstitial cells of the testis, and was probably malignant. The tumour-cells were large and appeared very active. The boy did well after operation, and his subsequent development will be interesting.

Before leaving the subject of testicular tumours it may be remarked that true mammary hypertrophy may sometimes follow the development of a testicular

teratoma, as in a case of Hamilton Bailey [65]. Various examples of this are cited by H. W. B. Cairns [66] from his own series of testicular tumours and from the literature on the subject [67]. In three of the cases (those of Cooke, Warthin, and Malloch) chorion-carcinoma was present in the testis.[1] It appears, indeed, that enlargement of one or both male mammæ may be an occasional result of a variety of endocrine disturbances and must not be regarded as a pathognomic sign of disease of any single endocrine organ [68]. Apart from the not very infrequent spontaneous developmental unilateral or bilateral gynæcomastia, mostly arising at about the period of puberty [68A], and gynæcomastia from atrophy or destruction of the testes from any cause in young adults, enlargement of one or both breasts has occasionally followed mere traumatism of one or both testes in young men [69], and in elderly men it has occasionally followed the operation of prostatectomy, as recorded by E. Kondoleon [70], Petit de la Villéon [71], and R. Oppenheimer [72], so that the existence of a special hormonic endocrine function of the prostate has been surmised by some. Oppenheimer's two cases were in men of 64 and 48 years respectively. Kondoleon's two patients were both aged 70 years. In one of them the enlargement was of the right breast only, attaining the size of a walnut, but subsiding gradually after a few months. Petit de la Villéon's patient was a man, aged 73 years, in whom very decided bilateral gynæcomastia had followed prostatectomy, and in one of the enlarged mammæ a carcinoma had subsequently developed. Mammary enlargement in males has likewise been observed as a very rare accompaniment of suprarenal and pineal tumours and the specific hyperpituitarism that gives rise to acromegaly, as I have noted in previous sections. Moreover, gynæcomastia has been noted by Silvestrini, Corda, and then A. Tattoni [73], as an accompaniment of hepatic cirrhosis. This rare combination has also been recorded independently by Hamilton Bailey [74] but in a somewhat complicated case.

Endocrine Tumours of the Ovary.

It would be useless here to try to refer to all the cases of ovarian tumour, sometimes malignant, described as teratoma, sarcoma, etc., which have been recorded as associated with premature sexual and bodily development (macro-genito-somia), or with female pseudo-hermaphroditism or virilism. Sometimes surgical removal of the tumour has been followed by normal development. One of the earliest cases of the kind was that described in 1888 by Clement Lucas at the Clinical Society of London [75]. Lucas performed oophorectomy for a tumour of the right ovary in a girl, aged 7, and the signs of precocious puberty gradually disappeared after the operation. A most remarkable and successful result in a case of macro-genito-somia was that of R. H. Harris [75A], in which he removed a teratoma of the right ovary containing adeno-carcinomatous tissue. Another successful result was that recorded by A. H. Southam [75B] ; the removal of a growth which microscopically seemed to be a right ovarian sarcoma, from a girl in her third year of life with sexual precocity, was followed by normal sexual development without recurrence of the tumour.

It would be interesting to know whether in any of the cases the neoplasm was really a primary tumour derived from endocrine "interstitial" cells. Similar precocity of sexual and general development seems to have been due to ovarian hyperplasia (? with functional over-activity) without any actual tumour-formation.

In 1927 Dr. J. W. McNee kindly showed me a remarkable case of macro-genito-somia in a girl, aged 13 years, who looked like a rather stout woman of 25 years. Her height was 5 ft. 6 in., and her weight was 14½ st. Menstruation had commenced at 11 years of age. Her general osseous development, according to Dr. H. A. Harris, corresponded to that of the age of 18 or 19 years. There were some cutaneous

[1] Mr. D. M. Greig has kindly shown me photographs of a young male Kaffir with mammæ equal to the female breasts painted by Rubens. Unfortunately, Mr. Greig could obtain no other definite information about the man, excepting that there was a tumour in the left scrotum, apparently not a testicular one.

"striæ" over the hips and lateral abdominal regions. Mental development was normal. She appears to me to be the female analogue or counterpart of Mr. R. P. Rowlands's male case (see above), but no abdominal tumour can be felt by ordinary palpation (which is hindered by the thickness of the abdominal walls).

In adult patients it has been suggested (compare Adolf Bingel [76]) that ovarian "luteomata" (lutein-cell tumours) may exert the supposed endocrine action which has been attributed to the normal corpus luteum and the abnormally persistent corpus luteum, that is to say, that they may inhibit menstruation.

Endocrine Tumours of the Pancreas (*Primary Tumours of Insular Tissues*).

Primary pancreatic carcinoma is sometimes, owing to microscopic appearances, supposed to have originated in the tissue of the islands of Langerhans. If such were the case one might expect that during life the actively proliferating insular cells, if they retained the insulin-secreting function of the islet cells from which they originated, would produce excess of insulin ("hyper-insulinism") and that the patient would have been subject to spontaneous hypo-glycæmic crises (with weakness, convulsions, drowsiness or coma), relieved by the intravenous injection of glucose or prevented by frequently eating sugar. This was exactly what was observed in a medical man, aged 40, whose case has been published by Wilder, Allan, Power and Robertson (of the Mayo Clinic, Rochester, U.S.A.) [77]. During one of his hypoglycæmic attacks his blood-sugar was found to be only 0·002 per cent. He died during one of the attacks, and the necropsy showed diffuse cancer of the pancreas with hepatic metastasis. Microscopically, the cancerous tissue resembled that of the islands of Langerhans, and experimental evidence in regard to the cancerous and non-cancerous portions of the liver seemed to show the presence of excess of insulin in the former. The authors regard the case as one of primary pancreatic carcinoma, originating in the islands of Langerhans, with resulting "hyper-insulinism" and spontaneous hypo-glycæmic attacks.

Another case (to which Professor Arthur Ellis has kindly referred me) of insular pancreatic tumour with spontaneous hypoglycæmic attacks was reported by W. Thalhimer and F. D. Murphy, in July, 1928 [77A]. A woman, aged 57 years, had for the last $2\frac{1}{2}$ years been subject to spontaneous attacks of somnolence followed by great restlessness and irritability. Gradually she developed an almost constantly stuporous state, with frequent hypoglycæmic convulsions followed by coma, after one of which attacks she died. At the necropsy the only important lesion found was a single small primary tumour of the pancreas, the microscopic appearance of which suggested that it was an adenoma or a carcinoma of low malignancy, which had originated from cells of the islands of Langerhans.

Since then, yet another case of "Adenoma of the Islands of Langerhans with Associated Hypoglycæmia" has been reported by W. U. McClenahan and G. W. Norris [77B]. The patient, a coloured male, aged 41 years, was subject to attacks which consisted essentially of loss of memory (coming on gradually), and were relieved by eating candy-sugar and by meals, but on one occasion, when he became comatose, the coma persisted and fatal bronchopneumonia ensued. The necropsy showed the pancreatic lesion to consist of a large circumscribed nodule (adenoma) which was found to be wholly composed of insular cells, the islands of Langerhans in the remainder of the pancreas being also hyperplastic. A new disease or morbid syndrome is established by the publication of the above cases and by a consideration of the problems of hypoglycæmia and hyperinsulinism or hyperinsulinæmia. Amongst the valuable work referred to in this connexion are the papers by S. Harris [77C] on "Hyperinsulinism and Dysinsulinism," and by S. H. Gray and L. C. Feemster [77D] on "Compensatory Hypertrophy and Hyperplasia of the Islands of Langerhans in the Pancreas of a Child Born of a Diabetic Mother."

Endocrine Tumours of the Thyroid Gland.

As an illustration I shall confine myself to the classical observation published by von Eiselsberg in 1894, after he had left Billroth's clinic in Vienna and when he was Professor of Surgery in Utrecht. The patient was a woman, aged 38 years in 1886, when Billroth excised her enlarged thyroid gland. The operation was followed by myxœdema. Subsequently a tumour appeared in the upper part of the sternum, and as it increased in size, the myxœdema disappeared. In 1892, the sternal tumour, which was then as large as a fist and appeared to be malignant, was removed by von Eiselsberg and was found to contain colloid material, as a thyroid tumour does. The woman recovered from the operation, but the myxœdematous symptoms recurred in the same order as after the original thyroidectomy operation, and they persisted [78]. In this connexion S. G. Shattock's earlier paper (1888) on a similar subject may be compared. The case described by him was one of "Hypertrophy of an Accessory Thyroid in a case of Spindle-celled Sarcoma of the Thyroid" [79]. Eiselsberg's paper has an important position in the history of the study of endocrine tumours. It was known to and quoted by Sacchi, when he wrote the account of his famous testicular case in 1895.

Hyperplasias and Endocrine Tumours of the Parathyroid Gland.

Tumours or tumour-like hyperplasias have been reported in various cases of osteitis deformans and osteomalacia [80] and Recklinghausen's generalized type of osteitis fibrosa [81]. The influence of the parathyroid glands on calcium metabolism makes the existence of hyperplasia and endocrine tumours of these glands not improbable in connexion with grave disorders of the calcium metabolism [82]. Some of the tumours may represent an adenomatous form of, or an attempt at, compensatory hyperplasia.

The Thymus Gland.

Primary tumours arising in the thymus have been called "thymomata," and most of them are probably lympho-sarcomatous in nature. Some of them constitute a mediastinal form of leuko-sarcomatosis (Sternberg) and are associated with a leukæmic blood-picture [83]. The thymus gland has been supposed normally to exert a restraining and regulating effect on sexual and bodily development before puberty. It is doubtful whether any tumours of the thymus give rise to endocrine symptoms, though tumour or hyperplasia or abnormal persistence of the thymus has been found associated with various endocrine disorders of growth in childhood and youth, as in two cases recently communicated by Dr. O. Leyton [84] to the Annual Meeting of the Association of Physicians of Great Britain and Ireland in May, 1928, under the heading, "Two Cases of Pluriglandular Disturbance associated with Tumour in the Thymus Gland." One of Leyton's patients was a boy, the other was an adult man.

The Spleen and Reticulo-endothelial Apparatus.

In order to recognize what may be the symptoms of functional over-activity of the spleen and reticulo-endothelial apparatus of the body—which condition may shortly be called "hyper-splenism" by analogy with the terms "hyper-thyroidism" and "hyper-pituitarism"—we must study the symptoms of diseases in which the spleen is usually enlarged, and which are known to be benefited by splenectomy; for instance, congenital or familial hæmolytic jaundice and thrombocytopenic purpura. Anæmia, which in hæmolytic jaundice, can be removed by splenectomy, tends also to a less extent to be present in Gaucher's disease, and one view of the nature of Gaucher's disease is that it represents a primary generalized tumour-formation of the spleen and reticulo-endothelial apparatus; in some atypical cases, I believe, there may even be tumour-like nodules of the "Gaucher cells." Gaucher's disease—if it is really accompanied by symptoms of hyper-splenism, and if this

hyper-splenism is partially hormonic in nature—may, therefore, be regarded as a kind of endocrine tumour-formation. This view is not altogether at variance with the constitutional familial metabolic explanation of the peculiar infiltration of the cytoplasm of the "Gaucher cells." But the whole question is much too problematical for discussion here. The peculiar type of primary splenomegaly, described by Foix and Roemmele in 1912 [85] seems to be due to a nodular tumour-like growth, arising from the Malpighian corpuscles throughout the spleen, accompanied by anæmia, thrombocytopenia and tendency to hæmorrhages. The apparent evidence of hyper-splenism is removed by splenectomy. Dr. Joan M. Ross [86] demonstrated a large spleen of this kind successfully removed from a middle-aged woman. It is tempting to suggest that this rare form of disease is a kind of primary endocrine tumour-formation of the reticulo-endothelial tissue of the spleen (with hyper-splenism). In regard to the question of the existence of splenic endocrine tumours no hint can apparently be obtained from the important part played by the normal spleen in ossification and in callus-formation after fractures and in helping the body to resist infections.

Note in Conclusion.—When one bears in mind that an affection of one endocrine gland must almost necessarily cause reaction in others, it does not seem surprising that the clinical picture in cases of endocrine tumours may sometimes be obscured by pluriglandular symptoms. Moreover, precocious bodily growth (macrosomia) combined in most cases with precocious sexual development (macro-genito-somia) may be caused by endocrine tumours of the suprarenal cortex, of the anterior lobe of the pituitary gland, of the pineal gland possibly, and of the interstitial cells of the testis, and likewise by some ovarian tumours. So also, as I have pointed out in a preceding section (see under "Endocrine Tumours of the Testis"), active enlargement of the male mamma may result from endocrine tumours or other disturbances of various endocrine glands. It must, moreover, be remembered that *non-endocrine tumours* may cause endocrine symptoms by destroying, or possibly even by stimulating endocrine glands; and that (as previously remarked) some tumours of endocrine glands may represent an *adenomatous form of compensatory, or attempted compensatory hyperplasia.*

Indeed, it seems that functionally active primary tumours and tumour-like hyperplasias of the various endocrine glands might be subdivided into: (*a*) Those which arise as a pathological manifestation of a vital compensatory reaction, on the part of the body, to counteract a specific hormonic deficiency; (*b*) those which I have especially considered in the present communication, namely, those which arise independently of any obvious need or stimulation, and are the cause of a functional hormonic over-activity, which in most cases must be injurious rather than beneficial to the rest of the body. In both (*a*) and (*b*) secondary symptoms (including further endocrine symptoms) may be produced by the local pressure or irritation and active hyperæmia, and moreover malignancy may supervene. Non-endocrine tumours may also, by local irritation (and the active hyperæmia that they cause), pressure, or destruction, give rise to endocrine symptoms.

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[3] On this whole subject see, for instance, the early writing of M B. SCHMIDT, "Ueber Secretionsvorgänge in Krebsen der Schilddrüse und der Leber und ihren Metastasen," *Virchow's Archiv*, Berlin, 1897, cxlviii, pp. 43-92; and Sir JOHN BLAND-SUTTON, "The Habits (Oecology) of Tumours," *Brit. Med. Journ.*, 1923 (ii), pp. 847-852.

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[6] Sir BERNARD SPILSBURY, "Case of Primary Carcinoma of the Lung," *Trans. Med. Soc. Lond.*, 1924, xlvii, p. 86.
[7] REEB, *Gynécol. et Obstét.*, Paris, January, 1928, p. 1; Abstract in *Brit. Med. Journ.*, 1928 (i), epit. No. 431.
[8] Compare Dr. NORAH SCHUSTER'S communication on "Bilateral Mucous Carcinoma of the Lungs," at the Section of Pathology of the Royal Society of Medicine, on November 20, 1928.
[9] See BLAND-SUTTON, *loc. cit.*
[10] Cf. W. SHAW, *Proc. Roy. Soc. Med.* (Sect. of Obst. and Gyn.), 1926, xix, p. 49.
[11] BLAND-SUTTON, *loc. cit.*
[12] References to the literature on such cases were given by Weber and Ledingham, *Proc. Roy. Soc Med.*, 1909, ii (Sect. Path.), p. 206.
[13] BULLOCH and SEQUEIRA, *Trans. Path. Soc. Lond.*, 1905, lvi, p. 189.
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[86] JOAN M. ROSS, "A Case of Splenomegaly of the Foix and Roemmele Type," demonstrated at the meeting of the Pathological Society of Great Britain and Ireland, on January 4, 1929 (not yet published).

Discussion.—Dr. LANGDON BROWN described a case of virilism in a woman, aged 42, whose first symptom was an obsession that she was turning into a man. This led to the finding of a malignant tumour of the adrenal cortex, which was removed by Mr. Girling Ball. The patient, however, unfortunately succumbed to an attack of acute hyperthyroidism, a point which in view of the feminizing influence of the thyroid, was in itself interesting, as if the thyroid had been trying to compensate for the over-acting adrenal cortex. He (Dr. Langdon Brown) next referred to the type described by Achard and Thiers as the diabetes of bearded women, which might be due to tumours either of the adrenal cortex or of the anterior lobe of the pituitary, or to a lesion in both of these structures. He had seen several cases, one of them through the courtesy of Dr. Parkes Weber, and he believed that a similar condition occurred in men, but was not so likely to be recognized because virilism was still quite common in males. A young man recently under his care presented all the stigmata of this syndrome—a high blood-pressure, albuminuria, glycosuria, obesity and large cutaneous striæ. Another physician who saw this patient was convinced that he had an adrenal cortical tumour, but this was not found at operation. The skiagram of the pituitary fossa was normal. The patient had recently died from acute pulmonary œdema, as did the female patient in one of Dr. Parkes Weber's cases. Unfortunately there was no post-mortem. He was sure that pituitary disorders might produce a local overgrowth, and that, as stated by Sir A. Keith, such overgrowth affected all the structural and not merely the bones in that part. He described an example in a boy. As to pineal tumours, it was striking to note how closely similar some of their effects were to those of adrenal cortical tumours, except that the sexual precocity did not always tend to virilism. There was also usually some disturbance of ocular movements and mental alteration. He described a case of a girl with such signs, who reached sexual maturity at the age of 9. He agreed with Lewellys Barker that the pineal probably did not belong to the endocrine system at all, but produced its effects through the central nervous system, with which it was so closely related. With regard to splenic tumours, he had had a case of splenic anæmia in which the excised spleen showed Gaucher's disease. He took McNee's view that splenic anæmia was a hæmolytic process which might result from various conditions stimulating overgrowth of reticulo-endothelial tissue, and that Gaucher's disease was one of these and therefore came within Dr. Parkes Weber's definition of endocrine tumours.

Professor ARTHUR ELLIS said that there was a second reported case of tumour of the islands of Langerhans, associated with attacks of hypoglycæmic coma. This case differed from the one reported by Wilder, in which there were secondary deposits in the liver—in showing only a single small tumour in the body of the pancreas [1].

Had Dr. Parkes Weber ever seen, or found reported in the literature, examples of precocious sexual development, associated with tumours of the anterior lobe of the pituitary gland? The work of Long and Evans [2], Zondek and Ascheim [3] and P. E. Smith [4] had established the presence of two hormones from this structure, one stimulating growth and the other sexual development, the growth hormone acting antagonistically and inhibiting the effect of the sex hormone. L. F. Hewitt [5], working at the London Hospital, had recently succeeded in separating these two hormones from extracts of the anterior lobe.

Evans and Simpson [6] had suggested that the growth hormone was produced by the eosinophilic cells of the anterior lobe and the sex hormone by the basophilic cells. Bailey and Cushing [7] had shown that adenomas of the anterior lobe of the pituitary might be classified according to the predominance of eosinophilic granules in the cells of the tumour, acromegalic symptoms being most strongly marked in those cases in which eosinophilic cells predominated. If Evans' theory that the sex hormone came from the basophilic cells were correct, one would have expected that cases of chromophobe adenoma would have shown sexual precocity. Such, however, was not the case. On the contrary, patients with chromophobe adenoma showed either no sexual change, or evidence of diminished sexual activity. This paradox might, perhaps, be explained by the fact that

chromophobe adenomata usually occurred in adult life, that is after sexual development was already complete, or it might be due to antagonism of the sex hormone by the growth factor. One would have expected, however, that if the basophilic cells were responsible for the sex hormone, some case of chromophobe adenoma would have occurred in which the conditions would have been favourable to the demonstration of their active principle. He would therefore be greatly interested to hear if Dr. Parkes Weber had ever encountered sexual precocity associated with pituitary anterior lobe tumour.

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Dr. PARKES WEBER said he could only reply by thanking those who had taken part in the discussion for the additional information which they had supplied. He knew of disturbance in sexual development associated with a basophilic adenoma in the anterior lobe of the pituitary gland. A minute, rounded adenomatous nodule (3 by 4½ mm. in the hardened sections) of basophilic cells in alveolar arrangement was present in the anterior lobe in a woman, aged 28 years, whose case he described in a paper ("Cutaneous Striæ, etc," *British Journal of Dermatology and Syphilis*, 1926, xxxviii, pp. 1-19).

ADDENDUM (February 9, 1929).—An interesting paper has just appeared by H. VAQUEZ, E. DOUZELOT, and E. GERANDEL (*Presse médicale*, Paris, 1929, xxxvii, pp. 169-173), in which the association of paroxysmal or permanent high blood-pressure with certain suprarenal medullary tumours is considered.—F.P.W.

Section of Neurology.

[*December* 13, 1928.]

A Note on the Association of Extensive Hæmangiomatous Nævus of the Skin with Cerebral (Meningeal) Hæmangioma, especially Cases of Facial Vascular Nævus with Contralateral Hemiplegia.

By F. Parkes Weber, M.D.

Abstract.—The main type of the cases under consideration is that in which extensive capillary nævus of the skin, especially of trigeminal distribution, is associated with contralateral spastic hemiplegia, developing in early life, probably sometimes connected with intracranial hæmorrhage from the abnormal blood-vessels of a diffuse leptomeningeal angioma on the same side as the main vascular nævus of the face. Various points related to such cases are referred to.

According to Harvey Cushing (1928) blood-vessel tumours contribute only about two per cent. of all intracranial tumours. In many cases in which the existence of a hæmangiomatous condition of the cerebral meninges has been proved by operation or post-mortem examination there has been no large hæmangiomatous nævus of the skin to suggest the nature of the cerebral disease. On the other hand, in the patients in whom epilepsy, especially of the Jacksonian type (irritative lesion), or spastic hemiplegia (paralytic lesion), or both, developed in early life the hæmangiomatous nature of the intracranial disease has been sometimes suggested by the presence of extensive hæmangiomatous nævus of the skin, especially of the face (more or less trigeminal distribution) on the same side as the intracranial irritative or paralytic lesion.[1]

Such cases have been recorded by S. Kalischer, Lannois and Bernoud (two cases), Strominger, Harvey Cushing (three cases), E. A. Cockayne, Mouriquand and Bernheim, C. P. Symonds, Brushfield and Wyatt (four cases), H. J. Hugo, and T. R. Aynsley (two cases). In D. M. Greig's case (to which I shall return) instead of unilateral or asymmetrical capillary nævus there was bilateral symmetrical multiple adenoma sebaceum of the face, of Pringle's telangiectatic type. In some of these cases the nature of the intracranial disease was elucidated by a subsequent post-mortem examination or by operation.

Cushing's first case was that of a boy, aged 4, with extensive capillary nævus of the face (especially of the right side) and unilateral right-sided buphthalmus. He had developed left-sided spastic hemiplegia after what Cushing supposed to have been an attack of right-sided intracranial hæmorrhage at 2 years of age. He died from some severe intercurrent infection and the necropsy showed that the right cerebral hemisphere was smaller than the left one, and the meninges covering it were unduly vascular, "in all probability representing a nævoid condition similar to that present on the skin" (Cushing).

Cushing's second case was a boy, aged 5 years, with extensive capillary nævus of the upper right part of the face and head and unilateral right-sided buphthalmus. There were epileptiform convulsions after an attack of what was supposed to have been intracranial hæmorrhage. Improvement followed right-sided craniotomy after ligation of the external carotid artery. A very vascular condition of the dura mater was found at the operation with adhesions to the leptomeninges.

Cushing's third case was a left-handed boy, aged 8 years, with extensive capillary nævus, involving especially the right side of the face and right upper extremity; there was unilateral right-sided buphthalmus. Left-sided spastic hemiplegia, epilepsy and idiocy followed what was supposed to have been an attack of right-sided intracranial hæmorrhage at 9 months of age. No operation was attempted and the nature of the intracranial lesion could only be presumed.

[1] In two or three cases the facial nævus has been partly pigmentary as well as hæmangiomatous.

In Strominger's case, as quoted by Cushing, there was extensive left-sided facial capillary nævus, with spastic hemiplegia (presumably of the right side). Death occurred from some intercurrent affection and at the necropsy the cerebral meninges (presumably on the left side) were found thickened and excessively vascular.

In Kalischer's case the patient died at 1½ years from bronchopneumonia after whooping-cough. There was an extensive capillary nævus of the left half of the face. Convulsions commencing on the right side had been observed since the age of 6 months, and for the last quarter year of its life the child had had right hemiplegia. The necropsy showed that the left cerebral hemisphere was smaller than the right and was covered by a diffuse meningeal angioma. The dura mater was very adherent to the skull, and the left frontal bone was greatly thickened and very adherent to the scalp (the side of the capillary nævus). Up to the age of 6 months the child had seemed normal in regard to the nervous system. But it does not seem certain that the subsequent epileptic attacks (of the Jacksonian type) and the ultimate right spastic hemiplegia were due to intracranial hæmorrhage from the meningeal angioma on the left side.

A left-sided meningeal angioma was found at the necropsy of one of Brushfield and Wyatt's cases (Case 3), and there were other cerebral defects noted. Congenital spastic hemiplegia had been present on the right side.

In Hugo's case there were angio-endotheliomata found in the brain, kidneys and liver, but apparently there was no typical meningeal angioma.

In Steinheil's case, quoted by Kalischer and others, the patient was a man, aged 49, who had no typical vascular nævus of the skin, but, since childhood, there had been aneurysm-like enlargements and ecstasies of the right frontal, temporal and external carotid arteries. He had had epileptic attacks for years before his death, and at the necropsy some of the arteries of the brain were found enlarged and there was a racemose arterial angioma of the front third of the right frontal lobe. In this case, though typical capillary nævus of the skin was apparently absent, there were developmental extra-cranial arterial abnormalities on the same side as the racemose arterial angioma of the brain. The case possesses a further special point of interest, in so far as it tends to show that meningeal angioma may be associated in the same patient with another kind of congenital or developmental arterial abnormality in the brain or other parts of the body, namely, "weak spots" in the arterial walls, an occasional cause of developmental or so-called congenital (sometimes actually congenital) aneurysms in otherwise healthy young individuals.

In Greig's case with the multiple adenoma sebaceum, the patient was a man, aged 18, the subject of convulsions for fifteen years, and of right hemiplegia for eleven years. The convulsions were of the type of Jacksonian epilepsy, commencing in the right hand, and operation showed a meningeal angioma covering the left Rolandic area. The sebaceous adenomata of the face might well have led one to suppose that the cerebral disease was of the nature of tuberous sclerosis, but it would have had to be *unilateral* tuberous sclerosis.[2]

I described the following case in 1922, when I wrote: "It is highly probable that the congenital cerebral disease is in some way connected with the presence of a vascular nævus of the meninges or brain on the left side—of the same nature as the extensive vascular nævus of the patient's body." But I have since (summer, 1928) obtained better skiagrams of the patient's head and, as a result, it may now be stated with practical certainty that the disease of the left side of the brain is of the nature of a diffuse meningeal angioma, comparable with the condition which was discovered post-mortem on the left side of the brain in Kalischer's case. This case of mine is, I think, the only one as yet described in which so great a difference between the two sides of the brain has been demonstrated by X-ray examination

2 Incidentally, Drs. G. Brown and T. W. Sheldon have shown me that partially calcified foci of tuberous sclerosis in the brain may give radiographic shadows.

during life, without the aid of injection of air, etc.[3] The radiograms, which were poorly reproduced in my paper of 1922, were the first reported of the kind. Similar unilateral cerebral changes, but not quite so marked or extensive, have been demonstrated by radiograms in V. Dimitri's case (1923), in A. M. Marque's two cases (1927), in No. 2 of Brushfield and Wyatt's cases, in T. W. Sheldon's case, and in certain, as yet, unpublished cases (including cases of L. B. Rawling and C. P. Symonds).[4]

The patient, R. R., a woman, now aged 28, is of a Hebrew family. Her mother is active-looking, well-developed, and has a moderate degree of xanthelasma

FIG. 1.

palpebrarum. A sister of the patient is said to have become insane "after air-raids." The mother says she has also two healthy normal children; none have died or were stillborn; she had two or three miscarriages. The patient has very widespread capillary nævus, chiefly of the superficial "port-wine stain" type (fig. 1). On the back of the trunk this port-wine angioma is almost entirely limited by the median line to the left side; in front the distribution, though very irregular, is more extensive on the left than on the right side. In parts, notably in the left cheek,

[3] The radiograms have been reproduced in figs. 2 and 3 without any touching up.
[4] *Cf.* H. Cushing and P. Bailey, "Tumours Arising from the Blood-vessels of the Brain," 1928, p. 26

there is, besides the "port-wine staining," a condition of more deeply seated thickening, though possibly also of angiomatous nature. She has right-sided spastic hemiplegia, apparently of congenital or almost congenital origin. The right limbs are smaller and notably shorter than the left limbs; in fact, there is a condition of unilateral right-sided hypotrophy, as one would expect, associated with the right-sided spastic hemiplegia.

The mammary regions are bulky, doubtless owing to her general obesity. But in 1922 I wrote: there is absence of pubic and axillary hair, and she has never menstruated; the sexual organs (rectal examination by Dr. Scheu) are probably infantile.

The left eyeball (1922) was larger than the right owing to congenital glaucoma or buphthalmus. Dr. C. Markus reported that the right eye was normal, but that there was atrophy and glaucomatous excavation of the left optic disc. She was blind in the left eye. There was so-called heterochromia iridis, the upper three-quarters of the left iris being brown, the lower quarter-sector of the left iris and the whole of the right iris being grey-blue. In August, 1926, Dr. Markus had to excise the left (buphthalmic and blind) eye on account of purulent corneal ulceration and severe pain. By macroscopic and microscopic examination of the excised eye there was no evidence of any retinal or other angioma-formation in it.

The patient is certainly not an idiot, although there may be some mental deficiency. She can speak well and in conversation seems not to be deficient in understanding simple matters and in ordinary emotions; she has never been to school, has not learned to read and write, and on account of the great paresis in her right upper extremity, can do only a little housework. There is much less paresis in the right lower limb than in the upper one. The knee-jerks are very active on both sides. The plantar reflex (1922) on the left side is of the normal flexor type; that on the right side is of the extensor type.

Whilst the patient was under observation in hospital in 1922 there were no convulsions, nor was there apparently a history of any kind of fits. Her pulse was 68 to 88, and her respiration was 24 per minute. Her brachial systolic blood-pressure was 115 mm. Hg. The blood-serum gave a completely negative Wassermann reaction. The blood-count showed slight excess of white corpuscles. The urine (quantity was about normal), when tested, was of specific gravity 1025; acid; free from sugar; containing a trace of albumin (probably due to presence of some vaginal discharge). No alimentary glycosuria followed the ingestion of 100 grm. dextrose; but the urine contained a trace of sugar after a meal of 200 grm. dextrose.

An X-ray examination of the head was made with Dr. James Metcalfe's help in 1922. The skull showed marked prognathism of the "anthropoid" or "simian" type; that is to say that the prognathism involved a projection of both jaws beyond the vertical line of the ideal orthognathous skull—a character of the lower races of mankind and anthropoid apes, not to be confused with the so-called "inferior prognathism" which is sometimes a family characteristic. The pituitary fossa was extremely small or infantile, a feature that might be considered in connexion with the causation of the patient's obesity and defective sexual development (see above). The left cerebral hemisphere appeared sclerosed; at all events, it was more opaque and gave a somewhat deeper shadow than the right hemisphere. It seemed to occupy only about two-thirds of the left half of the cranial cavity and to be surrounded by cerebro-spinal fluid (external hydrocephalus). But skiagrams of the skull, taken in June, 1928 (figs. 2 and 3), whilst confirming the findings of 1922 in regard to the prognathism and size of the pituitary fossa, have been much more successful in demonstrating the abnormal condition of the left side of the brain. The whole of the left side, excluding the cerebellum, appears to be small, occupying only part of the cranial cavity, as if sclerosed or bound down by thickened or partially calcified leptomeninges. The skiagrams strongly suggest that the left

FIG. 2.—Dr. Weber's Case: Lateral Skiagram, to show the intracranial calcification (description in the text).

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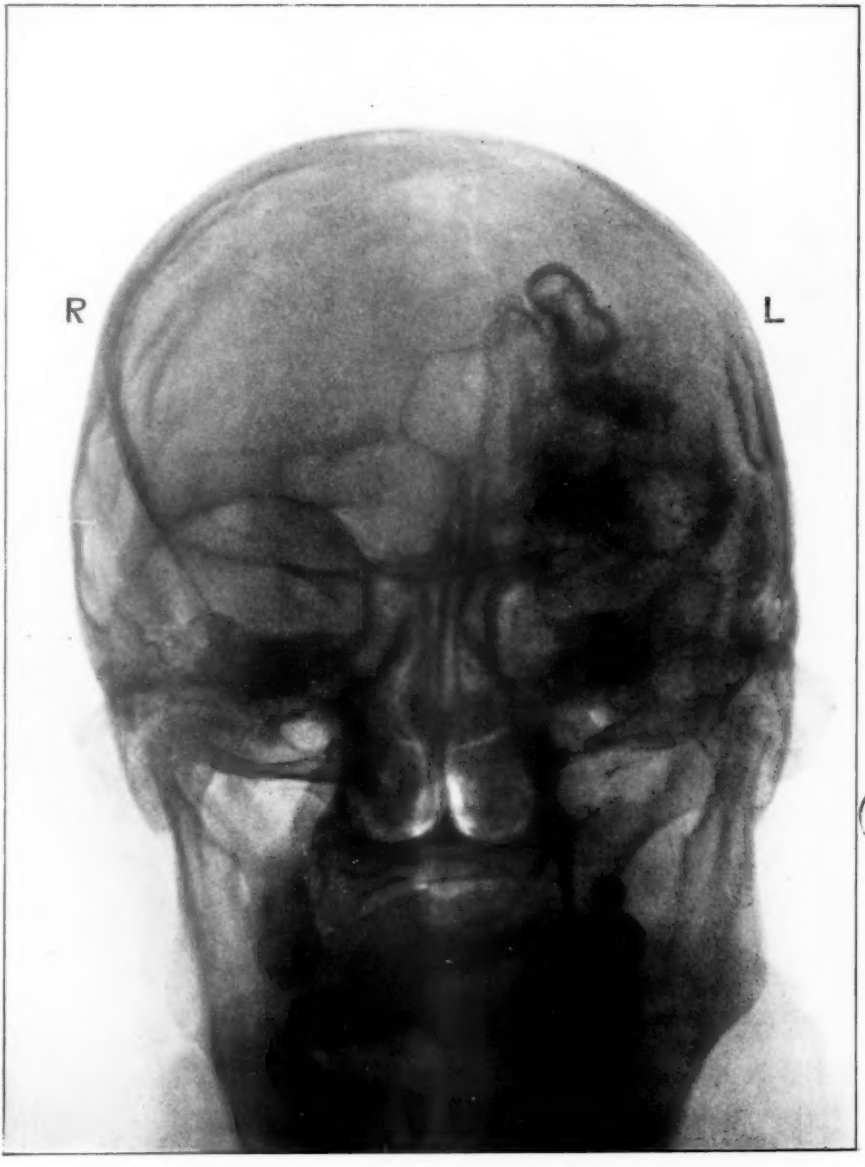

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FIG. 3.—Dr. Weber's Case: Antero-posterior Skiagram (description in the text).

cerebral hemisphere is in a condition analogous to that of the left hemisphere discovered on post-mortem examination in Kalischer's case; i.e., smaller than the right hemisphere and bound down by abnormal leptomeninges, the site of diffuse angiectatic hæmangioma. In the present case there is probably some calcareous deposit in the vessels of the meningeal angioma, as there was in Nielsen's recent case of racemose arterial angioma of the inferior cerebellar artery in a man aged 43. The shadowing, apparently due to calcareous deposit, does not extend beyond the tentorium cerebelli, and there is no radiographic sign that the cerebellum is affected. The calcareous deposit may be in the walls of angiectatic vessels or in their thrombosed channels, or it may be extravascular and a result of hæmorrhages or thrombotic necroses.

The lateral radiogram of the posterior portion of the head in No. 2 of Brushfield

FIG. 4.—Lateral skiagram of the posterior part of the brain in Brushfield and Wyatt's second case, kindly furnished by Dr. Brushfield. It shows typical appearance of a partially calcified leptomeningeal racemose aneurysm in the occipital region, above the tentorium cerebelli.

and Wyatt's cases (a girl, aged 11) I am able to illustrate here (fig. 4) by the kindness of Dr. Brushfield. In all cases of large facial hæmangioma a radiogram of the brain should be taken, especially if any operation on the face be contemplated, but in the interpretation it must be remembered that (deep) facial as well as intracranial hæmangiomata may be partially calcified.

Unilateral congenital glaucoma on the side of the main capillary nævus of the skin and the cerebral disease has been found in other similar cases, namely in all three of Cushing's, two of Brushfield and Wyatt's (their first and second), and both of Aynsley's cases. Galezowski, in 1898, showed a girl, aged 9 months, in whom extensive capillary nævus of the face was associated with unilateral left-sided buphthalmus. It would be interesting to know if signs of unilateral (left-sided)

intracranial disease subsequently developed. In all Brushfield and Wyatt's cases, and in those of Hugo and Aynsley, and one of Cushing's there was idiocy or imbecility. There may be mental deficiency, but there is not idiocy in my case.

Direct or indirect involvement of the basal region in the neighbourhood of the pituitary gland in my case may explain the obesity and defect in sexual development. A combination of obesity with infantile hæmiplegia (Babonneix, Hutinel and Widiez) might sometimes be explained in the same way.

Prognathism of the anthropoid type has, I believe, not been shown by skiagrams in other cases, excepting in one of Brushfield and Wyatt's cases (Brushfield, *Proc. Roy. Soc. Med.*, 1929, xxii (Sect. Dis. in Child.), p. 13, discussion).

Unilateral intracranial hæmorrhage from abnormal blood-vessels connected with the meningeal angiomatous condition, occurring in early or in intra-uterine life, may have been the original cause of the spastic hemiplegia and epileptic fits (especially those of Jacksonian type) in some of the cases. The history in Cushing's cases supports this view, as does the history in Brushfield and Wyatt's first case, where the left hemiplegia was first noticed after the child had had a severe left-side convulsive attack at the age of six weeks. But the history given in Kalischer's and some other cases hardly does so. Intracranial hæmorrhage from angiomatous blood-vessels was the final cause of death in certain cases recorded by Wirgman, A. Esser, O. Latham, Worster-Drought and Dickson, and L. Nielsen. It is at least probable that the spastic hemiplegia arising during intrauterine or early life in some of the cases at present under consideration was due to intracranial hæmorrhage from the abnormal blood-vessels of a diffuse leptomeningeal angioma of the opposite cerebral hemisphere, that is to say, on the same side as the main vascular nævus of the face.

I have not attempted to enter into the whole subject of intracranial hæmangiomata associated with hæmangiomata in the skin or other parts of the body (Lindau, Schuback) or associated with multiple abnormalities, such as in Schubert's recent case in which a cerebral angiomatous tumour and one in the medulla oblongata were associated with a cystic pancreas and a small hypernephroma. Schuback refers to the association of capillary angiomata, but not cavernous angiomata, with syringomyelia. A remarkable example of partially angiomatous tumours of the skin and cerebral meninges, associated with other abnormalities, has recently been described by Drs. G. Brown and J. N. Jacobson. Their case was that of an imbecile girl, who died in a condition of status epilepticus at the age of $4\frac{1}{2}$ years. She had nævus-like patches distributed on the face, neck and shoulders of the left side; microscopically their structure appeared to be intermediate between cavernous angioma and simple pigmented mole. She also had a common hairy mole on the nape of the neck, a lymphangioma-like stripe on the chin, and a little dermoid-like tumour on the cornea of the left eye. In addition to these external abnormalities there were two small symmetrically situated intracranial tumours, apparently of angiolipoma-like structure, one of them attached by a pedicle to the pia-arachnoid in the cerebello-pontine angle on each side of the brain. The authors give references to similar pedunculated tumours of the cerebral meninges described by German pathologists.

These cases of Schuback, Schubert, and Brown and Jacobson, are examples of, or at least allied to, the so-called "Lindau's syndrome"—a term introduced since the publication of A. Lindau's "Studies on Cerebellar Cysts" in 1926. In Lindau's syndrome hæmangiomatous or hæmangiomatous-cystic disease of the cerebellum is associated with capillary angioma of the retina, and sometimes with hæmangiomatous and cystic lesions in other parts of the body, such as hæmangiomata in other parts of the nervous system, renal and pancreatic cysts and suprarenal adenoma. H. Cushing and P. Bailey ("Hæmangiomas of Cerebellum and Retina," *Archives of Ophthalmology*, New York, 1928, lvii, p. 447) confirm the association of retinal hæmangiomatosis with hæmangioma of the cerebellum, and illustrate the occasional

hereditary or familial nature of "Lindau's syndrome." They quote a new paper (1927), by G. F. Rochat, on familial retinal and cerebellar angiomatosis and refer to an equally recent (1927) article on "Lindau's syndrome," by Wohlwill (both published in Germany).[5] F. H. Leavitt (*Archives of Neurology and Psychiatry*, Chicago, 1928, xix, p. 617) has recorded the occurrence of a cerebellar tumour (I have unfortunately not seen the original description) in two almost certainly homologous (uni-ovular) male twins. It is now clear that patients in whom retinal hæmangiomatosis ("Hippel's disease"), which is sometimes a familial condition, has been noted (several instances have been recorded in scattered publications in England), should be re-examined in regard to the possible co-existence of intracranial lesions. The relation of retinal hæmangiomatosis to Lindau's syndrome seems to me analogous to that of unilateral buphthalmus or congenital glaucoma to the syndrome which forms the main subject of my present communication, namely, cutaneous and cerebral meningeal hæmangiomatosis.[6]

At the Société de Neurologie (Paris) on November 29, 1928, Lhermitte and Cornil showed a man, aged 39, with syringomyelia and syringobulbia and with multiple cutaneous pigmentary and vascular nævi.

With the intracranial hæmangiomata associated with hæmangiomata elsewhere may be compared the rare cases of angiectatic hæmangiomata of the spinal meninges associated with hæmangiomata of the skin or elsewhere. In Stanley Cobb's case the hæmangioma of the spinal cord was associated with cutaneous nævi of the same metamere. In Berenbruch's case a spinal cord angioma was associated with scattered angiolipomata and with an angioma of the kidney capsule. Chaput clinically described the case of a man with a large superficial angioma of the right flank and with atrophy of the right leg, the latter being conceivably due to pressure of a spinal meningeal angioma. In E. Herman's case of cavernoma cerebri there was spinal meningeal hæmorrhage possibly related to an abnormal vascularity in the spinal meninges of angiomatous nature.

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5 Other papers on the subject are quoted in Cushing and Bailey's magnificent monograph, "Tumours Arising from the Blood-vessels of the Brain" (1928), including H. Kufs, "Ueber heredofamiliäre Angiomatose des Gehirns und der Retina, &c.," *Zeitschr. f. d. ges. Neurol. u. Psychiatrie*, 1928, cxiii, 651-686.

6 A difference in colour between the two irides was present in my case, in one of Brushfield and Wyatt's cases, and in T. W. Sheldon's case (see ADDENDUM).

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ADDENDUM.—Dr. G. L. S. Kohnstam kindly tells me of a man, aged 45, a grocer's assistant, who has hæmangiectatic nævi of the lower lip. Three years ago he commenced to suffer from occasional epileptic convulsions of the Jacksonian type, beginning in the left hand. The Wassermann reaction was negative. On exploratory operation, Mr. H. S. Souttar found a condition resembling racemose arterial angioma of the leptomeninges over the middle of the motor cortex on the right side of the brain. A beneficial result was obtained by the local extradural insertion of radium.

Dr. C. Worster-Drought has kindly sent me the following notes of two cases in which extensive right-sided hæmangiomatous nævus of the skin was associated with left-sided (contra-lateral) Jacksonian epilepsy.

Case I.—Extensive Right-sided (mainly) Cutaneous Hæmangioma with Left-sided Jacksonian Epilepsy probably due to Right Cerebral Hæmangioma.

G. G., a girl, aged 6, seen March, 1928.

Present Complaint.—"Fits."

History.—The first fit occurred when she was 14 days old, and she has since had attacks on an average of one in two months, except during the last twelve months when a fit has occurred about every fourteen days. The cutaneous hæmangiomata have been present since birth.

Previous History.—Labour prolonged, otherwise normal. Measles at two years.

Family History.—Seventh child of a family of seven, all alive and well excepting one, who died age 7 weeks from summer diarrhœa. No similar condition or "fits" known in family. Mother died six months ago (aged 35) from "heart-failure."

Medical Examination.—Normal size and development. Extensive cutaneous hæmangioma involving right side of face and neck (only a slight patch on left cheek), practically the whole of right side of trunk and right arm (on the left side there is only a small patch below the clavicle and faint naevi on the left arm), right leg almost entirely involved and the left leg to a slight extent only). There is a massive vein running from the manubrium sterni downwards slightly to the left of the mid-line and bifurcating below the umbilicus, each branch disappearing on the anterior aspect of each thigh.

Central Nervous System.—Pupils dilated, left larger than right, reactions to light and accommodation normal. (The right conjunctiva shows slight injection between 3 and 6 o'clock.) Slight nystagmus on deviation of eyes to right. Other cranial nerves normal. Sensation, as far as can be tested, normal. Upper limbs normal. Abdominal reflexes and coördination normal. Lower limbs normal in motor power and tone. Left knee-jerk brisker than right. Ankle-jerks equal and normal. No ankle clonus, plantars flexor.

Other Systems.—Heart, lungs and abdominal organs nil abnormal. Pulse 90. Blood-pressure 95/75.

X-ray of Skull.—Nothing abnormal.

Cerebro-spinal Fluid.—Definitely blood-stained, the blood being intimately mixed with the fluid which was ejected during lumbar-puncture with pulsations synchronous with the heart-beat. 180,000 red blood-cells per c.mm., 120 white cells. Wassermann reaction negative.

Description of Fit.—Two days before a fit she becomes restless and difficult to manage. The right eye becomes injected (pink). At onset of attack head and eyes turn to right, falls to ground, becomes rigid, left-sided clonic movements follow, the arm being more affected than the leg. She is incontinent and has frequently bitten her tongue. As a rule similar fits follow one after another with intervals of sleep, the whole series being spread over six to nine hours.

Case II.—Right-sided (exclusively) Cutaneous Hæmangioma, with Left-sided Jacksonian Epilepsy, probably due to Right Cerebral Hæmangioma.

C. H. W., male, aged 22; seen March, 1922.

Present Complaint.—"Fits" and occasional occipital headache.

History.—The cutaneous hæmangioma (see description) has been present since birth. "Fits" began only four years ago, at the age of 18, and has since averaged one per month.

Previous Illnesses.—Fistula in ano, 1916; operation.

Family History.—Negative. No similar condition known in family.

Physical Examination.—Intelligent and of normal mentality. Extensive cutaneous hæmangiomata, *exclusively right-sided*, as follows: (1) Involving whole of forehead and neck

FIG. 5.—Dr. T. W. Sheldon's Case (description on p. 34).

down to level of clavicle; (2) whole of trunk in front from fifth rib to groin and back from spine of scapula downwards to iliac crest, with extension on to axillary part of right arm; (3) right leg from groin downwards, mainly on anterior, posterior and inner aspects (outer side escaping).

Central Nervous System.—Cranial nerves normal. Upper limbs: Motor weakness of right arm compared with left, but no inequality of deep reflexes. Sensation normal. Left knee- and ankle-jerks definitely brisker than right. No ankle clonus. Plantars flexor. Abdominal reflexes: right greater than left. Coördination normal.

Other Systems.—Nothing abnormal.

Skiagram of Skull.—Nothing abnormal.

Description of Fit.—Feeling of malaise for one hour or so before onset of attack, with *aura of multi-coloured lights in left eye*, then sudden onset with cry and fall and left-sided

clonic movements affecting mainly the *left* side of the face and left arm. The fits usually occur singly and last from three to twelve minutes.

Dr. T. W. Sheldon has kindly sent me the following note of a congenital low-grade imbecile in Tooting Bec Hospital:—

A man, H. P., aged 67, born with left-sided facial hæmangiomatous nævus and complete right-sided hemiparesis.[7] Roentgen-ray examination shows what seems to be a left-sided racemose meningeal hæmangioma, extending from the frontal region backwards to just above the level of the tentorium cerebelli. There is likewise a peculiar density of bone (not an artifact) in the posterior parietal region, and Dr. Sheldon adds that repeated attempts to nullify an unnatural thickening in the petrous area have failed (fig. 5). The case is an excellent example of unilateral vascular nævus (involving the face and the ocular conjunctiva), with contralateral hemiparesis and X-ray evidence of a meningeal racemose hæmangioma on the same side as the facial nævus. The age of the patient is also interesting. Wassermann reaction negative.

From H. Cushing and P. Bailey's recent monograph on "Tumours Arising from the Blood-vessels of the Brain" (1928, p. 26), I must quote the following:—

"Three remarkable examples of congenital facial nævi associated with epilepsy and in each of which a calcified lesion was radiographically disclosed in the occipital lobe have been reported from the Argentine, the first of them by V. Dimitri (*Rev. Assoc. méd. Argent.*, 1923, xxxvi, p. 63), and the other two by A. M. Marque (*Rev. Oto-Neuro-Oftalm.*, Buenos-Aires, 1927, i, p. 202). The patients were all children, of 13, 6, and 10 years respectively at the time the calcification was first observed."

FIG. 6.

To Dr. W. J. N. Vincent of the South Yorkshire Mental Hospital (through Dr. G. Brown) I am indebted for the account of an imbecile and epileptic girl, aged 16 years, who died of tuberculous meningitis in 1926. She had a facial hæmangiomatous nævus confined to the distribution of the second division of the trigeminal nerve on the right side (see fig. 6), and was the subject of severe epileptic fits, which were general and not of the focal or Jacksonian type. There was no hemiplegia. On examining the brain at the necropsy, besides evidence of meningeal tuberculosis, there was an extensive localized hæmangiectatic condition of the vessels of the pia-arachnoid over the convexities of the cerebral hemispheres, especially over the post-Rolandic and parietal regions of the left hemisphere. The left hemisphere was smaller than the right hemisphere. It is interesting that in this case the meningeal hæmangiomatous condition was mainly contralateral to the facial nævus.

Discussion.—Dr. C. P. SYMONDS: Though familiar with the association of nævi in the trigeminal field with contralateral epilepsy or hemiparesis, of which I have seen several examples since the case (already referred to by Dr. Parkes Weber) which I showed at the Association of Physicians Meeting in 1925, it was not until September, 1928, that I first made acquaintance with an X-ray picture similar to that just shown. The patient, a boy aged

7 The case was shown by Dr. T. Brushfield (*Proc. Roy. Soc. Med.* (Sect. Dis. in Child.), 1929, xxii, 12), and these notes, with the figure, are given here by kind permission of Dr. E. Beresford.

11, was brought to me by Dr. W. H. Beach, of Hassocks, on account of a visual defect for which he had already been seen by Mr. Foster Moore. The characteristic X-ray appearances were discovered by Dr. Prowse, of Brighton.

The patient was born with a nævus affecting almost the whole of the left side of the face. This was contained within the field of the left trigeminal nerve, but by no means filled it. The upper and lower eyelids were both affected and the greater part of the cheek, including parts supplied by the third division of the nerve. Apart from this disfigurement he appeared to be in every way normal. He is at school and up to the standard of his fellows in work and play. His father is an invalid and is said to be suffering from tabes with optic atrophy. The patient's Wassermann reaction in the blood is, however, negative, and there are no clinical indications of syphilitic disease.

He was taken to see Mr. Foster Moore in September, 1928, on account of imperfect vision. The examination showed an incomplete right homonymous hemianopia (see charts, fig. 7). In addition to this there was a slight dislocation of the lens in the right eye and a congenital coloboma of the left disc. He suggested to Dr. Beach that the likeliest cause was a nævus of the visual cortex and advised that a skiogram of the skull should be taken. This was carried out by Dr. Prowse. I saw him a few days later and found nothing to add to the list of his physical defects except that the abdominal reflexes on the right side were diminished as compared with those on the left. Mentally he appeared normal. There was no defect of speech either on the receptive or the expressive side.

Stereoscopic X-rays make it clear that the shadows revealed in the lateral and antero-posterior views are those of a plexus lying throughout in close relation to the skull.

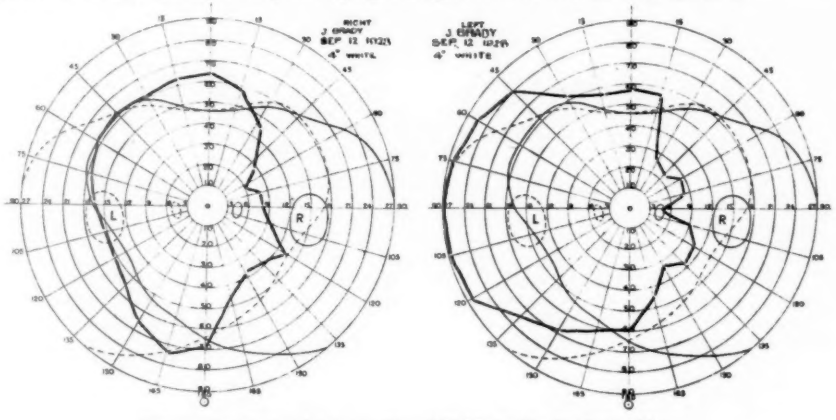

FIG. 7.—Dr. Symonds' Case: Visual Fields by Mr. R. Foster Moore.

A search through the literature of X-rays and of neurology provided no analogous picture, the nearest approach being the report of Dr. Parkes Weber's case already published in the *Journal of Neurology and Psychopathology*, 1922, but a few suggestive clues were obtained from papers dealing with calcifications of angiomata generally. Ruggles [1] reported five such cases in 1919. The clinical data are not recorded, but the diagnosis in each case was of an angiomatous tumour; one of these was of the jaw, the others of trunk or limbs. The X-ray pictures in each instance showed "multiple small cyst-like masses varying in diameter from a millimetre to a centimetre or more, with a thin shell, and an irregular mass in the centre, giving an appearance suggestive of encysted parasites. These spots are scattered throughout the growth and probably represent calcification of thrombi in the loops of the cavernous type of hæmangioma." Similar appearances in hæmangiomas of trunk or limbs have been recorded by Wakely [2], Ravold [3], Dunn [4], and Johnson [5]. I am indebted to Mr. Foster Moore for permission to quote his observations and reproduce the visual fields, and to Dr. Prowse for the X-ray pictures (figs. 8 and 9).

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[1] RUGGLES, *Amer. Journ. Roent.*, 1919, vi, 512. [2] WAKELY, *Arch. Rad. and Elect.-therap.*, 1921, xxv, 363. [3] RAVOLD, *Rad.*, 1924, iii, 231. [4] DUNN, *Ann. Surg.*, 1925, lxxxii, 880. [5] JOHNSON, *Journ. Amer. Med. Assoc.*, 1928, xc, 1108.

Dr. R. M. STEWART said that some time ago he had had the opportunity of seeing Dr. Brushfield's cases, to several of which Dr. Parkes Weber had referred. About a year ago a case of this type had been under his care. The patient was an epileptic imbecile with an infantile hemiplegia on the right side and an extensive nævus of the body, particularly well marked in the left trigeminal area of the face, the line of demarcation between the normal skin and the nævus being sharply defined.

It was anticipated that post-mortem examination would disclose a cerebral or meningeal hæmangioma, but no evidence of such a condition was found. The left cerebral hemisphere was considerably smaller than the right, and there was evidence of an old softening in the region of the left superior parietal gyrus, but little else was found. He (Dr. Stewart) concluded from this case that the association of infantile hemiplegia with extensive nævus of the skin might occur without an accompanying intracranial hæmangioma.

Mr. L. BATHE RAWLING said that he had experience of two cases. The first was that of a boy of 6 years old, who since birth had suffered from attacks of stupor and convulsions. There was some bulging of the right temporal region, and this was explored. Some bone was cut away, exposing normal dura. When opened, a large angiomatous plexus was seen, extending beyond the area of brain exposed, consisting mainly of large tortuous veins. The wound was closed. This was in 1920.

In 1926 the boy was admitted to hospital with a large hæmatoma of the right eye region, right face and extending on to the neck. It was obvious that some vessel had burst, and that the blood had escaped freely outwards. He recovered and is now attending school, better in all respects.

The second case was that of a boy, aged 10, one of twin sons, and regarded at first as the healthier twin, though he had a nævus of right face, involving exactly the area supplied by the first and second divisions of the fifth nerve. When 6 months old he had had a series of convulsions, with left-sided Jacksonian fits. The left arm had remained weak, and he had never used it till he was nearly 4 years old. Every year up to 1926 he had had one or more of these convulsive attacks, all of which had presumably been due to the bursting of a vessel and hæmorrhage over the surface of the brain. Since 1926 the condition had improved and he had not had any severe seizure. The mentality, however, was poor—he was very irritable, only obeyed his nurse, and liked to throw things about. He was weak on the left side of the body and spastic. In reference to treatment, it seemed obvious that operative measures were useless and highly dangerous. It was proposed to send him to the Radium Institute, where Dr. Pinch was undertaking the case, and it will be highly interesting to see if any benefit accrues. The skiagram showed a mass of tortuous vessels over the right side of the brain, most marked in the occipital region and entirely above the tentorium cerebelli, with smaller scattered vessels, all somewhat calcareous, over the mid-brain and the frontal region (see fig. 10).

There were five specimens in the museum of St. Bartholomew's Hospital illustrating various degrees of hæmangioma of the brain, and apparently these angiomata extended, as a rule, deeply into the brain substance, and were seldom truly superficial in position.

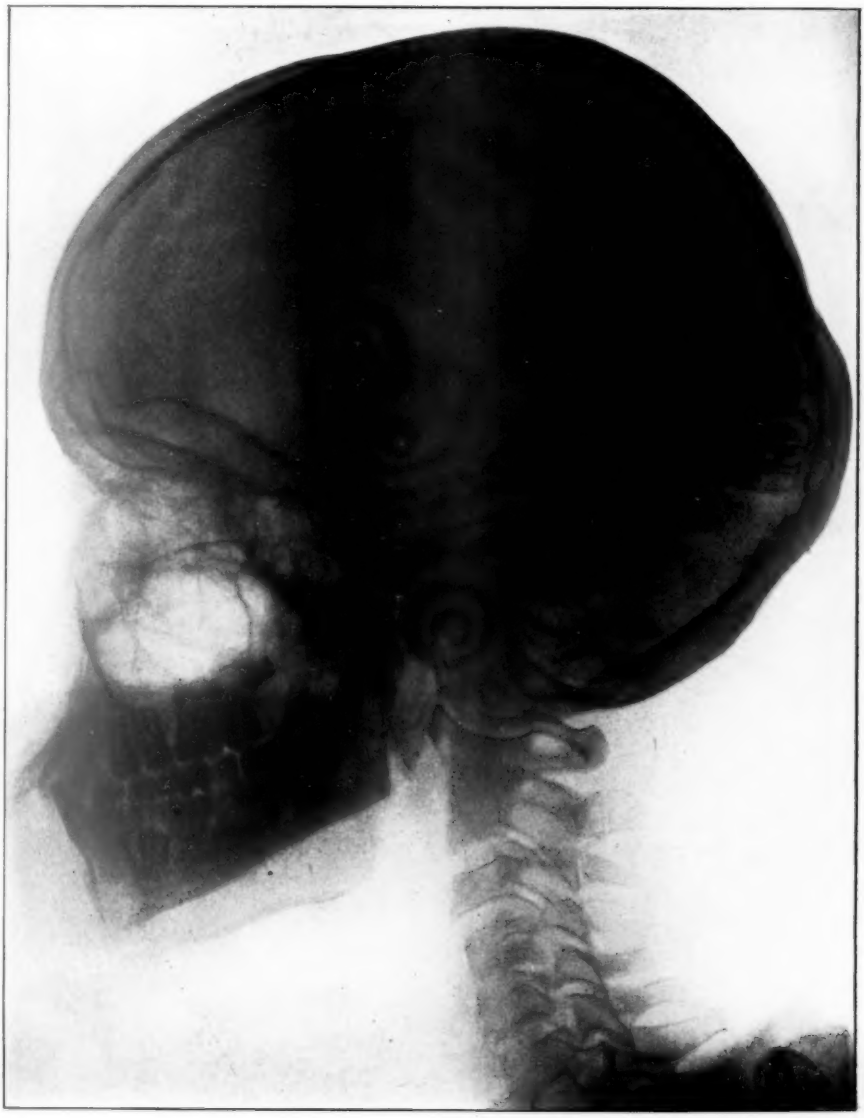

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FIG. 8.—Dr. Symonds' Case: Lateral Skiagram.

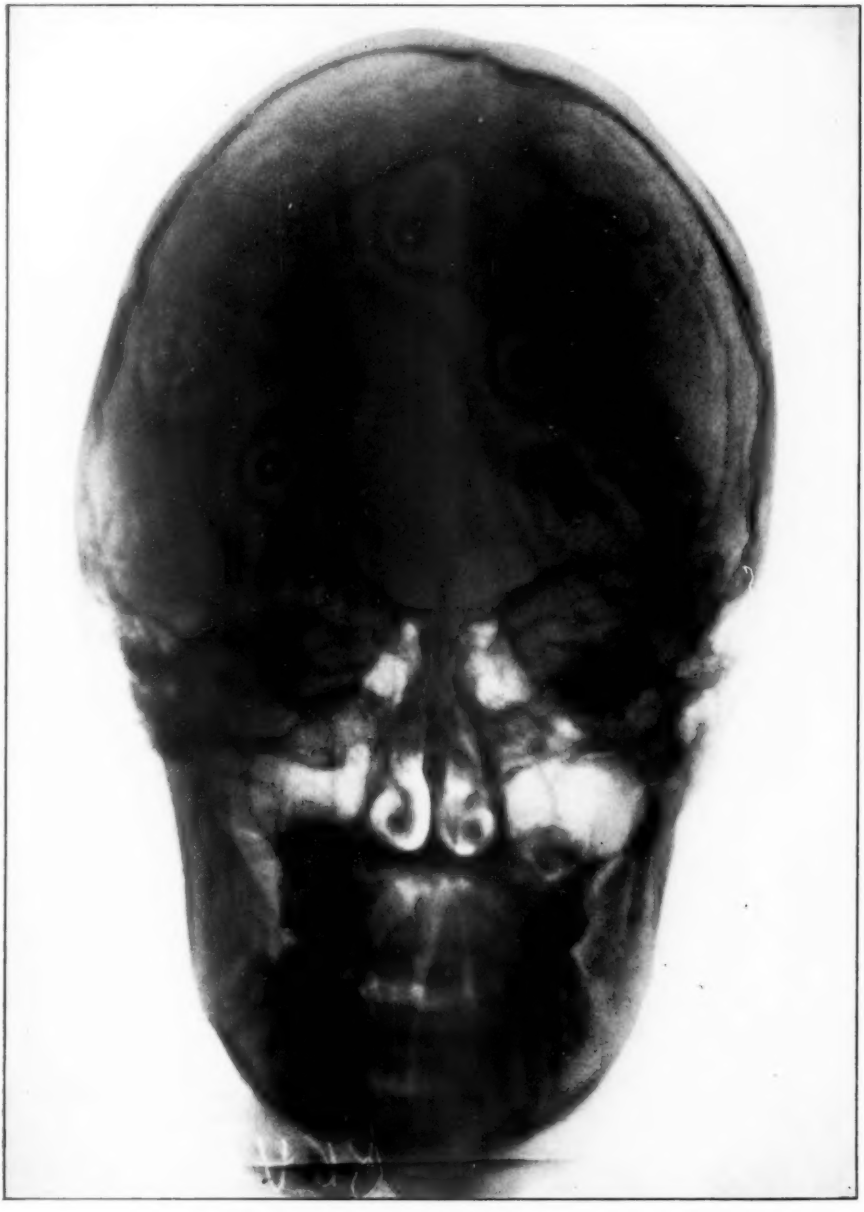

FIG. 9.—Dr. Symonds' Case: Antero-posterior Skiagram.

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FIG. 10.—Mr. L. B. Rawling's Case: Lateral Skiagram (description in text).

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Section of Ophthalmology.

[*December* 14, 1928.]

[CLINICAL MEETING HELD AT THE ROYAL WESTMINSTER OPHTHALMIC HOSPITAL.]

Retinal Hæmorrhages.—W. H. MCMULLEN, F.R.C.S.—Patient, a girl, aged 16. Blood-pressure normal. She has been examined by a general physician and by several specialists, who have passed her as perfectly normal. There is a little hæmorrhage on the disc, and another hæmorrhage below. The onset of blurred sight occurred when a menstrual period was about ten days overdue. This is the only indication of a possible cause for the hæmorrhages.

Mr. MCMULLEN also showed the following cases: (1) Embolism of Central Retinal Artery; (2) Hole at Right Macula; (3) Old Irido-Dialysis: Localized Lenticular Opacities; (4) Scar Tissue and Pigment at Right Macula; (5) Ophthalmoplegia Externa.

Flap Sclerotomy: Result of Operation Four Years Previously.—Sir RICHARD CRUISE, F.R.C.S.—This patient, a man, was operated on four years ago, and the case is merely brought forward as an instance of the satisfactory result that may be obtained in a case of glaucoma by this operation of flap sclerotomy, provided that healing of the incisions is deliberately delayed by early massage and expression of aqueous under the conjunctival flap for about a week, a method which I advocated eleven years ago (*Transactions of the Ophthalmological Society*, 1918).

There is a perfect filtration scar. Aqueous is escaping from the anterior chamber beneath the conjunctiva, but the process lacks pathological explanation, as an eye showing the condition has not as yet been obtained for microscopical investigation. The only hypothetical solution that I can give is that it is due to admixture of endothelial cells with ordinary scar tissue. It is a scar tissue which allows transudation of aqueous through the cicatrix underneath the conjunctiva. There need never be any anxiety once filtration has been established.

Sir RICHARD CRUISE also showed a case of Persistent Hyaline Artery with Degenerative Choroido-Retinitis between Disc and Macula.

Juvenile Tabetic Paralysis.—GEOFFREY VINER, F.R.C.S.—Patient, male, aged 24, is apparently suffering from taboparesis of a juvenile type—that is to say he has some of the characteristics of tabes and some of the characteristics of general paralysis of the insane. During the last six months he has noticed a rapid failure of sight due to optic atrophy, with marked contraction of the fields. His vision in the right eye is $\frac{6}{36}$, and in the left eye $\frac{6}{12}$ only. The interesting point is that he has been treated for some three months by means of malarial infection. The neurologist has informed me that treatment by malaria is distinctly good for cases of general paralysis of the insane, but of very little use for tabetics. There has been no improvement of the vision as the result of the treatment.

Diffuse Corneal Opacities.—GEOFFREY VINER, F.R.C.S.—Patient, female, aged 26. No history of any eye trouble. Both corneæ milky white owing to a uniform diffuse opacity from the centre to the extreme corneal margin. Observation with the slit lamp confirms the opinion that there is complete absence of any inflammatory change, e.g., blood-vessels in the cornea. The opacities appear to occupy the entire depth of the substantia propria leaving both epithelium and endothelium unaffected.

The acuity of vision is surprisingly good, being $\frac{6}{9}$ in each eye. The condition is probably congenital.

Pituitary Tumour, Successfully Treated by Operation.—MONTAGUE HINE, F.R.C.S.—This case shows the satisfactory result of an operation for pituitary tumour. The scar is not very noticeable. When she came to me twelve months ago the patient gave a short history of headaches and defective vision. On examination there was very little to account for the defective vision. There was nothing abnormal in the fundi. An examination of the fields showed what the diagnosis must be. She went into Charing Cross Hospital, and from there was sent to Mr. Broster, who performed a very satisfactory operation, which increased the field of the left eye and the vision went from $\frac{6}{36}$ to $\frac{6}{9}$. In the right eye the vision did not improve; if anything it was down a little, but the field had increased.

Mr. HINE also showed the following cases: (1) Concussion Injury to Left Macula Ten Years Previously; Secondary Optic Atrophy (right); Recent Optic Neuritis. (2) Glaucoma, Mole of Choroid, Hæmorrhages, Arterio-sclerosis (diabetic). Maculæ in a Boy aged 12; Juvenile Form of Macular Degeneration without Cerebral Changes (Batten's Disease).

Mr. ADRIAN CADDY, F.R.C.S., showed the following cases: (1) Dermoid Cyst; (2) High Blood-pressure and Albuminuric Retinitis; (3) Synchesis Scintillans.

Bright Blue Changes in the Deep Layers of the Cornea (Slit-lamp).—T. HARRISON BUTLER, M.D.—This man has been attending the Birmingham Eye Hospital for five years. He has been treated for chronic conjunctivitis with occasional superficial peripheral ulcers. He has always complained of irritation of the left eye in which there is now this peculiar appearance. The treatment has been along orthodox lines: zinc sulphate, yellow oxide ointment, and silver nitrate. On one occasion, two years ago, he had some drops of acriflavine. In October, 1928, a blue sheen was noticed in the lower part of the cornea, and examination with the slit-lamp showed a most extraordinary change in the deep layers. The posterior eighth of the corneal section is marked out into irregular areas by deep grooves which seem to radiate from knots. The tissue between these trabeculæ is fluffy white interspersed by a bright blue coloration. The hue is represented in a paint box by "cyanine." The abnormality involves the lower part of the cornea and is bounded by a crescentic line whose apex reaches a little higher than the centre of the cornea. The anterior part of the cornea sheds a golden sheen, and in the upper part of a corneal section the blue tends towards purple.

The case has been shown before the Midland Ophthalmological Society, but so far no cause has been suggested for the condition, which seems to be unique. The question arises: is the coloration due to actual pigmentation or is it a diffraction effect from the lace-like tissue at the back of the cornea? The fact that there is a tendency to polychromism supports the diffraction theory. The right eye shows no abnormality in the cornea. The man is a copper polisher but there is no history that a foreign body has entered the cornea, nor does copper ever produce this type of cornea. The disease has so far progressed rather rapidly and the visual acuity is reduced to $\frac{6}{18}$.

Extensive Crystalline Deposits in the Cornea.—C. B. GOULDEN, F.R.C.S.—Patient, male, aged 60. The right eye was inflamed in childhood and has been defective ever since. The left eye was inflamed fifteen years ago, and there was a recurrence in 1924 when the patient came under my care at the London Hospital. At this time the right cornea was completely opaque and in the left cornea there was a large recent patch of infiltration in the upper part. The Wassermann reaction was negative.

Locally, heat and atropine were applied, but the condition did not improve. Ultra-violet light baths were then given and have been given intermittently up to the present.

The left eye, although severely damaged, has still useful vision.

The substance of both corneæ is apparently infiltrated with silky-looking crystals, some of which are iridescent.

Copper Cataract.—C. B. GOULDEN, F.R.C.S.—Patient, male, aged 50, in 1916 received a perforating injury of the left eye from an exploding detonator. An intra-ocular foreign body, which was non-magnetic, was revealed by a radiogram.

There is left ciliary infection, and bullous keratitis due to the raised intra-ocular pressure.

There is a greyish-green coloration of Descemet's membrane similar to the discoloration in cases of Wilson's disease (hepato-lenticular degeneration). Mr. Butler has published a case in the *British Journal of Ophthalmology* with a coloured drawing of the condition of the cornea. The anterior chamber is of irregular depth and the iris tremulous, due to partial dislocation of the lens. There is a peculiar shallow opacity in the anterior and posterior part of the lens just beneath the capsule, of such slight density that a view of the fundus is obtainable. The portion of the lens in the pupillary area is clear.

The opacity, when seen by ordinary focal light *not* in the path of the directly reflected rays, is of a bluish appearance and consists of radii from the central clear area, up to the periphery. The appearance has been compared to that of a sun-flower. When the lens is examined in the zone of specular reflexion there is a brilliant iridescence from the anterior and posterior part, like polished copper.

Examples of this condition have been examined histologically and although a layer of copper of some sort has been found under the anterior capsule, none has been recovered from the posterior part of the lens.

Mr. A. D. GRIFFITH, F.R.C.S., showed the following cases: (1) Rodent Ulcer Successfully Treated by Radium; (2) Mooren's Ulcer.

Mr. FRANK HECKFORD, F.R.C.S., showed a Case of Corneal Ulceration following Mustard Gas Burns in the Eye Ten Years Previously. The case was shown on account of its exact similarity to that shown by Mr. Humphrey Neame in October, 1928.[1]

Case of Unusual Pigmentary Change at the Macula.—M. WHITING, F.R.C.S.—Patient, female, aged 64. She was first seen at Moorfields in my absence when the condition was described by an assistant as melanoma of the macula, and for this reason the case is brought forward. The condition resembles a melanoma of the ordinary type, except that the pigmentation is rather too dark. Melanomata are usually of a grayish colour, almost slaty, and rather lighter than in this instance. Five years ago this patient was struck in the eye by a piece of wood. She noticed at the time what she says looked like a half moon, a black area in front, and she had, I think, some hæmorrhage, which was followed by the deposition of pigment in that area. She has defective vision, due to a high degree of myopic astigmatism, not corrected until recently.

White Crescent round Disc; ? Coats' Disease.—LESLIE PATON, F.R.C.S., and F. A. WILLIAMSON-NOBLE, F.R.C.S.—Patient is a boy, aged 14. The whitish area has lately increased considerably. It is not, however, in appearance, typical of Coats' disease, because the surface swelling shows some fine striation. The boy's general condition is perfectly healthy, though a skiagram of the lungs shows a slightly thickened pleura. The blood coagulation time is normal. Probably this is an early stage of Coats' disease, but it is also possible that it is a growth in the disc, and it was shown for diagnosis (see figs. 1 and 2, p. 24).

[1] See *Proceedings*, 1928, xxii (Sect. Ophthal.).

Mr. WILLIAMSON-NOBLE also showed a Case of an Affection of the Choroid in a man, aged 56, with high blood-pressure, who had noticed that after coming down rather violently on one foot while walking, his vision had been obscured. There were hæmorrhages along the inferior temporal vein, and a certain œdema in the retina had increased considerably. There was a dark area in the choroid, hæmorrhagic or neoplastic in origin.

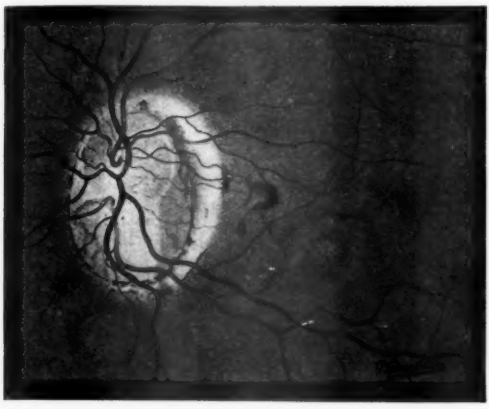

FIG 1. 21.4.28
White crescent round disc.

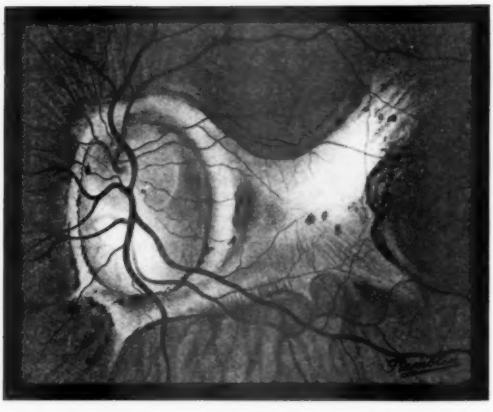

FIG. 2. 28.11.28
White crescent round disc; later stage.

Section of Electro-Therapeutics.

[*October* 19, 1928.]

PRESIDENT'S ADDRESS.

Clinical Remarks on Radiation Treatment.

By J. E. A. Lynham, M.D., M.R.C.P., D.M.R.E.

In choosing a subject for my address I thought it might be of interest to review briefly the therapeutic side of our work; to spend a while, as it were, stocktaking; to consider some of our successes; and still more, to ponder on our failures.

We who treat with radiations—with ultra-violet light, X-rays, and radium, are all conscious that in a large number of cases we can contribute to the essential object of a physician's life—to improve the bodily health of those who suffer, to relieve their pain and palliate their sufferings, and to cure.

But at intervals, from different directions, we hear such remarks as that ultra-violet light is greatly over-rated, that radium has proved a disappointment, that X-rays are of very little real use.

As a rule such remarks come from some disappointed man who asked too much from the treatment, or who with but limited experience reasons illogically from a particular case to the general. The patient who becomes well goes away and is forgotten; but those who do not improve come back to haunt us.

We still cannot say why it is that one patient becomes well, and another, in a case apparently very similar, fails to respond to the treatment.

The case for the use of ultra-violet light, in skilled hands, is proved. The benefits that are being daily wrought in rickets, in lupus, in surgical tuberculosis, in obscure debility, and a host of other diseases, are known to all. The exact biological mechanism by which the cures are accomplished may not yet be entirely understood, but some of the principal factors have been ascertained; the technique has been mastered, the effects are sure, and the results so satisfactory that the treatment is now thoroughly established and recognized. In spite of overcrowding and bad housing, we can hope that in the near future such a condition as rickets in a child will become a rare disease.

If anyone has any doubts as to what ultra-violet light—what sunlight—can do, let him visit Sir Henry Gauvain's wonderful establishment at Alton. I wish it could be arranged that every student of medicine in the country should include a visit to that institution as a part of his regular training.

Concerning X-rays, the great complexity of the subject stands out clearly, in view of the many claims made for the effects of X-rays on the human body.

To mention a few: (1) Irradiation is given to the *thorax* for asthma, spasmodic coryza, angina pectoris, and pertussis; (2) to the *spleen* in the anæmias, in asthma, in metrorrhagia, in epistaxis: (3) To the *hypophysis* in Graves' disease and in dysmenorrhœa; (4) to the *stomach*, in hyperchlorhydria, in gastric and duodenal ulcer. To the *spine* in syringomyelia and acute anterior poliomyelitis. To the *heart* in rheumatic carditis. In all these conditions it is claimed by clinicians that relief has been obtained in a series of cases.

Theories as to these actions abound: *Direct effects* on blood and lymph cells, on connective tissue, on the parenchyma of the ductless glands, on the sympathetic nerves and ganglia, or on the vagus nerve endings; *indirect effects* acting through the internal secretions, the production of such bodies as thrombokinase from the

destruction of white cells, or the reticular endothelium, stimulation, or inhibition of the blood-producing tissues.

All these, and many more, are amongst the theories advanced in an endeavour to explain the mysteries. They suffice to show that the action of radiations on the body is very complex, that profoundly different effects can be produced by different methods of application, and that the radiologist should consider that he possesses not one form of treatment, but a group of agents which can be applied in a variety of ways to produce clinical results.

An outstanding instance is reported in the book written by Professor Sydney Russ and Dr. Hector Colwell, of a case of myeloid leukæmia when the typical blood changes produced by irradiation of the spleen followed the application of radium to the splenic region after the spleen had been removed.

A further curious feature is the relative inertia to radiation of normally functioning tissues as compared with the responses found in pathological states. To give an instance, extensive keloids overlying the thyroid have been treated by radium or X-rays, until they disappeared without any apparent symptoms which could be attributed to an upset of the thyroid function, whereas similar dosage applied over the thyroid in Graves' disease would be followed by very definite changes.

It may be merely that the pathological cell is more radio-sensitive than the normal: but it is more probable that there is something more to be elucidated before such problems are understood. There is much yet to be discovered; we are still pioneers.

We owe a great debt to those physicists, physiologists, and biologists who have done so much to guide us in the right way. They must be grouped together, because the information they have given us concerning the character and production of the rays, the effects of filters, the absorption and scattering in the body, the chemical changes, the radio-sensitivity of cells normal and pathological, is the essential basis for our clinical work. Russ, Kaye, Degrais, Beclère, Lacassagne, Seitz, Friedrich, Dessaur, Ewing and Duane—such men are as pillars of the building wherein clinicians are but the common bricks. Their labours have been directed towards providing us with accuracy of measurement, exactness in dosage, facility in therapeutic routine, and clearness of thought as to our objectives and our technique. But in spite of all the advances that have been made, although we are confident that we can accomplish much more than formerly and are hopeful as to the future, there are still the great problems to be solved; there are still patients in the clinics and wards of our hospitals for whom the best we can do is unavailing.

Nevertheless to-day we have very great advantages as compared with workers of ten years ago.

Physicists the world over have devoted years to the study of the absorption of rays by the tissues, cells and fluids of the body, the percentage of the beam which is effective at a given depth, the character of the radiation at different voltages, the significance of intensity, of mean wave length, of shortest wave length. Elaborate tables have been constructed to help us and rules have been laid down for our guidance. Pathologists have endeavoured to show us the way to give a lethal dose to any given type of neoplastic cell, and have vied with each other in ingenious endeavour to give experimental evidence in support of their conclusions. The literature of the subject is enormous.

The methods of the physicist are precise. He tells us how to calibrate our apparatus in order to deliver at a given site a prearranged dose estimated in recognized units. His work is essentially exact. He is a little impatient with anything that cannot be reduced to a formula.

The outlook of a chemist is of the same order. He deals with definite substances which can be altered in a certain way by specified reagents. But the biochemist and the pathologist are concerned with things of a different order. They plan an

experiment in correlation with a plausible and satisfactory hypothesis; but when the experiment is performed, something very unexpected may be the result, and they have to begin again.

The clinician is still further hampered, for it is entirely impossible to calculate the multiple influences which govern the existence of any individual, and however specific we may endeavour to make our attacks on what we take to be similar types of diseases, there is no uniformity in the results obtained by any one of us.

Of great interest is the effort to establish a biological measure of dosage, a method by which a definite number of Röntgen units would produce the same biological results. Packard has given X-ray doses to groups of the eggs of the fruit fly, estimating his results by counting the number of eggs that survive. His papers present some interesting conclusions: He states that the percentage of eggs killed by increasing doses depends on the variability of the eggs—and since this is of a skew type, it is *impossible* to find a simple formula which will express the relation between dosage and effect. The death-rate of the eggs is independent of the quality of the beam, homogeneous, or heterogeneous, provided a given number of units has been given. The total lethal dose is difficult to find, for the presence of a few highly resistant eggs will give misleading results.

He goes on to experiments with tumour cells and he concludes that any kind of cells that show a high degree of variation in susceptibility can be killed in greater numbers by intense radiation, acting for a short time, than by a beam of lower intensity acting for a longer time. It must be remembered that he was dealing with groups of isolated cells and not with tissues. Nevertheless, there is here a possible argument in favour of intense doses.

In a study of the effects of X-rays of different wave lengths upon animal tissues, Russ found that in the rat a bigger differential effect between hard and soft rays existed for the skin than for the tumour cells, and from this he deducted a possible argument in favour of the use of very hard γ-rays, or X-rays, when tumours at a considerable depth have to be irradiated.

Other experiments appear to have a different significance. Contamin found that mice, inoculated with tumour cells which had been exposed to X-rays for a short time were rendered immune to further inoculations of the same tumour; but a long dose destroyed the immunizing power. Wedd, Morson and Russ showed similar immunizing effects with β-rays: and again, prolonged irradiation of the cells lessened the immunity-conferring effect.

Sugiura and Benedict concluded that the degree of immunization caused by progressive absorption of tumour after use of small doses of β radiation is more marked than that following the use of larger doses.

Here is something of interest, but hard to explain: these experiments may, however, suggest that there is an *optimum* dosage, and that by going beyond this something may be lost in results. It must never be forgotten that the radio-sensitivity of cells *in vitro* is probably very different from that of the same cells in the animal body; except perhaps in the case of massive and almost structureless tumours.

The exact significance of the term *radio-sensitivity* has not yet been determined. Whether it is a quality of the nucleus, of the chromosomes, of the cytoplasm, or of the cell membrane, appears to be in doubt. The essential feature in a radio-sensitive cell is that it is relatively vulnerable to the effects of radiation. Whether the injury to the cell is produced by a physical process, by a physico-chemical change, or a purely biological effect is still debatable; whether the damage results in the death of the irradiated cell, or in the dying out, in a few generations, of its descendants is clinically immaterial, if it is possible to give an application of radium or X-rays which will cause the dissolution of the aggregate of cells forming the tumour and replace the growth by a healthy cicatrix.

In every list dealing with the radio-sensitivity of cells the basal cell tumours are placed among the more radio-sensitive. Every one of us has seen superficial rodent ulcer dissipated by radiation, and this has been accomplished by many different methods: a short intense application of β-rays: a longer application of γ-rays; weaker intermediate doses of either; X-rays applied in many different ways, with different filters, and at different voltages—all these have been known to bring about the desired result: a permanent cure.

In some instances, however, the disease may recur. It may be that some few cells were beyond the range of the effective radiation; that some were more resistant than others; or that, as Sokoloff suggests, some of the cells, as a result of the irradiation, fall into a state of prolonged latency, a type of anabiosis.

Whatever the cause the result is a recrudescence of the disease. It may be treated again with apparent cure. It may recur again, and so on. And then there comes a time when the cells apparently cease to be radio-sensitive, and further treatment by irradiation is given in vain.

Clinically, we know that this failure to respond to irradiation occurs at once when the growth has reached and invaded the periosteum, the perichondrium, or the sclerotic of the eye.

One can then, by prolonged doses of weak radiation, perhaps slow down the disease, but it becomes an absolute impossibility to eradicate it. The malignant cells maintain their activity; they have ceased to be radio-sensitive. This failure to respond to irradiation is not, however, necessarily directly associated with previous irradiation; it is found where rodent ulcer recurs after repeated operation, after apparent cure by zinc ionization, or the application of carbon dioxide snow.

Is it that the fighting power of the tissues has become impaired? Is it that the vascular and lymph supplies, the access of phagocytic cells and the interchange of oxygen, are interfered with?

It appears impossible to reconcile such clinical facts with a theory that success depends on the interaction of our physical agent, the rays, and the specific characters of one type of cell. It seems that the clinical cure must depend on at least two things: (1) The sensitivity of the tumour cell, and (2) the coöperation of the intercellular relationship, a sensitive thing which can be destroyed by repeated irradiation, or by other traumatism of the tissues, a thing which cannot be called upon to assist, once the invading cells of the growth have established themselves in contact with cartilage or bone.

A similar change in clinical response to irradiation is sometimes seen, in the later stages, in cases of inoperable cancer of the breast. The primary tumour may be reduced to small proportions, and secondary glands may have shrivelled as a result of treatment, the general condition of the patient may be good and we may be congratulating ourselves on a measure of success; but if small nodules appear in the skin which has been subjected to irradiation, they rapidly increase in number and tend to coalesce and break down.

They can sometimes be checked by altering the character of the rays employed, by substituting radium for X-rays, or vice versa; they sometimes disappear after a sharp dose of X-rays given at a comparatively low voltage; they will sometimes respond even to ultra-violet light; but such nodules, developing in irradiated tissues, present a much more difficult problem than similar invasion of the skin away from the region which has been treated.

We seem to depend to some extent on something in the tissues, something that can be destroyed by repeated doses of radium or X-rays. Perhaps Gustav Peter is right when he speaks of the uselessness of radiotherapy when the tissues adjacent to the tumour do not possess blood-vessels, and when a new formation of capillaries is made impossible by natural or pathological conditions, or when previous irradiation has produced an irreparable impermeability of the vessels.

I have had under observation a series of patients who had been operated upon by efficient surgeons for carcinoma of the breast. In each of these cases the surgeon was convinced that he had not completely removed the disease, so the case was sent for what is called prophylactic irradiation. To some of these—thinking in terms of the lethal dose—relatively heavy doses of high voltage X-rays were given; but such treatment was quickly abandoned, for it proved an absolute failure.

To others a less intense dosage was given with varying results. The best results have followed weak doses, given to the scar area and the adjoining tissues, following the lines of lymphatic drainage. In these cases, when thickening or induration pointed to local recurrence, a more intense dose of X-rays, or of radium, repeated two or three times, has been given to a localized area; and though there have been a few exceptions, as a rule the results are most satisfactory. I think it is justifiable to speak of such doses as "protective," for there is an accumulating amount of evidence that the average duration of life in the groups of cases so treated is longer than would be the case if no treatment were given.

Such doses produce no obvious damage to the skin, no dryness, no telangiectasis. The general effect on the body, as evidenced by blood examinations, is helpful rather than hurtful. They fall far short of producing erythema.

Concerning erythema, there has been much controversy. Its significance may vary under different conditions; but it is a convenient clinical guide when considering the relative effects of successive treatments in a given case. It has been used as a standard of safety, to restrain the X-ray dosage given through a given area, and as a basis on which to estimate dosage at a depth.

Similarly, with a flat radium applicator, the time taken to produce an erythema is reasonably constant, even for different individuals. In endeavouring to treat some cases of extensive skin disease of the psoriasis type with radium, some interesting effects were noted. An application, the duration of which was approximately one-third of that necessary to produce erythema, usually made a selected patch of the disease disappear. A slightly longer dose, equal to about half an erythema dose, produced no obvious change; the patch remained. A longer dose than this actually made the condition worse.

Having established the dose for one area, and tried it out on a number of patients, with quite uniform results, we proceeded to treat at one sitting a patient who had patches all over his body and limbs: and to my dismay no effect whatsoever could be found; but when a further attempt was made, limiting the applications to one portion of the patient, e.g., one leg or arm or a quarter of the trunk, the skin cleared up satisfactorily. It was necessary to allow a few weeks to elapse before going on to the next region.

I have wondered whether these skin effects have analogies with other conditions. It may be possible to fail by irradiating too much of the body at one time. In a condition like lymphosarcoma where there are many groups of enlarged glands, it may be wrong to try to treat all the affected regions in too short a time; too much toxæmia may follow, too much damage be done to the blood and other tissues, or perhaps some type of migrant cell which plays a part in the restoration of the diseased tissues may be distributed over too large an area. Possibly it might be better to concentrate first on one region, and, after a time, go on to another.

Another difficult question is that of overdosage. Apart from causing massive necrosis or sloughing, one can overdo the treatment of a growth. In the earlier days of radium therapy, comparatively large quantities of radium were applied to the surface of the growth in such conditions as cancer of the jaw, and sharp inflammatory reactions were often seen. The results were not very good.

As time went on, doses were lessened, and improvement was more marked.

Then the use of radon needles was begun. At first these were charged with 30 mc. and were left in the growth for long periods, with the result that hæmorrhage

and sloughing occurred occasionally. Gradually the amount of radon used was lessened, and the time shortened until a lower limit was reached. Very satisfactory results followed the use of needles containing 5 mc., left *in situ* for about three hours.

Later the amount was still further reduced, and the time increased, leading to Regaud's system of needles of about 1 mc. left in the tissue for five or six days.

In Paris it is looked upon as essential that many of these needles should be placed not only actually in the nodular growth, but in the zone of tissue deep to it and surrounding it. This is combined with very thorough surgical removal of the lymph glands; for in cases of carcinoma of the tongue it is often found that however well the primary growth may respond to radium, secondary glands may prove resistant. Cases have recently been reported in which these glands have reacted to implantation of radon seeds; but it seems to be a better procedure that the glands should be removed by surgery. The cases are kept under observation over many years, so that expected and inevitable recurrences may be excised or treated at once when they appear.

Just at present there is a vogue for radon seeds. The surgery of access is being developed rapidly and the implantation of radon seeds sheathed in platinum is being used for almost every type of inoperable growth.

In this connexion I would like to point out that Regaud's method does not stop at the insertion of the needles. To do so would be to under-dose. Shortly after the needles have been removed, the patient is submitted to a fairly heavy dose of γ-rays applied from the surface. This irradiation, whether from radium, or from an X-ray tube, heavily filtered, is an essential part of the treatment. To omit it is to sacrifice a large part of the possible advantage that might be obtained.

Take another instance: a nodule of secondary breast carcinoma, adherent to a rib or to the sternum. Such a condition will sometimes continue to advance in spite of intense irradiation; whereas in recent years I have seen several such growths become entirely quiescent after comparatively weak applications of either X-rays or radium.

We must bear in mind the views of Ewing, Murphy, Bashford and others that the malignant cells are not acted upon directly, so much as indirectly by lymphocytic infiltration, fibroblastic proliferation, thrombosis of vessels and lymphatics, and probably other defensive measures of the surrounding tissues.

But growths differ in character even in the same patient. One must stop to consider whether one's objective is to act on the normal tissues, to secure this sensitive indirect effect on the neoplasm; if so, the dosage should be restrained.

When there is a rapidly growing tumour composed of a large mass of cells with relatively little stroma, the proposition seems to be different, and one should use intense radiation based on the lethal dose conception.

The accompanying photographs illustrate a case which bears on this point. The first shows an ulcerating spheroidal-cell mammary carcinoma, recurrent after two operations (fig. 1). The case was treated by Dr. Arthur Burrows and myself at the Radium Institute. There was complete retrogression with a restoration of the scars left by the previous operation.

The second illustration (fig. 2) shows this clearly, but I may add that in the skin even the scars of the sutures could be seen. The dosage was not heavy but the curious reappearance of the scar seems to prove that in spite of the ulceration the growth was essentially an interstitial infiltration of the tissues, and I have little doubt that the effect produced was due to such an indirect action as has been referred to.

The patient was warned of the possibility of recurrence. But she lived far out in the country, and she told me that she had been cured so easily that she did not worry when another tumour developed, further over to the right. This had become a very large mass when she reported. It was treated on the same lines as before; but, so far as I could judge, there was no response. The tumour progressed steadily and the patient ultimately died.

In the light of more advanced knowledge, if such a case presented itself now, the size and character of the mass would give warning that only a direct effect was possible, and I should endeavour to give an intense dose; bearing in mind the dictum of Quick that it is the last 10 per cent.—not the first 90 per cent.—of the dose which makes possible the desired result.

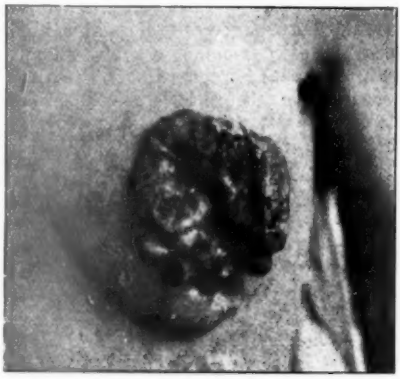

FIG. 1.

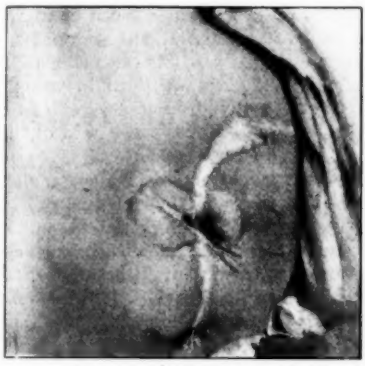

FIG. 2.

FIG. 1.—Recurrent ulcerating carcinoma mammæ. The breast and glands had been removed for carcinoma, and a previous recurrence had been excised from the scar area.

FIG. 2.—The same case seven months later. When the ulcerated tumour disappeared, the original scar and that of the second operation became evident.

[*These figures are reproduced by kind permission of Mr. Hayward Pinch.*]

In such cases Phaler advocates a saturation dose. A full 100 per cent. dose is given in one day, followed by a daily dose of 25 per cent. to 10 per cent., which is calculated to keep up the biological effect to a 100 per cent. dose for from 10 to 14 days.

Knox has written and spoken of endeavouring to sustain a continuous biological reaction over a long period with a view to establishing control of the disease. Holfelder writes of giving a full dose with very high voltage and very heavy filtration.

In all such treatments it goes without saying that every possible effort must be made to reduce the influence of the intense radiation on the healthy tissues to a minimum, and to prevent damage to the organism as a whole. When the full dose has been given a long interval must elapse before treatment is repeated.

Knox advocated differential blood-counts at regular intervals, and substituted general ultra-violet baths when progressive changes in the white cells count became evident.

There appears to be room for very considerable development of the use of ultra-violet light in combination with X-rays and radium in the treatment of patients suffering from inoperable carcinoma—radium locally; X-rays for surrounding lymphatic areas; and ultra-violet light for its general effects, particularly in combating lymphopænia.

One may draw an analogy between such a system and the treatment given in lymphatic leukæmia, in which if observations are kept on the blood-count the control of the number of white cells can be established by means of occasional small doses of X-rays distributed at intervals over long periods.

Thus, in inoperable uterine carcinoma, ultra-violet light should accompany the preliminary preparation of the patient to ensure the essential asepsis of the diseased area. Then radium should be used in the form of tubes, needles and seeds for the actual growth. Finally, a course of X-rays, carefully graduated by voltage and filtration, should be given with a view to the prevention of extension along the lymphatic channels and vessels.

One of the great advantages of radium is that with it relatively intense local effects can be obtained with comparatively little disturbance of the organism as a whole. But sloughing should not be produced. I always consider that a slough is evidence that the whole objective of the treatment, the retrogression of the disease without destruction of normal tissue, has failed. To use radium for a growth in order to produce a slough is as laudable as laudable pus!

At one time hopes were held out that particular tumour cells might show specific sensitivity to rays of certain wave lengths. But, with increasing experience, the important features seem to be the size and situation of a growth and its pathological structure.

The type of ray that is used seems to be of less importance, provided the effect can be produced without too much disturbance of adjoining tissues. Wave length is important, however, in this respect: the longer types can be safely used for superficial conditions, but they must be progressively shorter as one endeavours to influence deeper tissues without injuring the other structures traversed by the beam. Also when a general effect is desired over a large area, as in prophylactic or preventive treatment, it is now almost universally accepted that the medium ranges of voltage are most satisfactory; but because of their greater differential action the higher voltages are better if a local effect is to be produced on a localized growth at a depth.

When a case has been got under control and is apparently well, only too often the patient disappears, to return when the ascendancy of the disease has been re-established, and there is no reasonable hope of relief. I would like to suggest that it should be regarded as imperative for such a patient to attend for periodic preventive treatments, to be given at increasing intervals for two to three years. The doses need not be large, but if they are given the number of recurrences will be less.

Section of Psychiatry.

[*January* 8, 1929.]

Light Therapy in Mental Hospitals.

By H. Dove Cormac, M.B.

(*Lecturer in Psychological Medicine to the University of Manchester.*)

Abstract.—The position of actinotherapy in Mental Hospitals in this country is reviewed. An investigation of the results of ultra-violet irradiation in mental disorders at Parkside Mental Hospital is described and it is shown that certain types of the psychoses appear to benefit. The physiological action of actinic rays in relation to mental disorders is discussed and their mode of action on the nervous system suggested. Reference is made to substances which sensitize the body tissues to sunlight and ultra-violet radiation. An allusion is made to glass, penetrable by a portion of the actinic rays, and its uses. The need for ultra-violet ray apparatus in every mental hospital is urged both for treatment of mental and physical conditions and for the study of its action.

Introduction.—The value of sunlight in the maintenance of health has been recognized from early ages and history records sun-worship by many nations in the past as well as at the present time. In Europe records exist of the use of the light of the sun for medical and surgical purposes since before the Christian era, but it was only towards the end of the last century that its possibilities began to be studied and the value of the electric arc lamp as a substitute recognized. In 1893 Finsen demonstrated the value of sunlight and electric arc radiation in the treatment of lupus and at the beginning of the present century Bernhard of Samaden treated wounds and tuberculous lesions with sunlight. In 1903 Rollier opened his first clinic at Leysin, where he obtained excellent results in the treatment of tuberculosis, especially of the surgical type, by insolation in the brilliant Alpine sunshine. In 1908 Gauvain introduced heliotherapy at Hayling Island and at Alton. Nagelschmidt used the air-cooled quartz mercury vapour lamp for general irradiation in the same year and in 1913 Reyn commenced the use of the carbon arc for the same purpose. The value of ultra-violet rays in the cure of rickets was not recognized till demonstrated by Huldschinsky in 1918, though Palm [1] as early as 1890 urged that deficiency of sunshine was a cause of the condition.

During the past ten years greater attention has been directed in this country to the investigation of the effects of the actinic rays on the blood, endocrines and other tissues of the body. The press, both medical and lay abounds with allusions to the therapeutic use of sunlight, and at least two journals are entirely devoted to the subject.

Actinotherapy in Mental Hospitals.—A note in the report of the Board of Control for the year 1925 runs as follows:

> "The recognized value of ultra-violet radiation and of 'light' baths is prompting the authorities of mental hospitals to introduce this form of treatment into their institutions. While it is believed that treatment by actinic rays has an effective influence in ameliorating cases of neurasthenia, it might be thought premature to assert that it has any specific action in psychotic states. It is, however, only right and in harmony with enlightened opinion that the population of a mental hospital should have such modern facilities as are open to the general public for the treatment of such conditions as malnutrition, tuberculosis, autotoxæmic states, skin diseases, &c. Fortunately the requisite apparatus is not expensive."

The report adds that there were in 1925 one registered and four county mental hospitals provided with installations for this mode of treatment. Twelve months after the publication of that report we find the following statement by Dr. Edwin Goodall [2] in his Maudsley lecture:—

"Now that artificial sunlight treatment is being introduced in psychopathic institutions, its effect in basal metabolism in suitable cases should be recorded. Indeed, I consider it incumbent on those who have the installation for this treatment to furnish scientific evidence for or against the utility of the treatment in psychopathic cases—evidence based upon tests of function, blood-examination, &c. It will be time enough for mental hospitals in general to make provision when such evidence is forthcoming."

To Goodall's remarks no exception could have been taken if his concluding sentence had been omitted. The suggestion that the study of actinotherapy, in relation to psychopathic cases, should be limited to the staffs of the few hospitals provided with the apparatus is unfortunate. Such an attitude is incomprehensible in one so eminent in research and whose work has been so diffuse. Evidence such as he denotes would be more readily forthcoming if the research were extended to all mental hospitals. A keen spirit of inquiry exists in many medical officers, who would welcome the opportunity of studying the subject—and the cost of the equipment, as the Board of Control states, is not great.

An inquiry addressed by the Actinotherapy Sub-Committee of the Royal Medico-Psychological Association last May, to 200 mental hospitals, elicited forty-seven replies. From these it appeared that only sixteen hospitals had any apparatus, while eight hoped to instal one in the near future—five years after Horton Mental Hospital, the first in the field, was equipped.

In 1924, at Parkside Mental Hospital, enclosures were erected for male and female patients and provided with hammocks where courses of sun baths could be given. The summer that year provided a minimum of sunshine therefore little experience was gained as to the value of the treatment. It was then decided to instal electrical apparatus for actinic radiation. Adopting the suggestion of Turrell [3] in his book, an 80-ampere cinema arc lamp was obtained and about fifty patients treated during the year. Later additions to our equipment included six of the conventional 25-ampere carbon arcs, in two series of three, for general irradiation of groups of eight to ten patients, and three quartz mercury vapour lamps. The two types of lamp were obtained because of the difference in penetrating powers of their rays; experience showed that alternate use in treatment produced the best results. Details of equipment of a room for actinotherapy can be found in most text-books on the subject, but a rubber carpet is needed for insulation to prevent accidents in treating mental cases.

During 1925 an investigation was undertaken of the results of irradiation of about fifty cases treated for periods varying from two weeks to two months. An 80-ampere carbon arc was used, the electrodes being, in cinema parlance, "high intensity" carbons. These were selected after trial, because the best reaction was obtained by their use; I believe they are impregnated with nickel. Six to eight patients were treated at a time by irradiation of the whole body, all suitable new admissions during the year being chosen. No cases of epilepsy, mania, or general paralysis of the insane were included. The dosage was fixed at five minutes for the initial exposure of the front of the body and five minutes for the back, increasing each by one minute daily to the maximum of thirty minutes, at a distance of three feet from the arc. The metabolic rate was taken before, during and after the course of treatment, in each instance before the patient rose from bed in the morning. The blood-pressure was recorded and a full differential blood-count made at the same periods. The body weight was recorded weekly and regular notes made of mental variations, appetite, sleep, skin condition, etc.

The results showed that there was a marked and general increase of the body weight, averaging $3\frac{1}{2}$ lb. for each patient. The appetite generally improved and most patients showed greater muscular activity, with concomitant exhilaration and cheerfulness. The metabolic rates and blood-counts showed no constant variation. The blood-pressure records indicated a slight average decrease of the systolic pressure.

During the investigation it was early realized that, in melancholia with agitation, and in cases of hyperthyroidism, the condition was aggravated. Manic-depressive cases responded most readily, the period of depression being considerably curtailed, while mild cases of melancholia also benefited. Delusional cases, with much mental irritability due to the delusions, became more amiable, the delusions were less frequently referred to and did not appear to trouble the patient while undergoing treatment. In dementia præcox there was no amelioration of the mental condition, but some cases showed greater motor activity and talkativeness.

Since the above investigation was undertaken the results broadly defined have been confirmed. In addition, treatment of cases of confusional insanity, some cases of dementia præcox and anergic stupor has yielded good results.

K. K. Drury [4] records similar conclusions as to the types of mental disorder that appear to benefit and states that, in acute mania, the patient becomes quieter during exposure, but immediately afterwards grows more and more restless and noisy. In epilepsy he found that the physical condition improved, but that the number of fits increased by from 20 per cent. to 150 per cent.

Physiological Action of Actinic Radiation in Relation to Psychopathic States.—Actinic radiation effects a general improvement of the physical condition, increases body weight and improves muscle tone (Rollier refers to sunlight as the "best masseur"). Colebrook, Eidinow, and Hill [5] and others have shown that the bactericidal power of the blood is augmented, and Humphris [6] states that the immunizing power of the body is increased. These effects must materially assist in the therapeutic value of local treatment directed to eliminate sources of infection in those cases, which Cotton [7], William Hunter [8] and others ascribe to a toxæmia derived from a focal sepsis or intestinal absorption. These cases, which they would group under "septic psychoses," include dementia præcox, manic-depressive insanity, paranoid conditions, psychoneuroses and toxic insanities, in most of which our best results have been obtained by irradiation.

Saleeby [9] states that actinic irradiation raises the iron content of the blood and the iodine content of the thyroid, and I have noted an increased thyroid activity in cases of hyperthyroidism and agitated melancholia. This action indicates the benefit to be expected in exposure of those dull and lethargic cases which are often due to thyroid insufficiency. Grant and Gates, of the Rockefeller Institute (quoted by Howard Humphris [6]) recorded that the weights of the endocrine glands were all increased in experimental rabbits. The parathyroids showed the greatest hypertrophy and the calcium content of the blood was increased. T. C. Graves [10] treated with calcium lactate cases of excitement in epilepsy, all forms of mania and agitated melancholia, with satisfactory results. His treatment was based on the assumption (from Bayliss' view that calcium is necessary for the normal effect of adrenalin on sympathetic endings) that, though there could be no lack of adrenalin in the body in these cases, there was an absence of its "fixation ion."

Levick, quoted by Edgar Mayer [11], noted that the neurasthenic symptoms often accompanying rickets were the first to clear up under ultra-violet ray treatment. E. H. and W. K. Russell [12] state that they have treated many cases of neurasthenia and that all but two have benefited greatly. Humphris [6] states that in no disease may greater benefit be derived from ultra-violet radiation than in this condition, and that the benefit may be due to alteration in the blood chemistry, the endocrine balance or to the bactericidal improvement in the blood, or to all three. We should remember that many cases of so-called neurasthenia are early cases of melancholia with hypochondriacal tendencies.

The action of the rays on the endocrines should be of interest to that school of thought which holds that most kinds of mental disturbance are a direct result of faulty functioning of the endocrine system, and it cannot be denied that there is a

close relationship between the internal secretions and the mental state. But, where there is a disturbance of the endocrine balance, it has yet to be determined whether irradiation will tend to restore that balance or to accentuate the disturbance. My experience goes to show that only when the disturbance is slight can any improvement be expected.

Many observers have noted a marked reduction of high blood-pressures, which may give rise to certain psychotic states, and that in this respect the carbon arc was more effective than the mercury vapour lamp. They also noted that when the blood-pressure was previously normal no decrease could be demonstrated.

It is generally held that the ultra-violet rays affect not only the vasomotor system but also other parts of the vegetative nervous system, such as those controlling the pigmentation of the skin, the fibres supplying the hair follicles and those which affect muscle tone.

I have been able to trace few references to work on the action of actinic radiation directly on the nervous system, but that of J. D. Achelis and H. Rothe [13] appears of sufficient importance to be noted. They investigated the state of excitability in the external cutaneous nerve before and after irradiation of the arm below the site of experiment and used the other arm as a control.

The result of seventeen tests showed that :—

(1) The sensory nerve which innervates a radiated region goes through a variation of excitability during the latent period.

(2) Four phases could be distinguished : (*a*) A small increase of excitability. (*b*) A short variable period. (*c*) A strong slowly increasing diminution of excitability. (*d*) A fairly rapid return to normal when the erythema appears.

This rhythm in variation might be continued for some time with gradually decreasing intensity. The phases (*c*) and (*d*) were constant but the phase (*a*) was not always observed.

They comment on the marked and constant reduction of excitability, which is not limited to the place of irradiation, but can be shown along the course of the nerve below and above the stimulated area. They conclude that the whole neuron is implicated in the change and it would seem to be justifiable to infer that the whole of the sensory division of the nervous system is influenced by general irradiation. This reduction of sensitivity is to me analogous to the action, in small quantities, of the large group of anæsthetic and hypnotic drugs, including alcohol, which produce at first a feeling of exhilaration and mental vigour, with corresponding removal of care and annoyance. Hirschfelder and Serles [14] showed that these drugs, in spite of their chemical diversity, possess an important physico-chemical property in common. When added to an emulsion of oil and water they convert the oil-in-water phase into the water-in-oil phase. The effect of this is greatly to raise the electrical resistance of the emulsion.

The *Lancet* editorial of December 17, 1927,[1] commenting on the above, states : "It seems a justifiable hypothesis that these drugs act in the same way in the body and that, by altering the phase of the emulsion of lipoids, which constitutes the cell membranes of the neurones, they reduce the electrical conductivity in the nervous system, possibly at the synapses."

This leads to the question whether the action of the actinic rays on the sterols has any affinity with the above reactions.

The action of ultra-violet radiation then, in reducing the conductivity of the sensory nerves, produces a sensation of well being with mild excitement and freedom from care and annoyance, and may be said to paralyse certain inhibitions. Thus its beneficial action in neurasthenia, mild melancholia and the depressed stage of manic depressive insanity may be understood, as well as the aggravation of excitement in cases of mania. Here we find the explanation of its analgesic action also.

[1] *Lancet*, 1927, ii, 1299.

It has been suggested that the change in the excitability of the nerves may be due to direct action of sunlight on the nerve endings or to some chemical substance produced in the epithelial tissues, or to a hormone secreted by the skin.

Sensitizors.—Certain dyes and other substances can sensitize tissues to a radiation which would otherwise be without action. These have been termed "sensitizors," and they include eosin, methylene blue, iodides, bromides, hæmatoporphyrin, etc. Coal tar products, such as sulphonal, may produce a temporary hæmatoporphyrinuria in certain people who would at these times be extremely sensitive to the action of light. Beri-beri, an avitaminosis due to a diet of polished rice, is probably partly due to the action of sunlight on the sensitized individual. Pellagra, attributed by some to an avitaminosis, by others to the eating of corn or maize infected or altered in some way by the method of preparation, is also suggested to be due to sensitization. Drury [4] has, on the other hand, reported a case of pellagra in which irradiation resulted in considerable improvement.

Hypersensitiveness to the light may occur as an idiosyncrasy in some people. This possibility should be remembered in deciding the dosage for any particular case. E. H. and W. K. Russell [12] give a fairly long list of drugs as well as certain articles of diet which enhance the effects produced by ultra-violet radiation.

"*Vita*" *Glass.*—A discussion on actinotherapy in mental hospitals would be incomplete without allusion to glass which is partly penetrable by the actinic rays, the best known being "vita" glass. Ordinary window glass cuts out most of the rays, but "vita" glass, "sun-ray," and other varieties of this type, are said to have the property of transmitting rays up to 2,750 A.U., and are thus freely transparent to all the ultra-violet rays which occur in sunlight. This glass is being largely used in schools, hospitals, and private houses as well as in the London Zoological Gardens and for cattle sheds, stables, etc., with beneficial results.

As it is considerably more expensive than ordinary glass many hesitate to recommend it for verandahs without actual experience of the results from its use. To these I would suggest the erection of a small "vita" glass canopy, with side curtains,over a hammock for experimental purposes. Such a structure should entail a cost of no more than £10 or £12.

Conclusion.—Actinotherapy has been practised at Parkside Mental Hospital for nearly four years, and, from my experience, is a very necessary adjunct to the treatment of mental cases. It is employed as a routine for convalescent cases, whose recovery is thereby hastened.

Care is necessary in the choice of cases for treatment, the type which benefits to the greatest degree having already been indicated. Mention should, however, be made of the excellent results obtained in dementia præcox of the simple type, and also in the hebephrenic type after the acute phase has passed.

It is my opinion that every mental hospital should be equipped with ultra-violet ray apparatus for the treatment of mental disorders, as well as for the many physical diseases which are ameliorated by its use. Opportunity should also be given for more extensive study of this subject, which would give scope for wider research into its physiological relation to the psychoses and the nervous system generally.

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DISCUSSION.

Dr. ALLEN HANCOCK said:—At Horton Mental Hospital, at which pioneer work was done with ultra-violet light apparatus, treatment was carried out by direct sun rays as well as by various types of lamps for producing artificial sunlight. Sun baths were given by exposing the patients to the rays on couches in the gardens. General baths were given as well as exposures for treatment of local conditions, such as chronic ulcers and septic sores.

The types of lamps utilized for indoor treatment were: (1) Mercury vapour, (2) tungsten arc, (3) carbon arc.

The carbon arc lamps were those chiefly in use and were set up in batteries of two, arranged in series. These were especially used for general radiation and were selected for several reasons: e.g., they could be cheaply set up; many cases could be treated at once; the rays from such lamps were those most akin to actual sunlight.

Treatment was carried out along the lines of the school which avoids pigmentation or desquamation (both of which are protective in nature), and aims at keeping the skin in a light-sensitive state. Every patient was first examined; careful notes on both the physical and the mental condition were made, and particulars were entered on a special form. The minimum erythema dose was observed by the sleeve method, the distance (usually two metres) and the type of lamp used were noted, and treatment was prescribed by the medical officer in charge of this department. In order to make sure that the parts exposed were light-sensitive, the regular method was to begin with one minimum erythema dose to one-quarter of the body. The body was divided into four areas, each representing one quarter; thus, there was a front upper area, a back upper area, a front lower area and a back lower area.

These areas were treated successively with one minimum erythema dose on four days in the week. Each exposed area would thus have a rest for six days before the next exposure, and would remain light sensitive. The patients were made comfortable in chairs with, of course, the usual protection for the eyes; the chairs were placed in a circle around the lamps, with a protecting rail between.

Dr. Dove Cormac lays stress on rubber for the floors, but at Horton we found thick cork linoleum most satisfactory and warm for the patients' feet.

Light treatment was carried out for certified patients in conjunction with other forms of treatment, such as hydrotherapy, occupation, and, in many cases, thyro-manganese treatment. This last method, together with treatment by ultra-violet rays, has yielded most satisfactory results, especially to those cases which may be grouped together as "psychoses of toxic cause."

Undoubtedly in many mental cases, the patients, both certified and non-certified, owe their mental breakdown to some abnormal physical condition. In some cases this is obvious, in others some indefinite septic focus may be found after careful examinations. There are, too, many mental hospital patients in whom no definite focus of infection can be traced, but who are obviously toxic and unhealthy looking, and to whom the term "sun starved" can be applied.

Experiments by eminent observers[1] prove the action of light in increasing the natural bactericidal power of the blood and show this effect to be principally due to the ultra-violet rays. It seems rational, therefore, to assume that the mental cases mentioned in the above groups should benefit by any treatment that would improve the physical state upon which the abnormal mental condition depends. Observations on the cases which have been treated undoubtedly justify the use of sunlight, both real and artificial.

In cases with obvious septic foci, treatment was given for the local condition, but in all cases general radiation was carried out also; the results were, as a rule, rapid, satisfactory and lasting. Such good results were probably due to two factors; firstly, the removal of the septic focus, resulting in improvement of the general physical condition; secondly, the removal of anxiety as to the nature of the ulcer or discharging sinus which may have been looked on as incurable, and so have had a most depressing effect on the mental state of the patient.

In those cases with less obvious physical abnormalities, general treatment was carried out along the lines described earlier in the paper, and observations were made at regular intervals both as to physical and to mental changes. Physical changes were checked by special observations on weight, menstruation, blood-pressure, red-blood cells, hæmoglobin and colour index. Three lines were left on the form for other special items according to the wish

1 Dr. Leonard Hill, "Sunshine and Open Air."

of the worker. The basal metabolic rate has been checked by some observers, but this has not yet been done in the cases we have treated. In addition to special observations at given dates, remarks on the general physical state and on the mental condition were made at the same time.

In noting the effect on the mental state, I think that much stress should be laid on what the patient says of the treatment. In a great number of cases one finds that they volunteer the remark that they feel "better in every way" and ascribe the improvement to "that sunlight treatment." Whether the physical findings record similar improvement or not does not matter, although as a rule this is found to be the case. So long as we can get patients who appreciate the treatment, and of their own accord state that they benefit by it, we are going in the right direction towards the treatment of patients afflicted with mental disorder.

I am indebted to Dr. Dorothy Preston Hytch for help in making blood-counts, keeping records, etc., along the lines of team work as carried out at Horton Mental Hospital.

The chief types of cases on which observation was made were: dementia præcox, confusional insanity, melancholia, manic-depressive insanity, and the large group already referred to under the heading of "toxic insanity," which includes some cases of the above special types, as well as others in which a toxic condition was the cause, either alone or in conjunction with some psychogenic cause.

Generally speaking, all the toxic cases, whatever the most prominent symptoms, showed marked benefit, especially those in which light therapy was combined with thyro-manganese treatment.

Of the other types we found that confusional cases showed only slight improvement. In cases of melancholia if the patients were agitated, restless and influenced, by hallucinations and delusions, improvement was not observed and treatment often had to be discontinued.

In mild cases of melancholia, especially of melancholia associated with the menopause, there was marked and often rapid improvement. In notes on many such cases there are found entries such as "marked improvement mentally and physically"; "enjoys light treatment, expresses appreciation and hopes to continue it"; "now cheerful, hopeful of the future and full of gratitude for the treatment." Such reports on these cases are most frequent and are found in about 70 per cent. of cases of this type.

Cases of dementia præcox did not, as a rule, benefit, but most cases of the simple type showed decided improvement, both mentally and physically, and here I may quote notes from a case under treatment as being typical of many found in the simple type of dementia præcox. At commencement of treatment: "Dull, deluded, hallucinated, unoccupied."

After three months: "Slight improvement." After six months: "Slight improvement; now starts work; enjoys light treatment." After twelve months: "Works well; contented and says she derives benefit from the sunshine treatment." Weight increased more than a stone.

Manic-depressive: These cases showed variable results, but frequently the treatment could not be continued, and, after periods of radiation, patients were often more restless than before.

It may be of interest to give information on changes noted in the cases which derived benefit. In all of these there was an appreciation of the treatment and a more hopeful outlook as to the future. The improvement was shown by a more healthy and less "sun-starved" appearance, an increase of appetite and briskness in movement.

Among the special observations, the records show that:

The blood-pressure showed no constant variation. Weight showed an invariable increase, often most striking, usually amounting to a stone or more. The red blood-count, as a rule, showed a slight increase. The white blood-count, too, showed an increase in the number of leucocytes.

Neurasthenic cases have not come under our treatment in this series, but, being interested in letters on this subject in medical journals recently, I have obtained some extensive and instructive notes on cases treated by Dr. Elizabeth Mudie, showing the beneficial results obtained by the use of ultra-violet rays.

I must mention certain cases of definite malaise caused by this treatment. These occurred in members of the nursing staff, and in myself, all being apparently healthy, mentally and physically. After about half an hour in the treatment room, giddiness, cold sweat, nausea and rapid action of the heart were experienced. The nurses frequently had these attacks; I, however, had one only. It would be interesting to know if others have had reports of similar experiences.

Sir HENRY GAUVAIN said: My contribution to the subject under discussion, while not strictly relevant to work in mental hospitals, may nevertheless have a bearing on the light treatment of mental cases.

Investigations on the subject of light treatment have been largely confined to the clinical and biological effects recorded. Little attention has been drawn to the mental effects which follow rational and systematic exposure to light, and, while the alertness and vivacity of children undergoing treatment in hospitals where the sun cure is practised have been remarked upon by numerous observers, both medical and lay, few actual measurements of alterations in mental activity following such treatment have been recorded.

The subject has engaged my attention for a number of years, and, without detailing reasons for arriving at the following conclusions, I simply state that even as a student I recognized, perhaps instinctively, the value of lengthy summer holidays with abundant exposure to sun and coincident enjoyment of sea-bathing. The gain in physique to healthy adolescents cannot be over-emphasized and the effect on mental activity is equally marked. The value of the treatment is lessened by exposures continued over too long a period. I believe that from two to three months annually, represents the optimum for most subjects.

Irradiation with ultra-violet light has a tonic and stimulating action. Its prolonged exhibition, as in the case of other stimulants, is associated with progressive loss of response. Its excessive use produces depression, listlessness and irritability, which may proceed to exhaustion. There is marked divergence in individual response to exposure. A comparative estimate of the mental response to light exposure was afforded by the following experiment: A ward was selected containing twenty children under the age of five, all suffering from active tuberculous disease of the spine, and all being immobilized and treated recumbently. Young children were chosen in order to exclude the factor of conscious suggestion. Ten children on one side of the ward had regular and systematic artificial light treatment for a period of six weeks in the middle of winter. In other ways, treatment, diet and environmental factors of all the children were precisely the same. While the physique of those receiving light treatment showed improvement as compared with the others the mental effects were even more marked. Those exposed to light were distinctly happier, more vivacious, more alert, and, I may add, more mischievous. They would often laugh and sing, and appeared to be overflowing with animal spirits, while their fellows remained silent. They were notably more difficult to restrain. The contrast in appearance, spirits and mentality was most striking and was remarked on not only by those concerned with their care—the doctors, nurses and teachers—but also by casual visitors.

The ability of properly planned, intermittent insolation to evoke mental activity has been abundantly illustrated in my practice over a period of twenty years during the treatment of better class children, whose education, which would be of public school character, has, owing to their disability, been interrupted for prolonged periods—in some cases for several years. Notwithstanding their handicap, these patients, when able to return to normal life, have, with remarkable consistency, caught up—and often outclassed—their more fortunate fellows who had enjoyed the advantage of uninterrupted education. So frequently has this been the case that it cannot be regarded as accidental. The consistency of the mental speeding-up, which is more marked in the patients now referred to than in the ordinary hospital class, pointed to the existence of some mental stimulus common to all. By a process of exclusion the common factor suggested was intermittent and carefully-planned exposure to sunlight.

There is reason to believe that interrupted and periodic insolation is superior to continual sun treatment. Repeated exposure over prolonged periods seems to result in progressive diminution in response, while intermittent exposure, with periods of abstinence, results in stimulation. Insolation in this country would therefore appear to be especially valuable. Excessive heat, coincident with insolation as obtains in tropical countries, is decidedly exhausting and enervating. Light treatment in cool air, aided at certain stages by more intensive light associated with graduated sea-bathing, if properly planned, raises metabolic activity enormously and the resulting gain in physical development is associated with at least equal improvement in mental vigour and activity.

Independent confirmation of these statements will be found in the annual reports of Mr. A. J. Green, the headmaster of the L.C.C. open-air school at Stowey House, Clapham Junction, who, on my suggestion, has been permitted by the Council to introduce sun-treatment during the summer months since 1923.

The above statements, however, need verification by actual tests of mental activity on a sufficiently large number of cases, with controls. Such evidence I am able to produce, and it will be found set out in detail in *Maternity and Child Welfare*, July, 1925.

The mental tests were undertaken by Dr. C. R. McCrae. Mental tests were applied by him to children in special schools for physically defective children in London and to patients at the Lord Mayor Treloar Hospital, Alton. At Alton all the crippled children examined were unselected, but having either natural or artificial light treatment, or both. In London, 117 children of the average age of 11·13 years were examined; at Alton, 62, of the average age of 11·85. According to the tests applied, the London children showed a mental retardation of 1·95 years, the Alton children of 1·14 years, as compared to the normal child of equivalent age. Our object was to account for this difference of almost a year, and the conclusion forced upon us was that the difference was due to the light treatment which the Alton children enjoyed. Important confirmatory evidence was afforded by a comparison of the results between children in the wards receiving light treatment and our lads in the college attached to the hospital, who were not being so treated. These boys are physical defectives, fifty in number, residing in the institution under essentially the same environmental conditions, and receiving education and technical training, but no treatment. It might be reasonably expected that the college boys would prove mentally superior to the ward children because of the education and training they were receiving. Actually the exact reverse was established.

To both groups of children a variety of tests were given: the Binet-Simon test, an oral group test, performance tests, i.e., tests in which the subject deals with actual material instead of with words and symbols, picture tests and scholastic tests. In all cases there was a marked difference between the two groups, and always in favour of the children in the wards. On the average, the mental ratios of the college boys were at least ten points lower than those of the children in the wards. In other words, while the ward children were of a mental age which might be regarded as almost normal, that of the college boys would be regarded as definitely subnormal.

These investigations were not intentionally undertaken to show the bearing of light treatment in evoking mental activity but merely for the purpose of comparison of the mental intelligence of physically defective and otherwise normal children. It was only when considering the results of this research that we discovered the differences I have outlined and then sought an explanation for the findings.

While the bodily improvement and the raised metabolism noted as the result of exposure to sun and open air, would of itself be likely to have an indirectly beneficial effect on mental activity, it is unlikely that this would be the full explanation of the increased mental activity which follows such exposure. In the study of the chemical changes in the blood following exposure to ultra-violet light, which have recently attracted the attention of so many investigators, it has been demonstrated that such exposure is followed by an increase in the blood phosphate. Is it not a reasonable hypothesis to present that, not only may bone repair be induced in the treatment of rickets by exposure to ultra-violet light, but other tissues may also be nourished and fortified? The suggestion is advanced, which further investigation may confirm or disprove, that ultra-violet light, shown to be an important factor in effecting tissue change, may thus improve the nutrition of the grey matter of the brain and in this way increase the output of mental activity. In the course of the physical improvement in rickets, a synchronous improvement in the mental condition and behaviour of the patient is often strikingly manifest.

Inferences.—The facts stated above, if confirmed by further investigation, must have an important bearing on the education not only of the normal but also of the subnormal or ailing child.

With regard to the applications of this method of treatment I would, however, suggest: (1) That careful technique in treatment, varying with individual needs, idiosyncracies and powers of response, is essential; (2) That intermittent periods of treatment are preferable to continuous treatment; (3) That contrary to what is often thought, the climate of the South of England lends itself very readily to sun treatment; (4) That artificial light treatment by means of the short-flamed carbon arc as used at the Finsen Institute and by ourselves is the best artificial light treatment available, permitting sufficiently prolonged periods of exposure to be given; (5)[1] There are seasonal and even diurnal variations in response to treatment and this fact should be borne in mind when planning it out; (6) General light baths, for whatever purpose employed, should only be regarded as auxiliary means of treatment, though often of great value, and not be allowed to supplant but merely to supplement other methods of

[1] Gauvain: Light Treatment in Surgical Tuberculosis. *Lancet*, 1927, ccxii, 754.

proved utility; (7) That light treatment is supplemented and made of greater value by the associated use of other natural aids, e.g., climatic and environmental change, exposure to open air, and sea-bathing, when these are used with discrimination.

Two additional remarks are prompted by Dr. Cormac's paper:—

Dr. Cormac mentioned the use of rubber for the floors of an Actinotherapy Department. While such floors would, of course, be admirable, simple asbestos sheets would, from the point of view of economy, be found very useful. I have found them excellent for the purpose. They are non-conducting, decidedly cheaper than rubber, easily replaced if cracked, and not tiring either to the feet or to the eyes.

I have been using vitaglass ever since it was invented, and have found it very useful in the form of sliding roofs on balconies. In the fine weather the patients may be completely exposed to the natural sunlight, and when it it is raining there is no need to send them back into the wards; the vitaglass roof is simply lowered over them, and whatever light is available is transmitted with a minimum amount of loss. Especially do I like vitaglass as a roof light.

Dr. G. de M. RUDOLF said: Before entering upon the discussion as to the value of ultra-violet rays in mental hospitals, I should like to follow Dr. Dove Cormac in the use of the term "ultra-violet rays." As ultra-violet rays are invisible, the term "ultra-violet light" or more simply "light" is scarcely applicable to these rays. As shown in the diagram, the visible, or light rays, are bounded on one side by infra-red or heat-rays, and on the other by ultra-violet rays. As the infra-red rays are not termed light rays, being invisible, one would similarly not expect to term the ultra-violet rays, which are also invisible, light rays.

Light rays are only those rays which can be detected by our eyes. They are used in colour treatment such as Murray Levick's red ray treatment, and the combination of all the visible rays or white light is also of possible therapeutic value.

Turning now to the value of ultra-violet ray treatment in mental hospitals, there is no need to enlarge upon the treatment of the usual complaints dealt with in general hospitals, for if ultra-violet ray treatment is of value in general hospitals for certain conditions, it is clearly of value in mental hospitals for the same conditions. From this aspect alone, I consider that every mental hospital should be equipped with the necessary apparatus, and the experience at Claybury in the treatment of 185 patients during the past three years fully endorses my belief.

When considering the value of ultra-violet ray treatment, other factors, besides the rays, must be considered. The work by Rollier at first suggested that the ultra-violet rays were the source of the improvement obtained, but Leonard Hill's researches have shown that some of the benefit, particularly with regard to the basal metabolism, is due to the effect of cool air upon the body and to the effect of inspiring cool air. These observations tend to make us ponder as to whether we administer ultra-violet rays by correct methods. Should we play cool air upon the body at the same time as the ultra-violet rays are applied? Dr. Dove Cormac's report upon the basal metabolic rate shows, I think, that the ultra-violet rays by themselves do not change the metabolism of the body.

The experience at Claybury seems to teach us to be very cautious in our judgment of the effect of ultra-violet rays upon mental and physical disorder, and two groups of cases will be given as examples of the difficulty of assessing the value of this treatment.

General paralytics previously treated with malaria were first treated with ultra-violet rays at Claybury in 1926, and since that time there has been opportunity for watching the results of this additional treatment.

As the administration of quinine to a paralytic after malarial therapy is usually followed by an improvement in general health, it is difficult to decide whether an additional treatment has given additional benefit. No adequate control cases can be obtained. My impression, however, is that the patients to whom ultra-violet rays were given improved in their general health more rapidly than did those who did not have this treatment.

The second type of case in which there is difficulty in obtaining adequate controls is the poorly nourished, pale and depressed type. As new admissions of this type to a mental hospital usually improve on account of better food and longer rest, there is no justification for saying that ultra-violet rays applied in these cases have benefited them unless a series of accurately recorded control cases is also obtainable. We have studied in particular the weight of the poorly nourished melancholic at Claybury, excluding all recent admissions, and we find that the majority of the patients show a distinct increase. This increase may take place during, or shortly after, the cessation of the treatment. But another fallacy occurs

here. Some patients increase in weight with no treatment; for instance, the chart exhibited shows the increase in the weight of a patient during and following ultra-violet ray treatment [chart shown]. If, however, we turn to the records of the case before ultra-violet ray treatment was instituted, we find that the weight increased then also.

Although, on theoretical and practical grounds, ultra-violet ray therapy is of value, care is needed in assessment of the degree of value unless adequate control cases are at hand. Nevertheless, the opinion that I have obtained during the past few years of the treatment at a mental hospital is definitely in favour of the equipment of every mental hospital with the necessary plant.

Dr. R. B. BOURDILLON said: I wish to emphasize the need for more light in actino-therapy—more visible light, and less invisible radiation.

At present most actino-therapeutic treatment is carried out with mixed radiation including the infra-red, visible and ultra-violet groups. Of these groups the ultra-violet has the most obvious effect, and the most harmful if in large doses, and in order to avoid this harm, the possible dosage of visible and near infra-red radiation is limited to an amount which can have very little effect.

Indeed, much actinotherapy can be compared to a prescription ordering a uniform dose from every bottle on the top shelf of a druggist's shop. In such prescribing the dose of the mixture has to be controlled by that of the most potent constituent—with obvious loss of efficiency. As an instance of this, consider the relative amount of visible light in sunshine and in the light from a mercury vapour lamp. A patient at sea-level in temperate latitudes exposed to mid-day sunshine in September, receives about 0·5 gm. cals. of visible light per sq. cm. per minute.[1] A patient at 3 ft. from a powerful mercury vapour lamp (400 watts) receives about 0·01 gm. cals. per sq. cm. per minute, i.e., only $\frac{1}{50}$th of the former amount. Thus, as far as visible light is concerned, an ordinary mercury lamp dose of 15 minutes is equivalent to only 18 seconds of September sunlight. It is thus most misleading to talk of treatment from mercury arcs as light treatment. It should be called ultra-violet treatment.

Carbon arcs are in an intermediate class between mercury lamps and sunlight. The quality of their radiations varies so enormously, according to the nature of the electrodes and other factors, that it is difficult to give exact figures. It may, however, be said that the iron-cored carbons or the tungsten arcs give radiation relatively similar to that of mercury arcs in its high ratio of ultra-violet to visible radiation, while white flame arcs, especially if given through glass or vitaglass screens, are relatively similar to sunlight, and can be used like sunlight with prolonged exposure of the patients. A 20-ampere plain carbon arc at 3 ft. gives about $\frac{1}{2}$th of the visible radiation found in sunlight.

What are the distinctive effects of light treatment and ultra-violet treatment? The true answer to this question is, that very little is as yet known with certainty, since most work has been done with mixed radiation. It is, however, certain that ultra-violet radiation has two effects on patients. It produces an antirachitic vitamin, and it burns the skin. The antirachitic vitamin can now be given more cheaply and more easily by the addition of radiated ergosterol to the patients' diet than by ultra-violet treatment, and I would strongly urge such addition to the diet during the winter months in those mental hospitals where considerations of economy make it difficult to ensure a sufficient natural supply of vitamin D.

The burning of the skin is an effect of ultra-violet radiation, the value of which is difficult to appraise owing to its secondary effects on hæmobactericidal power, etc. I would, however, suggest that the effects are not necessarily beneficial when applied indiscriminately to large numbers of mental cases, and that all intensive ultra-violet treatment should be used only in specially selected cases, or even applied by specialist physicians.

About light treatment, even less is known than about ultra-violet treatment. It can however be stated with certainty that red and yellow light and the near infra-red radiations penetrate human tissues far more deeply than ultra-violet radiations. Moreover, heliotherapy gives magnificent results in its special field of surgical tuberculosis, and there is some reason to believe that these effects are largely due to the light and near infra-red radiations from the sun. It is further possible to say that baths of visible light and near infra-red radiations from which the far infra-red and far ultra-violet are removed by glass screens, are very pleasant and cheering to patients. I think they are worth using for this effect alone in mental hospitals where cheering and mentally stimulating effects are of such especial value.

Two instances of the value of visible light as distinct from far ultra-violet may be quoted

[1] Calculated from data given by Coblentz, U.S. Bureau of Standards, *Bulletin* 378.

in indirect support of these arguments. At the London Zoological Gardens gas-filled incandescent tungsten lamps were, at Dr. Leonard Hill's suggestion, installed in the lizard house. In order that the lizards might get ultra-violet light these lamps were made with vitaglass bulbs. The effect on the lizards was very great. Instead of being lethargic and refusing to eat, and dying, they woke up, took an interest in their food and lived. This effect was at first thought to be due to such ultra-violet radiation as the lamps emitted. It is now known that these lamps give very little more ultra-violet radiation than ordinary domestic lamps, and it is nearly certain that this improvement in the lizards was due to the visible and infra-red radiations.

Another instance of some interest is given by the plant growth in Gough's Cave at Cheddar, in Somerset.[1] This cave is lighted in places by incandescent electric lamps for the convenience of tourists. Two years ago the 60 c.p. lamp bulbs were replaced by 120 c.p. gas-filled tungsten bulbs of the type now in very common use. Since that change a growth of mosses, ferns and liverworts has been noticed within a radius of about 6 feet from most of the lamp bulbs.

In conclusion I would urge that the great utility of ultra-violet radiation in certain diseases should not be allowed to blind us to the possibly greater value of radiations of longer wave-length, especially in the treatment of mental conditions.

Dr. F. A. PICKWORTH said: It is perhaps rather important to call attention to a point in the treatment of mental hospital patients with artificial sunlight which makes this treatment, from a theoretical point of view, unsound for patients with certain types of intestinal infection. It is well known that certain infusoria, when treated with fluorescent substances in the dark, are not killed until they are exposed to light. In the fæces of some of the patients of the Birmingham Mental Hospitals—and, I have no doubt, every other mental hospital—there occurs an abnormal intestinal flora, in which *B. proteus*, *B. fluorescens* and *B. pyocyaneus* are by no means rare. It therefore seems fairly certain that the fluorescent pigments produced by these organisms occasionally find their way into the blood-stream; or what probably amounts to the same thing, cause an alteration of the blood-pigment, which then possesses this property of changing the longer rays, which can penetrate the skin, to the shorter ones which damage tissue. An illustration of this is afforded by one of our cases in which a skin lesion developed and became vesicular and pustular on that part of the skin exposed to sunlight—not on the covered parts. On so many occasions the lesion rapidly disappeared when the patient was kept indoors and reappeared when exposed to sunlight that the sunlight was definitely proved to be the exciting factor. During this period the patient had diarrhœa, and a bacteriological examination showed *B. pyocyaneus*, which organism, together with *B. fluorescens*, was found on post-mortem examination in the nasal sinuses, lungs and intestine. Artificial sunlight should therefore not be administered to patients suffering from certain types of acute or chronic intestinal infection.

Dr. W. A. POTTS said that with regard to the difference in value of the carbon-arc lamp and the mercury quartz vapour lamp, one possible reason why the carbon arc should be more beneficial in some cases had suggested itself to him because he had been using sunlight treatment for some time, both in private practice and in Hallam Hospital, and the results had been noticeably different in cases treated privately from those in cases treated in hospital. In the treatment of mental cases in hospital, ultra-violet irradiation had been distinctly effective, whereas in private practice the results of sunlight treatment had been disappointing. In the hospital there were obvious advantages, as the treatment was supervised by Dr. Dyas, an expert in radiology, with whom he (the speaker) had discussed this point. Dr. Dyas's suggestion was that in private treatment the patient went alone to what he looked upon as an ordeal, so that he was apprehensive and afraid, and this state of mind interfered with the efficacy of the result; whereas in the case of hospital treatment a number were taken together, and, backed up by the presence of the others for a similar purpose, they regarded it as a kind of social event. Dr. Dyas also said that when any of the patients seemed depressed, he was placed next to a cheerful or practically normal person, and this method worked well. In practically all the cases iodine or calcium treatment was given at the same time. He (the speaker) had given sunlight treatment to private patients, without so much benefit as to the mental cases shown at hospital.

[1] *Nature*, 1928, cxxii, 962.

He gathered, from Dr. Pickworth's communication, that one type of case which did not benefit from the treatment was that in which there was a *B. proteus* infection. He (the speaker) had had one case of proteus infection, and sunlight treatment had been beneficial in that case, so that the contra-indication did not apply in every case. Perhaps a good result would not follow unless the proteus infection had been to some extent dealt with first.

With regard to treatment by natural sunlight and vitaglass, he saw, a few days ago, an experiment in Stoke Park Colony for mental defectives, under the supervision of Dr. Branthwaite. It was on a group of children in the artificial sunlight room, and Dr. Branthwaite said that there had been definite improvement in the intelligence of these mental defectives. In that institution the vitaglass was chiefly in the roof, and perhaps that fact explained the good results. At Stoke Park again, they had the advantage of having a group of children of about the same age undergoing the treatment; they played games together, and had a good time. It was Dr. Branthwaite's intention, shortly, to record the mental age of the children; he expected that the record would show an improvement.

Dr. A. EIDINOW said that from many interesting statements in the discussion one would conclude that for "general treatment" by light therapy the carbon arc or a lamp giving long waves of irradiation was preferable, the short radiations emitted from the mercury vapour lamp not being beneficial. He asked for consideration of the nature and uses of the mercury vapour lamp. This lamp emitted very high intensity of short waves, producing a rapid erythema of the skin. The best use of such lamps was for treatment of acute and subacute disease; they were employed when a speedy form of treatment was necessary, with a rapid change in the blood. For the treatment of acute and subacute diseases, a lamp giving out much heat and light and radiant energy would produce harmful results. He had performed experiments on animals, trying to inoculate them with pathogenic organisms, such as pneumococci, streptococci and staphylococci; and when an intensive dose of visible light was given to those animals they were killed sooner than the controls. This was not a question of over-heating; there was some property in the radiant energy, not yet known, which was detrimental.

In the treatment of mental disease the problem was different; one was not then dealing with an acute illness, but with a chronic ailment. He endorsed the opinion that for these a longer wave radiation was required, also visible heat. The best means of giving that was by a powerful tungsten filament lamp. At present he was using a tungsten lamp with a capacity of 3,000 watts. This gave a bright, pleasant, warm light, and practically no erythema resulted from its rays. Was the production of erythema desired in the treatment of mental disease by radiation? This point required further investigation. Personally, he did not see any real indication for ultra-violet ray therapy in mental cases, and he hoped the issue would not be confused. The basis of the technique of treatment with short ultra-violet rays was essentially different from treatment with the longer radiations.

Sir ROBERT ARMSTRONG-JONES stated that the truth usually stood between two extremes. They had heard Dr. Cormac's unqualified praise of ultra-violet ray therapy, supported by that of Dr. Hancock, while Dr. Rudolf, with strong logic and considerable experience on his side, stood about midway between the former observers and the scientific laboratory contribution of Dr. Bourdillon who was in the strongest sense sceptical. Sir Henry Gauvain supported and reaffirmed the value of light therapy in cases of surgical tuberculosis, septic states and rickets. The facts recorded showed that mercury vapour, tungsten, and arc lamps were uncertain in their amount of ultra-violet light output. The dose also was a matter of uncertainty, and the Ministry of Health was unwilling to make grants so long as the whole subject of heliotherapy was undecided. None of the observers had referred to the special dangers which others had seen in the rekindling and renewed activities of old pulmonary foci. There had been deplorable results—as well as successful ones—from the use of ultra-violet light, which was undesirable in nursing homes unless there was medical direction as to dosage, duration of treatment, and selection of patients. Its application by nurses in the treatment of chilblains was, however, justifiable.

Dr. ELIZABETH C. MUDIE said one of the speakers had remarked that the application of ultra-violet radiation often had disagreeable effects upon the operator and assistants—a feeling of malaise, with giddiness and headache being frequently experienced. In her own experience, even on the busiest day when her lamps were constantly in action, neither herself nor any of her nurses had ever known the very least inconvenience to follow

(except an attack of conjunctivitis from inattention to goggles). She suggested that the malaise mentioned might be due to the carbon-arc lamp alone, and attributable to the abnormal percentage of carbon dioxide in the atmosphere. In her own practice she used only quartz mercury vapour lamps and the air rarely became vitiated, even in a fairly small room.

Sir MAURICE CRAIG (President) said that on hearing Dr. Dove Cormac's paper, he felt that there were definite types of mental disorder which more readily responded to this form of treatment, and that depressed phases were lessened, more especially the depression in manic-depressive disorder. Reference had also been made to the melancholia which was associated with restlessness and agitation, a condition in which the blood-pressure was low, but on the question of blood-pressure there seemed to be a difference of opinion.

What was most needed at present was a definite pronouncement as to the type of patients for which these rays were beneficial. He had been glad to hear Sir Robert Armstrong-Jones mention tuberculous cases, because two years ago he (the speaker) had suggested—in his ignorance—that a certain patient should be treated with ultra-violet rays, and the administrator wrote asking whether he (Sir Maurice) was aware that the patient had a tuberculous history, as he was afraid that the rays would light up lesions which then were quiescent.

The work of Dr. Bourdillon, based as it was on research, was most valuable, and he had indicated a question which, no doubt, would be carefully taken up, namely, as to whether the benefit came from the ultra-violet rays or from some other ray of shorter length.

Dr. DOVE CORMAC (in reply) said that Sir Henry Gauvain had pointed out the need for interruption of the treatment, as, if unduly prolonged, it eventuated in an irritability. It was known that the effect on the nerves was to increase excitability, followed later by depression. In the irradiation associated with ergosterol and vitamin B, there was an optimum exposure, and if the rays were administered for too long a time, the vitamin was killed. An Italian worker had noted that the effect of the rays on isolated muscle was first to increase and later to diminish its tone. In the case of the patients at his own institution it was found that their weight tended to increase in the early period of the treatment, but that later there was a loss of weight, and it was when such loss was continued that the treatment should be suspended. Further, if the rays were continued too long, bactericidal power was reduced.

He himself had not noted in patients or staff any symptoms such as giddiness and malaise to which Dr. Hancock had referred.

In the case of proteus infection, quoted by Dr. Pickworth, he thought the intestinal flora probably acted as sensitizors.

In reply to Dr. Potts, he agreed that the herd instinct was a help; it prevented patients from becoming scared.

In early days he had attempted to treat tuberculous cases with the rays, continuing the treatment for several months, and they had appeared to benefit. Certainly in no case was there any increase in the tuberculous mischief during or following the radiation treatment.

With regard to the type of cases chosen, no cases of extreme melancholia were improved; only the mild types, having merely lethargy and slight depression, seemed to benefit. One girl was in a state of terror as to what would happen, and after the first séance she appeared to be definitely worse. After a few days, however, her condition settled down, and in two months she had put on 23 lb. in weight, and had recovered sufficiently to be discharged. It must not be considered that every case would benefit from the treatment. When he began, he put almost every case under the treatment, but now he found that not more than twenty or thirty cases out of 200 admissions were likely to derive benefit from it.

With regard to the intelligence tests which Sir Henry Gauvain carried out on the children in his institution, Sir Henry had suggested that there was some chemical change in the grey matter, which resulted, after the rays were applied, in an increase in the mental output. Most people seemed to be under the impression that the mental output depended on the grey matter. It depended also on the conductivity of the nerves. The state of resistance at the synapses was probably of far greater importance than the condition of the grey matter, and was the more likely cause of mental retardation or acceleration, unless, of course, there was present some definite lesion of the grey matter.

Most of the observations showed that when high blood-pressure existed, it was reduced by the rays, but when the blood-pressure was low, the treatment made no difference. When the condition was made worse in some cases by the irradiation, he attributed the fact to the action on the sensory nerves rather than to the effect on the blood-pressure.

Section of Urology.

[*January* 24, 1929.]

Traumatic Rupture of the Urethra.

By Sir WILLIAM I. DE C. WHEELER (Dublin).

THIS communication is based on an experience of six cases of complete rupture of the urethra in the period 1921 to 1928. The injury is comparatively rare. Kidd [1], states that forty-one fresh cases of ruptured urethra were admitted to the wards of the London Hospital in the ten years 1909 to 1919. He concludes from this that only four cases of ruptured urethra are likely to be admitted in any one year, and that these four cases will be divided amongst twelve surgeons.

Bailey [2] indicates that twenty cases were admitted to the Liverpool Royal Infirmary during the past decade. The lack of individual opportunity to study a large series of cases may, to some extent, account for the expression in the literature, of many diverse views, especially on the question of treatment.

Until comparatively recently I believed that rupture of the urethra following external violence occurred most frequently in the membranous portion, and that an intractable stricture almost invariably followed. We now know quite definitely that rupture is far more common in the bulbous portion of the urethra, well in front of the triangular ligament, and furthermore, that if the membranous segment is severed, a tight stricture in the process of healing is the exception and not the rule. Marrion (quoted by Bailey) states that the membranous urethra, unlike the bulbous, shows very little tendency to stricture formation. It is frequently stated, especially by French authorities, that the accident occurs in young male adults, usually boys. I do not think this is so in all countries; the average age in my own six cases was 42, and I have estimated that in the fifty-seven cases collected by Kidd to record end-results, the average age was approximately 33. Then again [3], reference to the direction taken by extravasated blood and urine leads the uninitiated to suppose that this is determined with mathematical accuracy by anatomical considerations. It is always interesting to try and balance theory with practice and to ascertain if academic teaching is in harmony with known matters of fact. But although the attachments of Colles' fascia are anatomically attractive, anatomical barriers are apt to be swept away by the oncoming tide of blood and urine, helped in its depredations by subsequent infection. It is not uncommon to find extravasation irregularly invading areas far beyond the theoretical limit. The anal region, the ischio-rectal fossæ and the thighs are quite often the site of extensive ecchymosis. Anatomical limits are however more commonly overrun in cases of non-traumatic leakage behind old infected strictures.

In the first case in the series under review, the urethra was torn off near the neck of the bladder above the triangular ligament, and although the expected extravasation into the space of Retzius and under the pelvic fascia, was actually present, yet before operation the outstanding feature was the spread of urine and blood into the perinæum, scrotum and penis, and on to the front of the abdomen, in the manner so characteristic of rupture in the region of the bulb. It is often stated that with intrapelvic rupture, there is no perineal hæmatoma, but it must not be forgotten that when the pelvis is fractured there is widespread extravasation of blood into the perineum and elsewhere.

Diagnosis and Prognosis.—When the rupture of the urethra is in the common situation well in front of the triangular ligament, there is almost invariably a perineal hæmatoma (absent in four of twenty-nine cases mentioned by Kidd), blood trickles from the meatus immediately after the injury (absent in a few recorded cases), and there is an intense but ineffectual desire to pass water. Local pain becomes severe. Extravasation of urine is prevented by reflex spasm of the compressor urethræ muscle until many hours have elapsed, and it is thus an infrequent complicat on

This beneficent and enduring spasm is Nature's "first aid to the injured," but for which the mortality resulting from early extravasation of urine and infection would be materially increased.

The amount of bleeding from the meatus after the accident is often in inverse proportion to the amount of damage done to the urethra [4]. After partial division, hæmorrhage is likely to be more profuse and continuous; when the division is complete the retraction of the part favours hæmostasis, the line of least resistance for the escape of blood is into the cellular tissues. In this way the gravest rupture is accompanied by the largest hæmatoma. When large hæmatomata are found without rupture they indicate severe damage to the corpus spongiosum without involvement of the mucous membrane. Severe traumatic stricture may follow such injury [5].

The diagnosis of ruptured urethra presents no real difficulty, but the exact position of the rupture may not be so easily ascertained. If the pelvis is broken, the rupture is more likely to take place above the triangular ligament in the region of the apex of the prostate. It may be in any position between the bladder and the bulb. The possibility of ruptured bladder above or below the reflection of the peritoneum must be remembered. A ruptured bladder is empty and can never be palpated. A catheter passes freely through the urethra, and only small quantities of blood-stained urine return. Absolute reliance cannot be placed on this test, as is illustrated by the following case [6]: The patient fell from the roof of a house fracturing his jaw and both forearms, and so far as it was possible to ascertain, he had sustained some fracture in the region of the pubes. He had abdominal tenderness and made ineffectual attempts to pass water. Ruptured bladder was diagnosed, but on passing a catheter a full quantity of blood-stained urine was withdrawn. The inability to urinate continued and a catheter was passed every four hours. Blood was present only on the first occasion. The urine was withdrawn at every catheterization in moderate quantity. The abdomen became more rigid in a few hours and early signs of peritonitis were evident. At operation, twenty-four hours after admission, a large rent was found in the fundus of the bladder and the abdomen was full of urine and peritoneal exudation. A catheter was passed while the abdomen was still open, and it then became obvious that on each occasion the catheter had gone through the rent in the bladder and the fluid had been drawn from the peritoneal cavity. The patient did well.

Bailey draws attention to Sir Benjamin Brodie's observation that a black patch on the glans penis is a sign of fatal omen. It probably indicates that the whole of the corpus spongiosum is infiltrated with urine. When extravasation occurs in traumatic cases it is a consolation to know that there is not the pre-existing infection which is found when rupture and extravasation take place behind an old-standing stricture. There is not therefore the same fear of early rapidly spreading infective gangrene and death from septicæmia and uræmia [16]. The subjects of traumatic rupture of the urethra are generally the reverse of debilitated. The infiltration of the tissues, however, with a fluid containing urinary salts and urethral bacteria is inevitably followed by inflammatory reaction [7]. If operation is delayed and the patient escapes death, the parts become riddled with sinuses and urinary fistulæ, presenting a surgical problem of the gravest kind. In cases complicated by severe fracture of the pelvis, although the main issue is the rupture of the urethra, the fracture should be treated along orthodox lines [8].

Treatment and Results.—It would be a great advantage to those confronted with an early case of traumatic rupture of the urethra, if some uniform method of treatment, applicable to all early cases, was generally accepted.

So far, published results are not helpful in attaining this end. There is a monotonous uniformity in end results, good, bad, and indifferent, no matter what procedure is followed in a given series of cases.

At operation the extent of the injury will be found more than was anticipated from the signs and symptoms immediately after the accident, a point in favour of operation in every case.

In the six cases under review, the line of treatment was the same in all but the first. One patient (Case IV) died ten days after operation of post-operative pneumonia. The respirations ranged from 40 to 48, and the pulse from 120 to 140. There was no post-mortem examination. In this case the patient was aged 27; the operation was performed early, there was no fractured pelvis or other complication. There may have been an alcoholic history, but at all events it was difficult to connect the death directly with the local condition or the actual operation. The rupture was in the region of the bulb.

In the remaining five cases the patients report themselves cured, after periods ranging from seven years in the first case, to five months in the last. They are symptomatically well. By "symptomatically well" is meant that the patients are not conscious of anything being wrong. There is no frequency of or difficulty in micturition. The flow appears strong and normal. All are in robust general health.

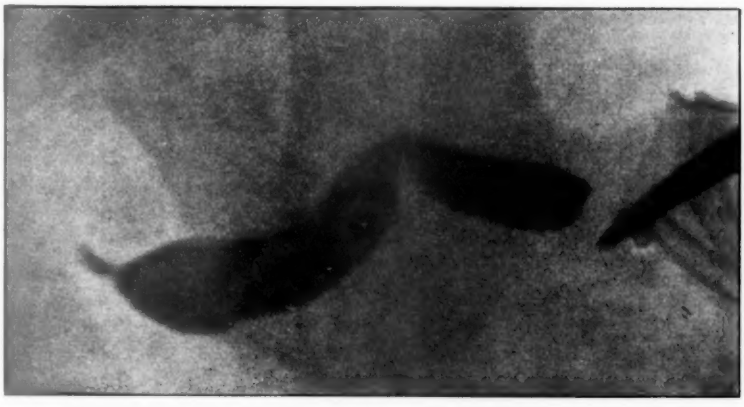

FIG. A. Case III.

Radiogram three years after rupture of the urethra. On the left the narrowing at the site of rupture in the bulbous portion is seen. On the right an artery forceps is holding rubber tubing round the glans. The patient is symptomatically well.

In the two cases with extravasation of urine (I and V), no attempt was made to deal with the rupture at the time of the accident. In the first case the secondary operation for repair of the urethra was performed ten days after the accident; in the second case an interval of two months was allowed to elapse. The first patient met with the accident seven years ago, the other, one year ago. There is no doubt that a urethroscopic examination or an examination with sounds would reveal the presence of some stricture formation in these cases, but they are reported completely cured.

In Case III, the patient, aged 60, presented himself for examination previous to the writing of this communication. He refused to have an instrument passed, but a skiagram, after the injection of lipiodol, showed definite narrowing of the urethra in the region of the bulb (fig. A). Symptomatically he is well.

In the most recent case (VI), the patient, aged about 70, operated on five

months ago, is at present under treatment. Definite narrowing at the point of rupture is shown by skiagrams, but full-sized sounds can be passed without any difficulty. I am indebted to Sir John Thomson-Walker for taking charge of this patient, and for letting me see the photographs and recent reports.

In the remaining case (Case VIII), the patient is in India. He also believes himself cured and is a healthy member of the community.

Cases in which there is no narrowing or fibrosis when examined with the urethroscope or sound, are few and far between, but I venture to say that when there is a complete symptomatic cure after twelve months' observation, there need be little fear of damage to the kidneys from back pressure, and treatment by dilatation in traumatic cases may be safely discontinued [9].

In twenty-five cases followed by Kidd, results seemed unsatisfactory for the first year or so, but in the end turned out extremely good.

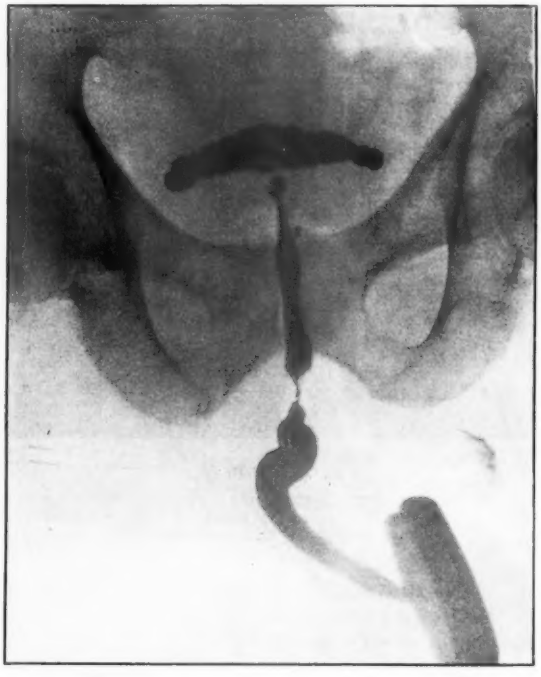

FIG. B.—Skiagram of recent case after operation showing apparent narrowing. Large instruments can be passed without pain or difficulty.

These results, although not ideal, encourage me to report the line of treatment adopted in each case. In two respects, viz., the employment of the indwelling catheter and the closure of the perineal wound, the operation recommended does not find favour in the writings of many of those whose opinions deserve the greatest respect.

Operation.—The operation must not be undertaken hurriedly; the result depends largely on complete hæmostasis and asepsis. A hypodermic injection of morphia is given, and if necessary, the distended bladder is emptied by suprapubic puncture before the patient is removed to hospital. The suprapubic and perineal regions are carefully shaved and prepared. The anterior urethra is washed out, by means of a

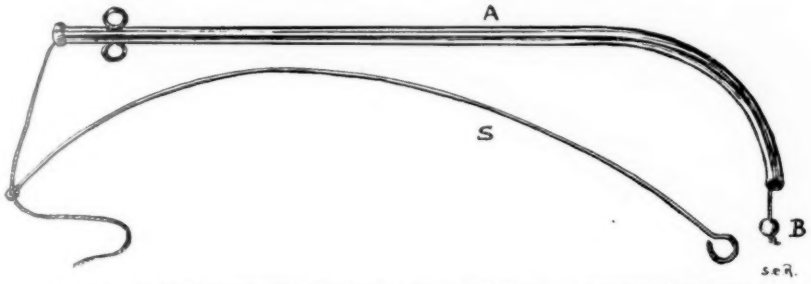

FIG. 1.—Author's instrument for retrograde catheterization. A small steel ball threaded through the catheter forms the blunt end. When exposed in the perineum the ball is cut off and the thread is attached to a rubber catheter passed from the meatus.

syringe, with mercurochrome or other suitable antiseptic. No attempt is made to pass a catheter at this stage if the signs and symptoms of the injury are sufficiently well marked to make the diagnosis clear. Instrumentation before operation is only justifiable if the diagnosis is in doubt. Gas-oxygen or spinal anæsthesia should be employed, preferably the former. The patient is placed on the table with the

FIG. C.—Position of patient and assistants after suprapubic cystotomy when the perineal incision is made.

buttocks over the edge ready for the lithotomy position and perineal section. The bladder is opened above the pubis.

With the finger as a guide, a catheter, or the instrument specially designed[1] for the purpose, is passed in retrograde fashion [10]. The instrument is held in

[1] The instrument is an adaptation of an aneurysm needle designed by Professor A. K. Henry, of Cairo. If a catheter is used there must be two eyes approximately opposite one another, through which the threaded needle can pass.

position during the subsequent procedures by an assistant. The legs are flexed on the abdomen by two nurses or assistants. Crutches are not used. A long median incision is made in the perineum. With the aid of a good head lamp all blood clots are gently cleared away; torn muscle and crushed tissues are excised by sharp dissection and bleeding is controlled. With a little patience the area

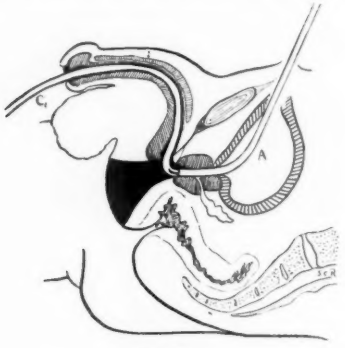

FIG. 2.

FIG. 2*a*.

FIG. 2.—The suprapubic and penile catheters are exposed in the perineum.
FIG. 2*a*.—Another view. The threaded ball is about to be cut off.

of operation can be rendered quite dry. The assistant in charge of the suprapubic wound gently insinuates the nose of the metal instrument through the proximal end of the rupture, which is thus secured without search or delay. A rubber catheter is then passed by way of the meatus into the perineal wound. It is secured to the thread passing through the metal instrument and pulled through the ruptured

FIG. 3.

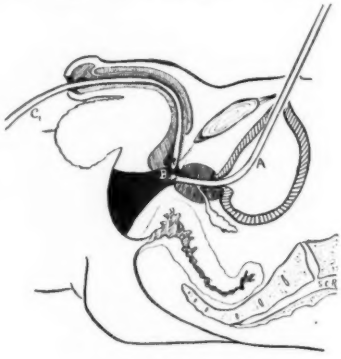

FIG. 4.

FIG. 3.—Attaching the thread to the rubber catheter.
FIG. 4.—By pulling on the thread the nose of the rubber catheter enters the metal instrument.

ends by withdrawal of the instrument from the bladder. The long thread attached to the rubber catheter is passed through a medium-sized rubber drainage tube inserted into the bladder above the pubis. The thread is fixed to the suprapubic tube or to a skin suture. In this way the catheter is anchored. The torn ends

of the ruptured urethra, through which the rubber catheter passes, are trimmed and defined and joined together end-to-end by interrupted fine catgut sutures; four or five are sufficient. If it is possible, these sutures should only penetrate the

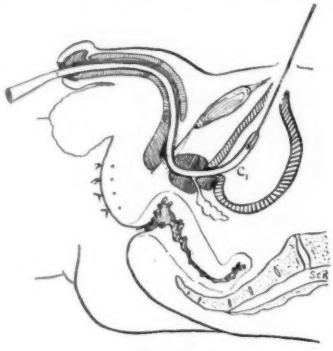

FIG. 5.

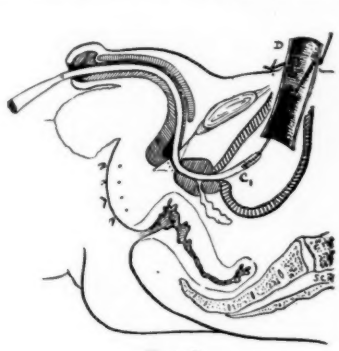

FIG. 6.

FIG. 5.—The metal instrument is removed. The rubber catheter passed through the rupture into the bladder.

FIG. 6.—Indwelling catheter anchored by thread.

outer walls of the urethra. One or two sutures of heavier catgut may be employed to take a bite of the tissues surrounding the urethra, to relieve tension, but when the legs are lowered tension disappears and these additional sutures are not always

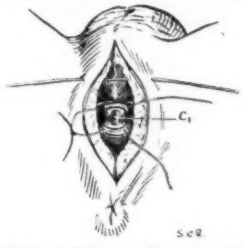

FIG. 6*a*.—Suture of the urethra over the catheter.

needed. The perineal wound before closure should be clear (as far as is feasible) of crushed or injured tissues, and should be quite dry. Bipp is smeared deeply but lightly into the recesses and over all the exposed area. The line of anastomosis is

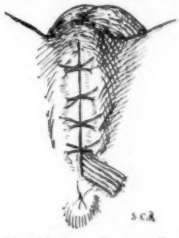

FIG. 6*b*.—Perineal wound closed.

reinforced by suturing some of the deep tissues circumferentially. Two or three further deep sutures are used to obliterate the dead space between the urethra and the skin, and finally the latter is closed by interrupted sutures. A small superficial drain

of rubber tissue is introduced at the posterior angle. Finally, the wound, with the surrounding skin and anus, is smeared over with bipp. The end of the bed is raised to assist the suprapubic drainage [11]. Each day after operation the bladder is gently irrigated by means of a syringe through the retained catheter [9 and 12]. The catheter is changed every fourth day, either by stitching a fresh one to the meatal end of the

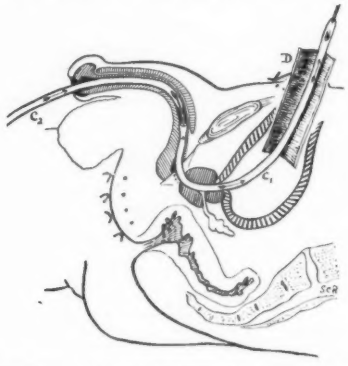

FIG. 7.—Change of catheter. A fresh instrument is stitched to the one *in situ* at the meatal end, the suprapubic thread is pulled upon. In this way one catheter is replaced by another.

one in use and pulling on the thread which comes through the suprapubic wound, or alternately it may be fixed to the suprapubic thread and pulled through the bladder into the urethra by distal traction. The introduction as well as the subsequent retention of the catheter, is rendered very simple by means of the guiding thread. At the end of a fortnight the retained catheter and suprapubic drain are discarded,

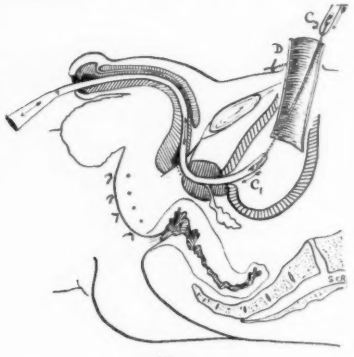

FIG.

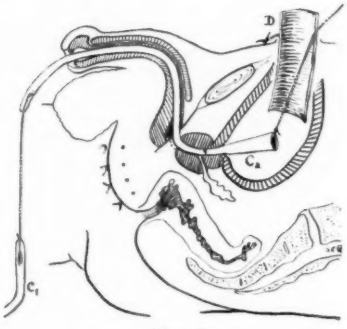

FIG. 9

FIG. 8.—Retrograde change of catheter by attaching the fresh instrument to the suprapubic thread and pulling on the meatal end of the one *in situ*.

FIG. 9.—Retrograde catheter in position.

and no instrument is passed until healing of the perineal and suprapubic wounds is complete. Sometimes there is a little leakage or weeping from the perineal wound, but usually both wounds are quite healed and the patient is passing water normally within six weeks.

A moderate-sized bougie is then passed more with the object of ascertaining the

condition of the urethra than with a view to dilatation. The patient should be seen every month for a year to make certain that no progressive stricture is in process of formation [9].

With the exception of the first case, in which I passed instruments for two years at fairly regular intervals, I was unable to carry out regularly either urethroscopic examinations or dilatations; the patients were so well that they would not submit to the ordeal.

When the rupture of the urethra is intrapelvic, the pubo-prostatic ligaments are torn and the bladder becomes displaced backwards. There is no alternative to the retained catheter when dealing with this form of complete intrapelvic rupture. It is the only means of correcting backward displacement of the bladder, and it is by general consent the standard method of dealing with this class of case [2].

In the two cases (Nos. I and V), the extravasation of urine was dealt with by making adequate incisions and by immediate suprapubic cystotomy. The incisions were longer and deeper than usually employed. The subsequent repair of the urethra was accomplished in much the same way as in recent rupture.

In a search through the recent literature, I find that there is a general consensus of opinion in favour of immediate suprapubic cystotomy in fresh cases of ruptured urethra. The reasons seem obvious; the proximal end of the urethra, so notoriously difficult to find [2, 17], can be made evident by retrograde catheterization. Some writers record that they spent from one and a half hours to two and a half hours searching for the elusive opening. In many instances search was futile. Others [13], however, have never met a case in which it was necessary to open the bladder for retrograde catheterization. The diversion of the urinary stream from the site of injury is of such paramount importance that this consideration alone justifies suprapubic drainage.

Condemnation of the indwelling catheter appears to be based on the assumption that it is one cause of post-operative stricture. It is contended that a low-grade infection is introduced by the retention of a foreign body in the neighbourhood of the wound. Hence the riotous formation of fibrous tissue. Yet it must be remembered that in traumatic stricture, as distinct from gonorrhœal stricture, the narrowing is extrinsic as well as intrinsic; there is fibrosis of the deep tissues of the perineum surrounding the urethra in addition to the irregular healing of the divided ends. If the perineal wound—in the absence of an indwelling catheter—is allowed to remain open and heal by granulation, the amount of scar tissue must be greatly increased.

It has been urged that when a catheter is left *in situ*, the sphincter relaxes, and urine trickles by the side of the catheter into the wound, but this objection does not hold good when suprapubic cystotomy is employed. We know, too, what excellent results follow suture of the hepatic and common bile-ducts and the descending colon over a rubber tube which is left *in situ*.

By employing the retention catheter for about a fortnight and avoiding perineal drainage, the following advantages are claimed: (1) The patient is much more comfortable and nursing is simplified; (2) scar formation in the perineum is reduced to a minimum; (3) subsequent instrumentation is simplified; (4) convalescence is hastened.

Amongst the operations which find most favour are [2]:—

(1) The sutureless operation, i.e., suprapubic cystotomy followed by perineal section. A catheter is introduced from the meatus to the bladder and the perineal wound left widely open. The ends of the divided urethra come into contact when the legs are extended. No indwelling catheter.

(2) Suture of the roof only [14, 15], combined with suprapubic cystotomy. The perineal wound is left open. No indwelling catheter.

(3) Both ends of the divided urethra are sutured to the skin of the perineum. Four to six months later, an ingenious plastic operation is undertaken.

(4) Mobilization of the corpus spongiosum, end-to-end anastomosis over a large catheter which is removed immediately after the operation.

(5) Suture of roof and sides, catheter passed through an incision in the urethra behind the rupture for perineal drainage [13].

(6) Suprapubic cystotomy [18]. Retrograde catheterization. No perineal incisions. Metal catheter passed through the external meatus along the urethra until it comes in contact with the first. The beaks lie in contact. In this way the second catheter is guided into the bladder. A self-retaining rubber catheter is attached to the latter in the suprapubic wound by a silk thread. The metal catheter is withdrawn and the self-retaining catheter is drawn into place past the rupture. The opening in the bladder is closed. Self-retaining catheter is removed in ten days.

Mr. Kidd, in the discussion before this Section, held in March, 1928, mentioned, *en passant*, that he recommended thyroid extract in one-grain daily doses in cases of stricture of the urethra. He found, as others did in cases of Dupuytren's contracture of the hand, that this preparation softens the fibrous tissues of a stricture and renders it more amenable to dilatation.[1] There is, I think, an additional reason for the administration of thyroid extract in these cases.

A mild attenuated form of infection, which does not necessarily lead to frank suppuration, is always present in injuries of the kind under discussion. It is the form of infection which precedes the tragic occurrence of sudden death from pulmonary embolus after prostatectomy and other pelvic operations. The veins in the neighbourhood of urethral rupture empty by tributaries into the internal iliac, from which an infected clot may easily become detached.

Case III of my series was nearly lost from pulmonary infarct.

Walters, of the Mayo Clinic, has recently reminded us that in order to combat the depression of metabolism after operation, two-grain doses of thyroid extract thrice daily are indicated. In 2,000 cases in which thyroid was administered and the patient allowed to move freely about in bed, no pulmonary embolus occurred. Thrombosis and embolism never occur in cases of hyperfunctioning thyroids, and there is experimental evidence to show that these complications are prevented by raising the metabolic rate.

The following is a brief résumé of six consecutive cases of complete rupture of the urethra:—

Case I.—Patient, aged 28. Accident September 8, 1921. Horse fell backwards on the rider. Double vertical fracture of the pelvis; complete rupture of the posterior urethra. Extravasation of urine into the space of Retzius and in front of the triangular ligament; perineum, scrotum and abdominal wall involved. Primary operation, free incisions and suprapubic drainage; secondary operation ten days later, perineal repair of the urethra; perineal drainage; no indwelling catheter. Sounds passed at regular intervals until April, 1923. Difficulty in passing instruments. Has marked stricture. Several perineal abscesses opened and drained.

Result.—Two years later (August, 1925), he writes: "The last abscess was opened in November, 1923. Since then I have had nothing done and have been very well indeed." The left hip-joint became infected in this case and complete bony ankylosis resulted. In November, 1928, he writes: "I have had nothing done to me since October, 1923, and have been very well since. I am unable to get on a horse, but play a lot of golf. It is not very convenient for me to go to Dublin at present and personally I do not see the necessity."

Case II.—Patient aged about 25. Accident May 1, 1924. Horse rolled on top of him while hunting. X-ray showed a stellate fracture of the pubes. Complete rupture in the region of the bulb. No extravasation of urine. Suprapubic cystotomy; retrograde catheterization; indwelling catheter. Closure of perineal wound. Discharged from hospital completely healed in six weeks. One month later he was examined under an anæsthetic; full-sized sounds passed without difficulty.

[1] *Proceedings*, 1928, xxi (Sect. Urol.), p. 40.

Result.—On a few occasions afterwards, instruments were passed before he rejoined his regiment. This patient is in India. His local doctor writes that he communicated with his uncle, November 7, 1928, nearly five years after the accident: "He seems to have had no trouble with his bladder or urethra since you last saw him; plays all outdoor games, polo included, and in his letter to a friend last week says 'he never was better.'"

Case III.—Patient, aged about 60. Accident May 12, 1925. Fell across a girder when inspecting the roof of a house. Operation same as in Cases I and II. Rupture in the region of the bulb. Convalescence retarded by pulmonary infarct. Patient very nervous and resented the passage of instruments. Three and a half months after operation two small sounds passed with difficulty (March 8, 1926). Sounds again passed two months later (May 19, 1926).

Result.—This patient was examined November, 1928, approximately three years after the accident. He was symptomatically cured. The flow of urine was strong. He refused to have an instrument passed for examination. X-ray photographs with lipiodol show narrowing at the site of the old rupture.

Case IV.—Patient, aged 27. Rupture near the bulb. No complications. Died of septic pneumonia. (*See* p. 33.)

Case V.—Patient, aged 27. Fell astride the side of a truck. Accident February 29, 1928. Rupture in region of the bulb. Extravasation of urine. Suprapubic cystotomy; free incisions. Secondary operation two months later by orthodox method. Discharged from hospital one month after secondary operation; passing urine normally; full-sized bougie passed. Before the secondary operation there were transient signs of pyelitis on several occasions, coincident with premature and partial closure of suprapubic wound.

Result.—Local doctor writes (November, 1928): "The man is perfectly fit and complains of nothing; is in perfect health." Refused to come to hospital for examination.

Case VI.—Patient, aged about 70. Riding with long stirrups; jolt against pommel of saddle. Emergency operation of perineal drainage by assistant surgeon, August 13, 1928. Two days later, usual suprapubic drainage, perineal suture and indwelling catheter. In this case the catheter came out on the fifth day and was not replaced. Some slight weeping from perineal wound. Discharged from hospital two months after the accident.

Result.—Three months after the accident, Sir John Thomson-Walker reports: "The instruments passed with perfect ease up to 12/15 English, and would have gone higher if I had cared to push it. There is still a ridge where the rupture was, but the lumen is very free." X-ray before passage of instruments shows narrowing.

Conclusions.

1. Traumatic rupture of the urethra is a rare injury.
2. Rupture is most common in the bulbous portion; the membranous urethra usually escapes. A tight stricture following injury to the latter is the exception and not the rule.
3. The diagnosis usually presents no difficulty. Large hæmatomata in the perineum are indicative of complete rupture, but may occur in severe injury to the corpus spongiosum without rupture. Stricture frequently follows the latter.
4. The amount of bleeding from the meatus is in inverse proportion to the amount of injury to the urethra.
5. In cases of fractured pelvis the urethra is usually ruptured near the prostate.
6. Operation should be undertaken in all cases whether an instrument can be passed or not.
7. Suprapubic cystotomy and retrograde catheterization are indicated as a rule.

8. The urethra is sutured over an indwelling catheter and the perineal wound completely closed.

9. By an operative technique suggested, the catheter can be changed three or four times during the first fortnight without difficulty.

10. After ten or fourteen days the catheter should be removed and the suprapubic wound allowed to heal.

11. No instruments need be passed after the lapse of twelve months if the patient is symptomatically well.

12. The administration of thyroid extract appears to soften fibrous tissue, and is a prophylactic against pulmonary embolus.

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DISCUSSION.

Mr. A. RALPH THOMPSON said that in a long series of autopsies at Guy's Hospital, deaths due to venereal stricture numbered forty-eight, and traumatic strictures two.

A shorter series of autopsies at the London Hospital showed twenty-seven cases of death from venereal stricture and none of death from traumatic stricture. Thus about 3 per cent. of all cases dying from stricture were due to injury.

From the information compiled from the same series of cases, injury to the urethra may be expected in about 33 per cent. of cases of fracture of the pelvis. Out of thirteen cases of fractured pelvis the urethra was damaged in four. In his own experience, since 1910, when the Genito-Urinary Department at Guy's Hospital was first opened, there had been fifty cases in whom there was evidence of injury to the urethra.

He agreed with the lecturer that the average age was by no means so low as was usually stated; a series of cases had shown it to be thirty-four. The same series had shown that the average distance of the injury from the meatus urinarius was $4\frac{3}{4}$ in. These figures confirmed Sir William Wheeler's statements. He could not find that attempts at suture of the damaged urethra had been made at Guy's Hospital, unless in recent years, but it must be remembered that when the urethra was damaged in a case of fracture of the pelvis, the fracture became compound. A plea, however, might be put in for more radical treatment than had hitherto been adopted, and the lecturer had fulfilled a useful purpose by drawing attention to the need for some such suture of the urethra as had been described. He quite agreed with the suggestion as to suprapubic cystotomy and retrograde catheterization. The message which the Section of Urology should deliver to the profession might be summed up in Sir William Wheeler's own words, namely, that suprapubic cystotomy must be performed, and also suture of the urethra after retrograde catheterization through a perineal incision. He did not quite like the method described of removing bruised tissues that were evidently in the region of the damaged urethra, as he thought that an incomplete rupture of the urethra might thus be overlooked and valuable urethral tissue sacrificed.

Those who saw the results of traumatic stricture sometimes felt that perhaps more might be done as soon as possible after the injury than had hitherto been done. No doubt, however, the condition of the patient and the extent of his injuries had to be taken into account. Some few cases of traumatic stricture did well, but he (the speaker) had under his care at the present time several cases that were causing him great anxiety.

Mr. FRANK KIDD said he agreed that a description of the best methods in treating ruptured urethra was not always to be found in the text-books. The general surgeon was as

likely to meet with such cases as the urologist, so that it was essential that the writers of surgical text-books should take note of this discussion, and they would be the more inclined to do so as the discussion had arisen out of a paper read by such a well-known general surgeon as Sir William Wheeler.

With the help of Mr. Neligan he (the speaker) had written a report on fifty-seven cases of ruptured urethra admitted to the London Hospital as emergencies during a period of ten years. This had appeared in *Transactions of the International Society of Urology* for 1921, but as such publications were not widely read, the report had not received so much attention as it might have done had it been published in one of the regular medical journals. It had, however, brought out many points of interest in relation to the subject.

During a period of ten years, twelve surgeons had to divide fifty-seven cases of ruptured urethra amongst them, including cases of fractured pelvis. On the average any one surgeon could not expect to meet with more than four or five cases in ten years. It was this scarcity of cases which led to a lack of agreement as to how to deal with them in the most effective manner. The main result of his (the speaker's) report was to show that the one essential detail in all operations for ruptured urethra was to begin by opening the bladder so as to be able to make use of retrograde catheterization and to employ suprapubic drainage for the urine after the operation. Provided this was done, the exact devices used to deal with the rupture and the hæmatoma in the perineum seemed to matter very little. He hoped that the need for this preliminary step would at long last creep into the surgical text-books.

Summarizing briefly some of the main conclusions of the report, Mr. Kidd said that in twenty-five cases the final result was fully known. Twenty-four patients were alive, nineteen were practically cured, and sixteen no longer needed bougie treatment. Nineteen further cases were followed up for a time and were then lost sight of. Nine patients had no bougie treatment at all and considered themselves cured. Ten had bougies for a time and then ceased attending as they considered themselves cured. Seventeen out of the nineteen were practically cured. Out of forty-four cases uncomplicated by fractured pelvis, thirty-six (nearly 85 per cent.) could be looked upon as practical cures. This showed that the prognosis was infinitely better than was usually imagined. Despite prolonged bougie treatment, pyuria was present in only two of these cases, a fact in sharp contrast to its occurrence in cases of venereal stricture; and what was even more surprising, only four cases developed orchitis.

He (the speaker) had been able to inspect the urethra with the urethroscope in fifteen cases, and all but one showed the appearance of stricture at the site of the injury. All these strictures were well forward of the compressor urethræ, and lay in the middle of the bulbous urethra. This showed that the ordinary teaching as to the site of the rupture was incorrect.

As a result of his observations he had come to the definite conclusion that the cause of stricture formation was the amount of pulping of the corpus spongiosum at the time of the injury. As a result of pulping, so-called "auxetic" bodies were poured forth by the pulped tissues, which called for the overgrowth of fibroblasts, and resulted in the formation of extra-urethral fibrous tissue. This tissue, though often to be felt as a hard mass in the perineum, was frequently quite resilient to dilatation. If a skiagram was taken of the stricture after filling with silver iodide emulsion or lipiodol, an appearance as of severe stricture might be seen in the X-ray picture. Yet a few minutes later, quite a large bougie might be passed with ease and the stream of urine might be quite free. These observations showed how difficult it might be to appraise the exact amount of true narrowing of the canal that was present in these cases at a late date after the injury. There might often be a practical cure, even when a mass of fibrous tissue could be felt in the perineum, and when a stricture appeared to be present by urethroscopic and X-ray methods of examination. The fibrous tissue seen and felt might be quite resilient, might open out reasonably for the passage of urine and lead to no back pressure on the bladder or kidneys.

He has defined in his report the main necessities of the operation for ruptured urethra:—

(1) Preliminary suprapubic cystotomy under full anæsthesia should be deemed essential in every case. This was the most important lesson to be learnt.

(2) Attempts should be made to pass a catheter. If it slipped in with ease it might be justifiable to leave it as an indwelling catheter, and to do nothing more.

(3) In every case, however, it was probably wiser to make an incision into the perineum and urethra, either on to the catheter already passed or to a Wheelhouse staff, so as to give exit to the major portion of the extravasated blood and clot, with the idea of diminishing the production of "auxetic" bodies.

(4) A retrograde sound at once showed up the proximal end of the ruptured urethra, and the distal end was identified without difficulty by means of the staff or catheter passed from the meatus.

(5) It seemed to matter very little now as to what exactly was done *so far as end-results were concerned.* The quickest way was to fix a catheter into the bladder and leave the perineal wound to drain, without suturing the urethra at all. *Or* the roof of the urethra could be sutured and the wound left to drain. *Or* the whole urethra could be sutured round a catheter and the perineal wound be stitched up.

The catheter could be left *in situ*, or could be removed altogether. Whatever was done, the end-results appeared to be much the same.

The real essential was to drain the bladder for at least fourteen days, and finally not to pass large bougies too soon. At least three weeks should elapse before any attempt was made to pass a bougie. The urethra should be inspected by the aero-urethroscope before any bougie was passed. The first bougies should be of medium size and should not exceed $\frac{10}{12}$ on the English scale. Metal bougies were preferable to gum-elastic bougies.

The cases should be followed up for at least a year by means of bougies and the Kollmann's dilator, guided by observations made with the aero-urethroscope. This could best be done in a proper " stricture " department in a urological clinic, such as he (Mr. Kidd) had established at the London Hospital as early as in 1911.

If a traumatic stricture proved intractable, internal urethrotomy often helped matters. If this proved unavailing total excision of the stricture should be tried.

With careful and skilful treatment the prognosis was not so gloomy as had been pictured.

Mr. FOWLER WARD said that the end-results of rupture of the urethra depended to a large extent upon the age and weight of the patient and the stage at which he was seen. During the last ten years he had had about ten cases of traumatic stricture of the urethra. In small children the usual account of the accident was that they had been standing on a rail and had dropped down. In these cases the injury was not very severe; after the operation an indwelling catheter was passed and prompt healing resulted. With regard to after-treatment, he did not think his cases went elsewhere, as he was the only surgeon practising this work within a range of thirty miles of his home. The reason they did not come back was that there was an impact on the pubic arch, so that the injury was more or less in the anterior urethra, not the posterior. But he had had cases in which men who had been boating had fallen heavily on the boat, the resulting injury being very severe. There was no one at hand to look after such a case surgically, consequently the urologist did not see it until the third or fourth day after the accident, and then no great injury was apparent, though the patient had not passed water for a day or two. In such a case, however, it was found on operation that there was a severe injury of the urethra, two or three inches of which, with all the tissues in the vicinity, were sloughing. In such a case there was no chance of mobilizing the ends of the urethra and bringing them into union. What he had done in several cases was to pass a catheter through the penile urethra. If that was not possible, he performed suprapubic cystotomy, and then a retrograde catheterization. Sometimes that was not easy, and to find the opening he got a malleable Playfair's sound and turned it round almost into a circle. This he introduced into the wound, and it came out well underneath the pubic arch, where it was wanted, and so the opening was found at once. One must leave a sloughing wound to clean up with an open perineal wound.

Mr. Ralph Thompson had mentioned the important point of suture of soft tissues. In a thin man, if the wound was left open it would heal with the indwelling catheter *in situ*, but the skin of the perineum became more or less adherent to the urethra by scar tissue; and every time one began to dilate this urethra during after-treatment, one broke down the perineal wound and there was apparently a perineal fistula. He (the speaker) turned over a large flap in these cases, and it was necessary to put in a much longer piece of skin in the perineum than appeared necessary, as the skin shrank. That was turned in and the flap from the corresponding side was turned over it. That would heal up practically by first intention, and he had found such cases treated in this way do very well. On using the urethroscope three or four years afterwards he had found that the skin which had been turned in, being cutaneous skin, had apparently acted as a mucous membrane, with little or no trouble. Occasionally he had found hair growing in the urethra, but it did not cause trouble. When a case was seen at a later stage. and sloughing had taken place, the modus operandi must be different. When he had not had to do retrograde catheterization he had not opened the bladder;

but in future cases of rupture of the urethra of any magnitude which he might see he would do suprapubic drainage, as in doing that one felt safer, and the condition would heal up well.

He preferred a silver catheter, the double-bend form. This could be left in for a week or ten days without fear of sepsis. The ordinary rubber catheter occasionally became nasty, and he had known patients declare they tasted rubber when they had had a rubber catheter in the urethra. Silk-web also was liable to go wrong.

Mr. WINSBURY WHITE said he had at present under his care a case of fracture of the pelvis involving partial rupture of the urethra. The patient was a boy, aged 12, admitted to the East London Hospital for Children seven years ago after a "run-over" accident. The symphysis pubis was separated about an inch, and there was considerable extravasation of blood in the perinæum and external genitals, as well as in the upper part of the right thigh, but no sign of extravasation of urine. The boy was able to pass water freely, and no catheter was used. There was no record in the notes about any blood being found in the urine, but it was reported that albumin was found in the urine on several occasions subsequently. The fracture of the pelvis was treated by fixation in plaster, and several days later the patient began to have incontinence of urine.

He (the speaker) did not see the boy until five years after his accident, when urine was dripping from the external urinary meatus, and he was wearing a rubber urinal; he understood that this had been the condition of affairs ever since the accident. There was no stricture evident to the urethroscope in the anterior part of the urethra. He introduced the cysto-urethroscope into the bladder and noticed two obstructions in the posterior urethra; one in the fore-part and the other just before the instrument entered the bladder. He was able to identify these two points accurately by keeping a finger in the rectum. The cysto-urethroscope showed, in the floor of the prostatic urethra, a large mass of what appeared to be granulomatous tissue. Suspecting this to be the cause of the incontinence, he thought it was better to destroy the mass by diathermy. He did this, and the incontinence completely disappeared within a week. Subsequently, however, it definitely returned, and he thought, therefore, that the dilatation by the passage of an instrument was the real cause of the disappearance of the patient's symptoms. He continued to pass sounds every fortnight, and the boy now took No. 16 English and had no incontinence at all if no longer than a fortnight intervened between the use of the sound.

It was an interesting point to consider why injury to the prostatic urethra, which did not produce a marked stricture, should give rise to incontinence. The obstructions in the posterior urethra could scarcely be called strictures, as he was easily able to introduce a large cysto-urethroscope.

It was also of some interest to recall that there was a large hæmatoma in the perineum at the time of the accident, as he gathered from the notes; this bore out Sir William Wheeler's statement that in cases of fractured pelvis involving the prostatic urethra there was sometimes hæmatoma of the perineum.

Mr. CYRIL A. R. NITCH said that of late years he had had practically no experience of ruptured urethra, for the reason that the type of injury which occurred in London did not conduce to rupture of the urethra. In the country these cases were usually a sequel to a motor accident and were dealt with on the spot instead of being sent to London as heretofore. Before the war he had operated on a good many cases, but in the last ten years there had not been one in his wards, though there had been several with traumatic stricture following the injury. The extent of the damage was influenced by the nature of the accident. The man who fell astride, generally sustained a clean transverse division of the urethra without much bruising of the surrounding tissues, whereas when the rupture followed a motor accident, the injury both to the urethra and its surroundings was so extensive that the results both immediate and remote were bad.

In the last year he had had three children under his care suffering from the effects of a ruptured urethra, due, in each case, to a motor accident. Two were complicated by fracture of the pelvis and followed by extensive scarring of the bulbous and membranous urethra. In each of these cases repair of the urethra had been attempted shortly after the accident, and in each the injured urethra had been replaced by dense scar tissue extending from the bulb to the prostate, and the urine was escaping through several perineal fistulæ in one and through a suprapubic opening in the other. The only way of relieving such a distressing condition was by transplantation of the ureters, which had been successfully accomplished in both cases. In the third case, a boy aged 9 had been injured in August, 1928, and had

been operated on immediately by an expert surgeon, but even in this case there was now a stricture which only admitted a No. 6 French bougie.

He agreed that the best method of treatment was suprapubic drainage and suture of the urethra, and, like Mr. Ralph Thompson, he was averse to removing any more tissue than was absolutely necessary.

In view of the satisfactory results of Hamilton Russell's operation for excision of stricture, he did not favour complete annular suture but preferred to unite the urethra with a few dorsal sutures, insert an indwelling catheter and allow the other edges of the urethra to fall together with the tissues of the perineum.

Mr. HUGH LETT said that there were two types of stricture which followed traumatic rupture of the urethra. In one there was much fibrous tissue, and the stricture was very tough and difficult to dilate. In the other, although the stricture was evident on urethroscopy and X-ray examination, it offered little resistance to dilatation, and the stream was good.

In his opinion the dense fibrous stricture developed in those cases in which urine had escaped into the peri-urethral tissues.

Suprapubic cystotomy for the purposes of retrograde catheterization and drainage of the bladder ought to be generally recognized as an essential part of the operative treatment of traumatic rupture of the urethra.

Mr. G. E. NELIGAN said he strongly supported what the opener and others had said about suprapubic drainage of the bladder, suturing the urethra and tying in a catheter. He had had the good fortune to see cases of Mr. Kidd's when he was working the procedure out, and certainly those cases did much the best which had suprapubic drainage and the indwelling catheter.

He also agreed with Sir William Wheeler that sometimes an incomplete rupture bled much more than did a complete rupture. Two months ago he was called to see a man who had been walking on a scaffold and had fallen astride a beam. To get that injury the bulb must have been damaged, not the membranous portion. He had never seen so much bleeding as in this case; there was an enormous hæmatoma of the perineum and buttocks, and blood was pouring out of the urethra. Attempts to pass a catheter were futile. He (the speaker) gave the man an anæsthetic, and then found that a 12 to a 14 bougie passed easily. Afterwards, by some manipulation, he was able to introduce a fair-sized catheter. The bleeding went on for two days, then healed up, and the catheter was removed at the end of ten days. When using the urethroscope six weeks afterwards, one could see where the injury had been; there was a narrowing af the lumen, but no sign of trouble.

Last week he had had the case of an officer who, coming home from India, slipped when playing a deck game and sat astride something, tearing the urethra. It was not a complete tear, and he was able to pass urine. That was one of the tightest strictures through which he (the speaker) had tried to push anything. There was much scarring, yet there had never been much extravasation of blood or urine.

The other type of case was that of the run-over person who sustained a fractured pelvis, and there was rupture between the prostatic and the membranous urethra. There the difficulty was to get the ends of the urethra together. He had had three such cases, and the only thing to do was a suprapubic operation, putting the finger into the prostatic urethra, then passing a catheter through from the anterior urethra. One could manipulate the two ends together and slip a catheter into the bladder, and fix it in position. It did well in those cases.

Mr. J. SWIFT JOLY (President) said that almost all the speakers were agreed that in the first treatment of these cases, especially when there was total complete rupture, the whole urethra being torn into two, should be suprapubic drainage. Many years ago he had seen Professor Kocher operate in his clinic on one of these cases of complete total rupture of the urethra, and that surgeon had spent two hours looking for the proximal end of the urethra through the perineal incision. He did not carry out suprapubic drainage. Finally he was able to pass an instrument.

With regard to the age of these patients, in four of seven cases that he (the speaker) could remember at the moment, the patients were aged under 20, and from 15 to 20 seemed the commonest age for these accidents to occur. Such ruptures could follow a very trivial accident. He saw a middle-aged man who was walking along a footpath when his foot slipped through a circular coal-hole and he sustained a complete transverse rupture of the

urethra, in front of the bulb. He had seen another man who, while engaged in lighting lamps in railway carriages, and stepping from the platform on to the buffer of a carriage had slipped and come down astride on the buffer.

Another case had the longest history of traumatic stricture that he had ever known. In 1872 the patient had fallen astride a plank of wood. He had had the resultant stricture fifty years when he (the speaker) had seen him. No suprapubic operation had been done, and the stricture was very tight and tough. He was about 70 years old when he (Mr. Joly) saw him, and during those fifty years he had been under continuous treatment. He would not allow excision of the stricture, through which a filiform bougie could just be passed. He (the President) performed internal urethrotomy and so kept the patient going for four years, after which time he disappeared.

These cases of ruptured anterior urethra might be divided into two types: (1) Those in which a suprapubic operation had been done; (2) those in which it had not. The difference in the stricture in those two types was amazing. In the first class it was practically negligible. He had not radiographed these cases, but in all a 14 or 15 English size bougie could be passed. In the second there was a dense, hard, tight stricture, for which the sole treatment was excision. The only case which he himself had not so treated was that of the old man just mentioned, who had had the stricture for fifty years.

The posterior urethra presented a different proposition, and a very difficult one, especially when it was combined with fractured pelvis. First, one must do suprapubic cystotomy. The surgeon did not know what was wrong with the patient, and the suprapubic incision was partly curative, but chiefly exploratory. This had been forcibly impressed upon him by one of the first cases he had seen; it was in the post-mortem room of the General Hospital, Vienna. The patient had sustained a ruptured urethra and fractured pelvis a few days before. The catheter was not in the bladder, it had passed through the rent in the urethra and was in the space of Retzius. A suprapubic incision might have saved this patient's life.

Sir WILLIAM WHEELER (in reply) said that when he had operated on his first case in 1921 he had referred to the text-books and found them indecisive, and not helpful with regard to the details of treatment.

An operation for ruptured urethra should not be approached in an atmosphere of hurry. If the bladder was distended it was best to aspirate it in order to give plenty of time for adequate preparation. Asepsis was all important, and this was the type of case in which in the absence of great care it was likely to be lacking. He avoided crutches on the table, and thought it was much better to give one leg of the patient to one assistant and the other to another, so that at the psychological moment they could be lowered and the tension taken off the urethral ends.

With regard to the resection of the devitalized tissues, one or two speakers criticized what he had said in that respect. He did not remove these bruised tissues in any carnivorous sense, but trimmed them away by sharp dissection. This helped primary union.

He agreed entirely with what Mr. Thompson had said about deep sutures. His own practice was to close the perineal wound in four layers. The first united the ends of the urethra, the second joined together any available structures to strengthen the line of anastomosis, the third closed the dead space in the perineum, and the final series were inserted into the skin.

He had omitted to mention that when the rupture of the urethra was in the prostatic region in cases of fractured pelvis, there was usually a backward displacement of the bladder. In such cases there was no possible alternative but retrograde catheterization and the use of the indwelling catheter.

In conclusion, he ventured to think that the views expressed by those who had taken part in the discussion would do much to eliminate the "intellectual untidiness" in the literature which heretofore had handicapped those endeavouring to discover the best form of operation.

Section of Balneology and Climatology.

President—Dr. G. L. Kerr Pringle, M.C.

[*January* 10, 1929.]

The Excretion of the Constituent Ions of Sulphur Water in Fæces, Urine and Sweat.

By David Brown, M.D., B.Sc., and Arnold Woodmansey, M.Sc.

This inquiry was begun in 1923 and finished in 1927.

Circumstances have prevented an earlier account of our work. As is the case with many similar researches, the original aim of this inquiry has been overshadowed by the discovery of various phenomena of general importance which have come to light during our investigations.

Data will be placed before you which, in our opinion, prove considerable changes in general metabolism, evidenced by retention of certain ions of the strong sulphur water, or the plus excretion of these same ions forming part of the ordinary body tissues.

We shall also ask consideration of our results with regard to that much discussed, but little understood constituent of the sulphur water—barium—both as to the percentage excreted and to the particular avenue of excretion.

Conditions of Experiment.—Four experiments were made on three subjects (two aged about 40, one aged about 60). Experiments are denoted by initials, *A*, *B*, *C*, *D*. The subjects, in their usual condition of health, were placed on fixed mixed dietaries, the details of which were determined by the appetite and inclination of each subject.

Diet consisted of: Milk, bacon, beef or mutton, eggs, "force," bread, potatoes, carrots, boiled pudding (flour, suet, fruit and water in fixed proportions), marmalade, butter, water, tea and coffee. Each article of the diet was carefully weighed out each day.

In Experiment *A* the calorie value was 3,400 per diem. In Experiments *B*, *C*, *D*, the calorie values were from 2,800-3,000 per diem.

The period of each series of observations was 12 days (in *A* 9 days), preceded by a period of 6 days (in *A* 3 days) on the fixed and measured diet. The 12 days were divided into three periods of 4 days each:—

4 days on the fixed diet.
4 days on the fixed diet, with sulphur water in lieu of an equal quantity of drinking water.
4 days on the fixed diet (in *A* the periods were each of three days).

These periods are termed Basal or Pre-Sulphur, Sulphur and Post-Sulphur periods respectively. Fixed work, exercise, rest, and, as far as possible, sleep were observed.

Two subjects were laboratory workers, living for the most part in a temperature of from 11° to 15° Centigrade. The third subject was a bath attendant, and the experiment in his case was done during his vacation, and did not start till some ten days after the cessation of his ordinary employment. In short, all subjects were under as constant physiological conditions as are attainable.

The fæces, urine and sweat were quantitatively analysed for barium, manganese, calcium, sodium, potassium and magnesium. Phosphate was removed as ferric

phosphate in acetic acid solution. Manganese was estimated after oxidation to permanganate with sodium bismuthate. Potassium was obtained from a solution of the alkalies by precipitation as cobaltinitrite.

In addition, in one experiment (*D*) sulphur as sulphate was estimated in fæces, urine and sweat.

In *B* and *C* chlorine was determined in the urine. In *D* chlorine was measured in the sweat. In *C* the total nitrogen of the urine was estimated (Kjeldahl). In *C* the basal metabolism was determined by Dr. Holmes. In *B* and *C* the blood-pressure was observed by Dr. Edgecombe. In *C* the daily weight of the subject was observed.

The fæces were collected daily and ashed so that the analyses could be performed when convenient. In two cases the moisture content was estimated by drying for twenty-four hours on the water bath. The urine, to which a few c.c. of toluene were added to keep it sterile, was treated like the fæces. The process on this large scale was rendered less intolerable by the use of pure fuming nitric acid.

The sweat was collected by the following method. This procedure seemed to us entirely novel, as indeed we still believe it to be, so far as the collection of the total metallic ions of unconscious or normal sweat is concerned. A similar method for the collection of chlorides excreted by the skin during violent exercise (route marching), has been used in Germany with soldier subjects. In our experiments the utmost care was taken to avoid conscious sweating.

Details of Method of Collection of Ions of the Sweat.—(1) All towels and cotton underwear were twice boiled and repeatedly washed out with distilled water and dried out of contact before use. (2) Distilled water was used throughout for all washings and extractions. (3) At the beginning of each experiment the subject had a bath and was then sluiced down with distilled water and dried. (4) He than put on the prepared underwear, which he wore continuously for four days and nights (one experiment three days and nights). (5) At the end of four days the parts not covered by the underwear were washed, sluiced with distilled water, and dried. (6) Underwear removed and put by for extraction. (7) Subject scrubbed down with lint, sluiced with distilled water, and washings and lint put by for extraction. (8) Subject, after being dried, put on fresh underwear, and the processes were repeated for the second and the third stages of the experiment. (9) The underwear of each period was repeatedly extracted with boiling distilled water, to which a little acetic acid had been added. (10) The extractions were then added to the washings and lint, and the whole was ashed in porcelain dishes for analysis. The volume of the washings and extractions was 1½ gallons or more.

N.B.—The strictest precautions were taken to avoid contamination of the underwear by urine or fæces. Thus the excretion of total ions of roughly 80 per cent. of the body surface was obtained.

Results.

The experiments were called *A*, *B*, *C*, *D*.

Fæces.—The water and solid content of the fæces showed little difference in *B* throughout the experiment, but in *C* the increase in water content was over 100 per cent. during the sulphur-water period.

TABLE I

B.—Water—Daily Loss in Fæces.

Pre-sulphur period	...	3·5 oz.	Sulphur period	...	3·5 oz.

C.—Water Daily Loss in Fæces.

Pre-sulphur period	...	5·4 oz.	Sulphur period	..	10·6 oz.

TABLE II

Experiments B, C, D.

URINE (Experiment *B*).

	Day	Amount in c.c.	Sp. gr.	Weight in grm.	Solids in urine in grm.
Pre-sulphur or basal period	1	1,260	1020	1,285	25
	2	1,300	1018	1,323	23
	3	1,855	1013 +	1,888	33
	4	1,425	1022	1,456	31
	Totals	5,840 c.c.		5,952 grm.	112 grm
	Daily averages	1,460 c.c.		1,490 grm.	28 grm.
Sulphur period	5	1,425	1022	1,456	31
	6	1,470	1022	1,502	32
	7	1,580	1020	1,611	31
	8	1,780	1014 +	1,805	25
	Totals	6,255 c.c.		6,374 grm.	119 grm.
	Daily averages	1,564 c.c.		1,643 grm.	29·75 grm
Post-sulphur period	9	2,175	1014 +	2,206	31
	10	1,890	1014 +	1,918	28
	11	1,625	1015	1,649	24
	Totals	5,690 c.c.		5,773 grm.	83 grm.
	Daily averages	1,863 c.c.		1,924 grm.	28 grm

	Day	
Total salts excreted	1– 4 = 1·0 5– 8 = 1·04 9–12 = 1·0	Experiment with slight constipation.
Volume of urine	1– 4 = 1·0 5– 8 = 1·07 9–12 = 0·94	

URINE (Experiment *C*).

	Day	Amount in c.c.	Sp. gr.	Weight in grm.	Solids in urine in grm.
Pre-sulphur or basal period	1	1085	1020	1106	21
	2	1755	1014	1779	24
	3	2100	1010	2122	22
	4	1285	1018	1308	23
	Totals	6225 c.c.		6315 grm.	90 grm.
	Daily averages	1555 c.c.		1577 grm.	22·5 grm.
Sulphur period	5	1395	1020	1422	27
	6	1310	1022	1338	28
	7	1370	1026	1405	35
	8	1565	1022	1599	34
	Totals	5640 c.c.		5764 grm.	124 grm.
	Daily averages	1410 c.c.		1441 grm.	31 grm.
Post-sulphur period	9	2500	1008	2520	20
	10	1430	1014	1449	19
	11	1985	1012	2008	23
	12	1615	1014	1638	23
	Totals	7530 c.c.		7615 grm.	85 grm.
	Daily averages	1882 c.c.		1904 grm.	21 grm.

	Day	
Total salts excreted	1– 4 = 1·0 5– 8 = 1·33 9–12 = 0·93	Experiment with much purgation.
Volume of urine	1– 4 = 1·0 5– 8 = 0·95 9–12 = 1·2	

URINE (Experiment *D*).

	Day	Amount in c.c.	Sp. gr.	Weight in grm.	Solids in urine in grm.
Pre-sulphur or basal period	1	1950	1010	1969	19
	2	1820	1012	1841	21
	3	1945	1011	1966	21
	4	2345	1010	2368	23
	Totals	8060 c.c.		8144 grm.	84 grm.
	Daily averages	2015 c.c.	...	2036 grm.	21 grm.
Sulphur period	5	1460	1012	1477	17
	6	2400	1010	2424	24
	7	2390	1011	2416	26
	8	2495	1012	2524	29
	Totals	8745 c.c.		8841 grm.	96 grm.
	Daily averages	2186 c.c.	...	2210 grm.	24 grm.
Post-sulphur period	9	1945	1012	1959	14
	10	2145	1012	2166	21
	11	1555	1016	1578	23
	12	1995	1010	2014	19
	Totals	7640 c.c.		7717 grm.	77 grm.
	Daily averages	1900 c.c.	...	1919 grm.	19 grm.

	Day		
Total salts excreted	1—4	= 1·0	Experiment with some looseness of bowels
	5—8	= 1·14	
	9—12	= 0·9	
Volume of urine	1—4	= 1·0	
	5—8	= 1·08	
	9—12	= 1·07	

Urine.—In *C*, where purgation was marked, the diuresis observed in the earlier experiments on the same subject, living on a non-purin diet, was not observed. On the contrary there was a lessened volume, 5,630 c.c., in the sulphur-water period, as against 6,325 c.c. for the pre-sulphur or basal line period. There was marked purgation. The average amount of solids calculated from the specific gravity, and compared with that of the basal line period, was increased by about 30 per cent. (31 grm. as against 22·5 grm.), falling again to just below the basal line in the post-sulphur stage.

This increase denotes plus metabolism.

In *B*, which was not associated with purgation but rather with moderate constipation (there being no action of the bowel on one day), the diuresis was moderate, about 6 per cent., but the amount of solids in the urine was not markedly increased.

In *D*, where likewise there was no considerable purgation, but from time to time looseness of bowels, there was an 8 per cent. increase in the volume of the urine in the sulphur period with an increase of 3 grm. per diem in the excreted solids. In the post-sulphur period this diuresis continued with 10 per cent. reduction in the amount of solids passed.

This result was thus intermediate between that where purgation was present and where comparative constipation obtained.

Total nitrogen of the urine in *C* also showed an increase as in the earlier experiments (on non-purin diet), but the short duration of the sulphur period probably did not allow this to develop to the amounts previously observed so that it only amounted to $2\frac{3}{4}$ per cent. (instead of from 5 to 8 per cent.).

TABLE III.

Experiment C.—Total Nitrogen (Kjeldahl).

Days		
1 to 4	Average daily excretion of N	16·1 grm.
5 to 8	" " " "	16·5 grm.
9 to 12	" " " "	16·1 grm.

Average increase in daily excretion of N, $2\frac{3}{4}$ per cent.

The basal metabolism, taken by Dr. Holmes before the subject rose in the morning, showed no essential change.

The blood-pressure, taken by Dr. Edgecombe in *B* and *C* experiments, remained stationary (130 S., 88 D.).

Weight during experiment *C*—observations at same hour of day and under same conditions :—

Pre-sulphur period, loss 5½ ounces daily
Sulphur period, loss 8 ounces daily
Post-sulphur period, loss 1½ ounces daily

A total loss of just under 4 lb. for the whole experiment.

CHART I.

ANALYSIS OF RESULTS.

Total excretion (*F.U.S.*) *in*
(1) Sulphur Period Compared with Basal Period).
(2) Post-sulphur Period " " " "
(3) Sulphur + Post-sulphur Periods " " " "

Ba—Never totally recovered. None in urine.
" " sweat.
" " blood (?)

A 20 per cent. recovered from fæces.
B 26 per cent. " " "
C 80 per cent. " " "
D 61 per cent. " " "

Mn—Mainly in Fæces and Urine.
A 30 per cent. retained in excess
B 20 per cent. " " "
C 300 per cent. " " "
D 4 per cent. " " "
} Excess Excretion of Mn in 3 cases out of 4.

	(1)	(2)	(3)
Ca			
A	16 +	85 +	50 +
B	— 51	— 1	— 26
C	4 +	— 20	— 7
D	15 +	27 +	21 +
Mg			
A	— 52	— 19	— 35
B	— 9	56 +	23 +
C	— 32	— 6	— 19
D	17 +	70 +	43 +
Na			
A	— 31	13 +	— 9
B	— 71	26 +	— 23
C	— 46	— 30	— 38
D	— 14	13 +	— 5
K			
A	12 +	— 21	— 5
B	— 26	42 +	8
C	84 +	8 +	46 +
D	8 +	— 10	— 1
SO_4			
D	89 +	17 +	53 +

SUMMARY.

General Retention of Na ion during sulphur period. The + excretion in three cases out of 4 during the sulphur period of Ca, K and Mn ions suggests + metabolism of compounds containing these ions.

The deficient excretion of the Mg ion is probably only due to the method of calculation which supposes all ions of sulphur water to be excreted during the sulphur-water period.

Possibly the + secretion of K ion is accompanied by replacement of this ion by the retained Na ion.

Analyses.—See first chart. During the sulphur period

Manganese excretion	increased	in	3	experiments	out of 4
Calcium	"	"	3	"	" 4
Potassium	"	"	3	"	" 4
Magnesium	"	decreased in	3	"	" 4
Sodium	"	"	4	"	" 4

During the post-sulphur period the percentage results were about equally divided between plus and minus, except that the retained sodium tended to be excreted and the excretion of magnesium was increased in two experiments out of four.

N.B.—All the metallic ions taken in the ingested sulphur water were supposed to be eliminated during the sulphur water period, and were in each case deducted from the results of the sulphur water periods, before the calculations were made of + or − excretion of the ions during those periods, in order to apply the most unfavourable conditions in calculating the obtained results of plus excretion of ions.

The total percentage excretion in the sulphur and post-sulphur stages when taken together show nothing conclusive except a net retention of the sodium during the combined stages.

In experiment *D* (the only one thus investigated) the percentage excretion of total sulphur was markedly increased during the sulphur water period, over and above that contained in the sulphur water, namely, 89 per cent. during the sulphur period, 17 per cent. during the post-sulphur period, an average excess of over 50 per cent. for the combined periods.

This sustained elimination of sulphur seems to us a very noteworthy result, considering the importance to life and health of sulphur-containing amino-acids (especially such an essential body as cystein and the recently discovered autoxidizable constituent of the cell—glutathione), as so far nothing has been known of the effects of sulphur water on the sulphur metabolism of the body.

It will be noted that by all three avenues (bowel, kidney and skin) the elimination of total sulphur was markedly increased. This result probably has a distinct clinical bearing. Dr. Eve, of Hull, has pointed out that, in some cases of arthritis, neutral sulphur of the urine is increased, in some instances as much as to eight or ten times the normal amount. As is well recognized, neutral sulphur is that part of the sulphur of the urine which is least oxidized and probably comes direct from tissue breakdown.

Our results throw no light on the question of the ultimate origin of the increased sulphur output, but only point to increased breakdown of sulphur-containing substances, either forming part of the food or coming from the tissues.

Further experimentation on the relative quantities of total, neutral, ethereal, and inorganic sulphur, would appear a promising line of research, if combined with accurate observation on the excretion of total nitrogen and urea.

The chlorine excreted in the urine in experiments *B* and *C* in the sulphur period shows an interesting relationship to the sodium excreted during the same period when compared with what takes place during the basal and post-sulphur periods.

The excretion of the chlorine ion during the sulphur period is much more increased than that of the sodium ion during the same period, when these are compared with the results obtained in the pre-sulphur or basal period. (See second Chart.)

CHART II.

COMPARISON OF EXCRETION OF NA AND CL IONS DURING PRE-SULPHUR AND SULPHUR PERIODS.

Pre-Sulphur period		Sulphur period	
Na 7·38 (B)	Cl 7·98 (B)	Na 8·50 (B)	Cl 11·6 (B)
7·00 (C)	5·60 (C)	7·25 (C)	9·5 (C)
Average 7·02 : 23	:: 6·08 : 35·5	Average 8·00 : 23	:: 10·5 : 35·5

Atomic weights: Na = 23; Cl = 35·5. This chart refers to URINE in B and C.

The figures of the sulphur period for the ions of chlorine and sodium roughly correspond with those of the atomic weight of those elements.

This is probably another way of showing the preferential retention of the sodium ion, or we may say that the body extrudes the chlorine ion with greater ease than it does the sodium ion.

The Possible Modes of Action of Sulphur Water.—(1) The simplest theory is that it acts as a common drench, none being absorbed, the effect being due to osmosis, sulphur water being a hypertonic saline solution, with excretion of water into the intestinal canal. If this were so, the constituents of sulphur water should be found in mass in the fæces. Reference to the tables of the daily excretion of these ions in

the fæces negatives this idea. The retention of the sodium ion, as shown in Chart I, col. 1, is alone sufficient to establish this point; furthermore, the excess excretion of the other ions of sulphur water in the fæces in almost every instance is insufficient to account for the daily intake of each basic (metallic) ion in the sulphur water. That being granted, we have to deal with a problem in metabolic change, the mode of which may be simple or composite.

(2) The body ferments responsible for changes in the tissues may be released or able to act better by the removal of waste products inhibiting or reducing their action. Compare the results obtained in experiments *B* and *C*, in relation to the excretion of the ions of calcium, magnesium, sodium and potassium. In *B* (with constipation) the excretion of each of the four ions is minus on this strict method of computation. In *C* (with purgation), on the other hand, calcium and potassium are both plus; and furthermore the excretion of manganese is noteworthily increased.

The evidence then points to there being increased metabolism when free purgation occurs.

Visitors to Harrogate who experiment with the strong sulphur water in minimal doses, come to the consulting room feeling really ill. Also the cases which do not show the ordinary laxative action of sulphur water do not find the treatment invigorating.

In experiment *B* the total of the solids of the urine was little altered, whereas in *C* the increase of solids thus voided was about 30 per cent., a remarkable metabolic effect.

(3) This increased metabolism may be due to direct stimulation of tissue ferments and processes by one or more of the constituent ions of sulphur water. Such an element does exist in sulphur water, i.e. manganese. This analogue of iron, copper, etc., is for all practical purposes a new-found constituent. One of us has already suggested a possible significance attaching to this element. It is true that it is present in very small proportion (·0002 gr. per pint), but this is no argument against its action as a catalyst, since Warburg's experiments on oxidation of metabolites in the presence of minute quantities of iron have been accepted. Further, the earlier work of one of us showed a great stimulation of the xanthine oxidase of the body, resulting in the plus excretion of both fractions of uric acid, that is to say, where both the uric acid derived from the body tissues and that due to food were greatly increased. Now the oxidation of hypoxanthine to uric acid has never been accomplished by ordinary oxidizing agents *in vitro*, but only by the action of xanthinoxidase, a ferment whose action is very special and specific, and which occurs widely in the body and even in milk.

The researches of Dixon (*Biochemical Journal*) into the anaerobic oxidation of precursors of uric acid by the xanthine oxidase of milk, in the presence of methylene blue, and with or without iron (*in vitro*) unlike most such oxidations, gave a negative result. Manganese has not been tried so far as we know, but this metal generally corresponds qualitatively with iron so far as catalytic reactions are concerned.

But if a direct stimulation of this ferment by a catalytic agent is the cause of this increased production of uric acid, then we put forward the opinion that the manganese element in sulphur water is the constituent of that fluid likeliest to be responsible. In an earlier paper it was suggested that this plus formation of uric acid was due to some catalyst or oxidase, the presence of manganese in sulphur water then being unknown. At the same time it must not be forgotten that a release of this ferment by the removal of blocking waste products may cause this activation of xanthine oxidase. These last remarks of course apply to the nuclear metabolism from which the true body uric acid is derived and from the nuclear elements of food, in contradistinction with the general metabolism of the body of the cell. At all events the plus excretion of manganese found during these experiments (in three cases out of four) suggests that compounds in the body containing manganese have been broken down into simpler bodies, and then been excreted.

Manganese tends specially to form peroxides, and the splitting up of manganese compounds of the cell—as shown by the plus excretion of manganese—may also be the source of some of the oxygen required to form the hydrogen peroxide so often found during the anaerobic oxidations of intermediate metabolites (*in vitro*).

That manganese exists in sulphur water in very small proportion is no counter-indication to such action is further suggested by the oxidation *in vitro* of those sulphur-containing compounds cystein and glutathione where the presence of infinitesimal quantities of iron greatly accelerates such oxidation.

A catalytic power is undoubtedly present in sulphur water as in most natural waters containing manganese ions. The magnitude of this catalytic action will form the subject of a further paper.

The greatly increased amount of sulphur excreted during the taking of sulphur water is probably another instance of the stimulation of a tissue ferment (sulphatase originally found by Neuberg in takediastase and subsequently extracted from the kidney and other organs in man) which may be affected by a similar manganese catalytic reaction.

The simpler dipeptides such as glutathione seem to be resistant to oxidation in the body, and the cell is impermeable to them. It has been suggested to us that there is a possibility that sulphur water increases the permeability of the cell to such compounds and that thus increased oxidation of such substances may be brought about.

This oxidation, by whatever means effected, is a very important reaction, since it must concern the sulphur-containing amino-acids of the food, and is another proof of the increased metabolism due to sulphur water. It will be remembered that the increase of sulphates for the whole of the sulphur and post-sulphur periods amounted to 53 per cent.

Radium and its breakdown products may produce similar reactions through the emission of α and γ particles. When, however, we consider the widely different amounts of this element existing in natural mineral waters, we think we are justified in believing that if this constituent were the main cause of the metabolic and therapeutic results of the natural mineral waters of Harrogate, the paucity of radium in those springs (as compared with that of some of the Continental Spa waters) would long ago have caused our own sources to pass into desuetude.

(4) *Sodium.*—That this element is solely responsible for the therapeutic and metabolic effect of sulphur water is negatived by the absence of actions such as commonly occur in sulphur water when sodium is taken in solution as sodium chloride. Again, Sanarelli remarks that at Montecatini the laxative action of waters does not depend on the percentage content of sodium chloride. Of two waters there, he says, the laxative powers of one containing the lesser percentage of this salt has the greater effect. That sodium chloride has in all probability some effect in connexion with the metabolic (metallic ion) alterations, we should be the last to deny.

The absorption and retention in the body of so large a proportion of the sodium ion must have a great effect in changing the crystalloid and colloid constituents of the cell. Such change we suggest must be responsible at least in part for the metabolic changes which result from the taking of sulphur water.

In the United States Whelan has carried out extended experiments with hypertonic solutions of the chlorides of sodium, potassium, magnesium, calcium, etc., injected intravenously: ·07 gm. per kilo of body weight of 10 per cent. solutions was injected into healthy young women and dogs within such a short period as seven minutes without obvious disturbance. The results obtained may be thus summarized:—

(1) Rapid disappearance of sodium from the blood.

(2) A plus excretion of sodium and chlorine in the urine.

(3) The increased excretion in the urine of the other ions (with the possible exception of calcium) followed the injection of any one of the chlorides.

The tissues seem to be able to take up almost unlimited quantities of sodium. That this has a bearing on our results is shown by the retention of sodium during the whole period of each of the experiments.

Sodium excretion in Whelan's experiments seemed to be a measure of the diuresis. Meyer and Kohn's experiments of feeding sodium and potassium chlorides to healthy infants may be mentioned. Sodium chloride added to the diet caused a plus excretion of potassium, and the weight of the infants increased. On the other hand, potassium chloride caused a plus excretion of water, sodium, potassium and chloride and loss of weight.

Here then we have a fourth possible mode of action of sulphur water. This is connected with the storage of large quantities of sodium, either in the cell or its fluids. Such a condition entails an alteration at least of the cell fluids, possibly of the substratum, as it is termed, with consequent modification of metabolic processes.

Clinically, we are accustomed to note an increase in the general body weight during the first two or three days of taking certain saline sulphur waters. This has also been observed when the subject has been on a fixed diet and conditions of life. We suggest that this effect is the reverse one to that seen in treatment of cases of œdema associated with kidney disease (the patient being fed on a saltless diet to lessen the œdema-osmosis effect).

Potassium.—These results suggest that the absorbed and retained sodium is connected with the plus excretion of calcium and potassium observed in three out of four of our experiments (all but the one in which constipation occurred (*vide* Whelan's results).

That calcium and potassium excretions are both increased shows the far-reaching effects on general metabolism, both with moderate and with considerable purgation. Even in the former case, where the increase of excretion of solids in the urine (as shown by increases in specific gravity of that fluid) is not markedly plus, there is an increased excretion of both calcium and potassium.

Magnesium.—As to the temporary retention of magnesium we have no observation to make, except that this is limited to the sulphur period. Excretion was increased in the post-sulphur period. These figures may have been influenced adversely owing to the calculation of the results on the supposition that all the magnesium taken during the sulphur period was eliminated during that period. The percentage results for sulphur and post-sulphur periods taken together show two with plus and two with negative excretion, and no inference can be drawn from these figures.

Calcium.—The excretion of calcium is mainly by the bowel; as might be expected from the experiments of Meyer, which show that whatever route is used for the introduction of barium, strontium and calcium, these ions leave the body by the bowel, rather than by any other channel. He suggests that mass analysis procedures are not delicate enough to show very small quantities of these ions, which should get through the kidney. He compares the want of delicacy of the ordinary methods of analysis with the electroscope methods for showing the presence of minute quantities of radium, a process many thousand times as delicate. The application of this will be apparent now that we come to deal with barium.

Barium.—This was found only in the fæces. There was none in urine or sweat and attempts to show its presence in the blood have as yet been unsuccessful. Both mass analysis and spectrographic methods were used in the latter case.

Further experiments are in progress.

The barium recovered from the fæces varied in amount from 20 to 80 per cent. (average under 50) and the largest amount recovered was in the experiment where there was much purgation. By far the greatest amount excreted was during the sulphur period (ratio, 3 to 1). But the figures put before you with regard to the excretion of barium in the post-sulphur period do not tell the whole story.

The excretion of barium in the post-sulphur period in two experiments, where the fæces were analysed from day to day, show a very rapid fall following the cessation of mineral water drinking. In C, on the last day of the experiment (the fourth day

after cessation of sulphur water) only a trace of barium was found in the fæces. In *A*, only traces of barium were found on the second and third day of the post-sulphur period.

Hence we must conclude that a considerable but variable amount of barium is retained in the body tissues, probably in a nearly insoluble state. Whether this retained barium gets further than the intestinal mucosa, or gets as far as the liver, must be determined by other methods. As far as the examination of the blood takes us, we have no right yet to suggest that it enters the general circulation, and the absence of any effects on this last (rise of blood-pressure, etc.), or of any toxic effects due to absorption where the ordinary laxative effects obtain, sustain us in the opinion that the retained barium is locked up as an insoluble or nearly insoluble compound for some considerable period. As before noted, Meyer's experiments show that barium, strontium and calcium, however introduced, tend to come out by the bowel. There is one point of analogy which may help us in this matter.

A chemical body, a substitution phenol compound containing arsenic and bismuth, a successor of trépol, has been used by French observers in the treatment of rabbit and human syphilis. It was given intramuscularly with disappearance of the treponema in all cases in from two to four days. There was no relapse, yet at the end of a month on removal of the tissues about the site of injection (in rabbits) almost 100 per cent. of the bismuth was obtained on analysis. The authors suggest that the absence of relapses is due to the fact that there was continuous medication during the whole period. Only 40 micro-milligrams were recovered from the whole liver (*Lancet*, 1926). Possibly the fate of barium in sulphur water is similar, but only extended experiments on animals can settle the point as to the ultimate fate of the retained barium, and the micro mass analysis methods of Pregl would, we think, require to be utilized.

Because of the alteration in metabolism and the consequent excess or diminished excretion of the ions contained in sulphur water it is impossible to give percentage results as to the excretion of these ions in the fæces, urine and sweat. Only actual increases and decreases can be shown.

CHART III.

(+ and −) PERCENTAGE EXCRETION OF THE IONS.

Ca, Mg, Na and K in the fæces (F), urine (U) and sweat (S) during Sulphur period.

	A	*B*	*C*	*D*
F				
Ca	142 +	+ 23	89 +	42 +
Mg	− 14	− 40	57 +	63 +
Na	9 +	− 88	245 +	− 3
K	− 45	− 12	76 +	− 5
		Ca 3 out of 4 +		
		Mg 2 ,, ,, 4 +		
		Na 2 ,, ,, 4 +		
		K 1 ,, ,, 4 +		
U				
Ca	102 +	− 3	15 +	42 +
Mg	− 12	− 28	18 +	62 +
Na	30 +	16 +	25 +	57 +
K	− 35	− 24	35 +	12 +
		Ca 3 out of 4 +		
		Mg 2 ,, , 4 +		
		Na 3 ,, ,, 4 +		
		K 2 ,, ,, 4 +		
S				
Ca	390 +	64 +	20 +	45 +
Mg	52 +	160 +	12 +	200 +
Na	238 +	126 +	28 +	61 +
K	380 +	− 26	19 +	17 +
		Ca 3 out of 3 +		
		Mg 3 ,, ,, 3 +		
		Na 3 ,, ,, 3 +		
		K 2 ,, ,, 3 +		

Results in case (*A*) neglected.

Here the great point seems to be the greatly increased excretion by the skin (70 per cent. for the mass d ions), a fact worthy of note in connexion with the clinical results obtained in skin cases not infected with staphylococcic organisms. It is only in the case (*B*) in which constipation occurred that any of the skin excreta were lessened (results + in 11 out of 12 items).

Generally we can say that in accordance with Meyers' researches, calcium tends to be excreted in a greater indicated percentage from the bowel than in the urine.

With potassium there seems to be selection of avenue as between bowel and kidney in favour of the kidney. Sodium with much purgation was excreted in greatly increased quantities by the bowel.

There is a final point, possibly of very great clinical importance. This refers to the excretion of the elements of sulphur water by way of the skin during the sulphur-water period (Chart III).

Here the results in experiment *A* require elimination, as the basal part of the experiment was done under conditions of external temperature and humidity differing from those of the sulphur and post-sulphur periods. Such factors have considerable effect on excretion by the skin.

Here sodium, potassium, magnesium and calcium are excreted by the skin in very greatly increased proportion. A massed average increase is observed in all the cases of over 70 per cent., and only one moderate negative result occurs in the whole of the twelve findings.

In the post-sulphur period there is no result to note. Two out of four of the collections of this period were unfortunately lost.

This increase of excretion of the inorganic constituents should be thought of in connexion with the type of skin disorders treated in our spas. The plus excretion of calcium, magnesium, and sodium was greatest; this is interesting in relation to the use of preparations of calcium, etc., in the ordinary treatment of such cases. The proportion of chlorine excreted by the skin also showed a plus amount of roughly 50 per cent. in *D*. To sum up the results of sulphur-water drinking which we claim to have established:—

(1) An increase of metabolism.

(2) The action of manganese in sulphur water probably provides the catalyst necessary for the stimulation of body ferments and possibly also of xanthine oxidase.

(3) That barium of sulphur water has so far not been found in the general circulation; on the average 50 per cent. is retained. Whether this is slowly fed to the tissues or is locked up in the intestinal mucosa and thence gradually excreted, or whether it is lodged in the liver and thence gradually fed to the tissues is undecided. In consonance with pharmacological experiments with pure preparations, the avenue of excretion, if any, seems to be the bowel.

(4) There is distinct retention of sodium in the body with an increased extrusion of calcium and potassium. This agrees with results obtained by other experimenters using pure sodium preparations. There must also be a consequent modification of the crystalloid constitution of the cell.

(5) The sulphur metabolism of the body is increased by 50 per cent., and this increase, by analogy with known effects of iron on free cystein and glutathione, may be due to the presence of manganese in sulphur water.

(6) The chlorine ion is extruded from the body with greater ease than the sodium ion.

(7) The percentage increases in excretion of chlorine were greatest by the bowel. The increased percentage excretion of sodium was greatest by the urine, except where there was great purgation.

(8) The excretion by the skin of calcium, magnesium, sodium and potassium was increased to about 70 per cent. above the normal.

(9) During constipation the excretion of all ions (except sodium by urine and fæces) was decreased. Purgation increased the metabolism most.

The plus excretion of the ions which exist in sulphur water was a result entirely unexpected and especially the plus excretion of manganese. The retention of barium in some position almost if not entirely outside the blood-stream was also unthought of.

The retention of sodium in the general tissues was dimly known, but so far as we know, there were no definite figures proving its extent. The plus excretion of calcium and potassium might have been prognosticated from reading the literature on hypertonic injections of sodium chloride.

The effects on excretion of the various ions by the skin appear to us to be very striking.

As to the possible methods by which sulphur water acts we place :—

(1) The more rapid removal of waste products when formed.

(2) The probable stimulation of the body ferments, possibly also of xanthine oxidase and sulphatase, by manganese, a constituent of sulphur water.

(3) An alteration in the cell fluids consequent on the absorption and retention of the sodium ion.

(4) Possible increase of permeability of the cell to sulphur-containing bodies.

N.B.—(1) may only be a consequence of (2), (3) and (4).

We are unable at present to suggest a definite and satisfactory part for barium in the increased metabolism which follows ingestion of sulphur water in quantities clinically suitable.

We wish to express our indebtedness to Professor Sir Frederick Gowland Hopkins, F.R.S., for much encouragement and kindly criticism during the progress of our investigations.

We also wish to express our indebtedness to Professor Pembrey, F.R.S., of Guy's Hospital Medical School, for interest shown in our work, to the Committee of the Harrogate Royal Bath Hospital for facilities granted, and to Mr. Morris, Senior Bath Attendant at the same hospital, for very exact and careful service rendered.

Analysis of Sulphur Water.

	1 pint O.S.W.	24 oz. for C.
Ba	0·0401 grm.	0·048 grm.
Mn	0·0002 ,,	0·00024 ,,
Ca	0·2431 ,,	0·2914 ,,
Mg	0·1050 ,,	0·1260 ,,
Na	2·9780 ,,	3·5736 ,,
K	0·0417 ,,	0·0501 ,,
Cl	0·3000 ,,	0·3750 ,,
S as SO_4	0·1680 ,,	—

Daily dose in

A, B, D.	20 oz.
C.	24 ,,

Protocol of Average Daily Excretion in Experiments A, B, C, D.

A.

		Pre-sulphur period	Sulphur	Post-sulphur
F.				
	Ba	—	0·0088	0·0025
	Mn	0·00148	0 00109	0·00187
	Ca	0·1152	0·2785	0·2641
	Mg	0·1564	0·1350	0·1184
	Na	0·4875	0·5326	0·3347
	K	0·2376	0·1079	0·0734
U.				
	Ba	—	nil	nil
	Mn	0·0007	0·00029	nil
	Ca	0·1145	0·2318	0·1657
	Mg	0·1075	0·0953	0·0974
	Na	4·98	6·484	5·891
	K	1·140	1·434	1·012
	Volume	1,598 c.c.	1,370 c.c.	1,401 c.c.
S.				
	Ca	0·0026	0·0123	—
	Mg	0·0021	0·0032	—
	Na	0·0489	0·1054	—
	K	0·0114	0·0528	—

B.

Experiment with Relative Constipation.

		Pre-sulphur period	Sulphur	Post-sulphur
F.				
	Ba	nil	0·0061	0·0035
	Mn	—	0·0000107	0·0000556
	Ca	0·3958	0·2442	0·4651
	Mg	0·1064	0·0512	0·2676
	Na	0·4678	0·0445	1·5360
	K	0·222	0·1529	0·3557
U.				
	Ba	nil	nil	nil
	Mn	0·00001	0·00001	0·0000371
	Ca	0·3381	0·3159	0·267
	Mg	0·1787	0·2295	0·1777
	Na	7·38	8·5434	8·324
	K	1·9016	1·4528	2·6712
	Cl	7·98	11·6	9·85
S.				
	Ba	nil	nil	—
	Mn	nil	0·0000333	—
	Ca	0·0047	0·0123	—
	Mg	0·0015	0·0024	—
	Na	0·0987	0·01251	—
	K	0·0355	0·0262	—

C.

Experiment with Considerable Purgation.

		Pre-sulphur	Sulphur	Post-sulphur
F.				
	Ba	nil	0·0309	0·0107
	Mn	0·000025	0·0C073	tr.
	Ca	0·3405	0·638	0·2872
	Mg	0·0665	0·157	0·0635
	K	0·1269	0·224	0·152
	Na	0·0251	0·0868	0·2476
U.				
	Ba	nil	nil	nil
	Mn	nil	tr.	tr.
	Ca	0·0493	0·0568	0·0497
	Mg	0·0297	0·0346	0·0313
	K	1·755	2·217	1·867
	Na	7·00	7·25	5·387
	Cl	5·6	9·5	6·34
S.				
	Ba	nil	nil	nil
	Mn	tr.	0·000018	tr.
	Ca	0·372	0·0446	0·0049
	Mg	0·0055	0·0062	0·0015
	Na	0·0428	0·0600	0·0390
	K	0·0603	0·0712	0·0156

D.

Experiment with some Looseness of Bowels.

		Pre-sulphur period	Sulphur	Post-sulphur
F.				
	Ba	nil	0·0150	0·0093
	Mn	0·00175	0·0021	0·0015
	Ca	0·675	0·959	0·937
	Mg	0·1489	0·2331	0·236
	Na	0·0892	0·0862	0·1560
	K	0·300	0·285	0·272
	SO_4	0·1020	0·1792	0·1285
U.				
	Ba	nil	nil	nil
	Mn	0·000035	0·000075	tr.
	Ca	0·180	0·257	0·175
	Mg	0·0887	0·1445	0·1312
	Na	4·127	6·490	3·737
	K	1·915	2·152	1·665
	SO_4	0·775	1·674	0·895
S.				
	Ba	nil	nil	nil
	Mn	tr.	0·000017	0·000007
	Ca	0·0404	0·0587	0·0311
	Mg	0·0039	0·0113	0·0044
	Na	0·0272	0·0439	0·0780
	K	0·0310	0·0363	0·0706
	PO_4	tr.	tr.	tr.
	Cl	0·0017	0·0087	0·0122
	SO_4	0·0564	0·0801	0·0711

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Dr. FORTESCUE FOX said the findings of Dr. Brown and Mr. Woodmansey were of considerable importance in hydrology. He referred especially to the observations which appeared to show a definite increase of metabolism in the sulphur period on the evidence of an excess of manganese, calcium and potassium ions. It might be better to concentrate on definite limited objectives of this kind. He had, however, often pointed out the limitations of research on metabolism in healthy subjects. Sulphur waters were not given to healthy persons. From new knowledge along more than one line of approach, there was reason to believe that the abnormal metabolism of exudations and deposits was more easily effected by external treatment and waters than was the metabolism of the healthy body. It should not be impossible to carry out investigations of this kind upon persons whose metabolism was disordered.

Mr. RACE said that although he presumed that the ions in the ingested water had been deducted in arriving at the final figures, he thought that this should be definitely stated.

The most remarkable increase in the excreted ions during the sulphur period was that of sulphate, and it was a matter for regret that, although a large amount of research work had been directed to the various forms in which the nitrogen portion of the protein molecule was excreted, there was comparatively little reliable data as to the nature of the end products of the sulphur portion, with the exception of the fully oxidized that is excreted as sulphate.

In the cases investigated at Buxton there was no suggestion of an increased sulphur to nitrogen ratio in the urine, although samples contained a proportion of unoxidized sulphur that was larger than the usually accepted normal value.

Dr. HALLS DALLY referred to McCarrison's experiments in India on very large numbers of rats fed on deficiency diets. An addition of Mn 003 to the daily ration obviated in great measure the results previously obtained by such diets.

The PRESIDENT and other Members of the Section also took part in the discussion.

Dr. BROWN and Mr. WOODMANSEY (in reply to Dr. Fortescue Fox) said that their intention was to extend their observations to various types of pathological cases (especially those of arthritis), but pointed out the difficulty of adequate control of diet, etc., of ambulant cases in the mineral water hospitals.

Mr. Race's suggestion, for which they thanked him, would be incorporated in the published paper.

They also thanked Dr. Halls Dally for his favourable criticism of the paper, and also for drawing the readers' attention to the results of McCarrison's work with manganese, and the possible analogy which might exist between the action of this element and that due to vitamins.

Section of Dermatology.

[*December* 20, 1928.]

Benign Lymphogranulomatosis (?).—H. HALDIN-DAVIS, M.D.—T. N., a boy, aged 18, previously shown October, 1928. He had an extensive rash covering the whole trunk, with lesions on the face and limbs, extending on to the hands, but not below the knees. These were irregular in shape, purplish in colour, and in places somewhat scaly on the surface. They were indurated, but the induration was softer than is found in syphilitic lesions. Soft subcutaneous nodules were also to be found in some places. During the last two months the lesions have become somewhat flattened down, and a few have disappeared leaving stippled scars. New ones have, however, arisen. He has also general enlargement of all the lymphatic glands, not, however, to so great an extent as when first seen.

The condition has been present about a year. The patient says that he has felt much better since being in hospital and has undoubtedly improved.

The Wassermann reaction (taken twice) is negative. The blood-count shows no great change from the normal. Radiograms both of the chest and of the hands show nothing abnormal, either in the contents of the thorax or in the bones. A remarkable feature of the case is the histology (investigated by Professor Hadfield) of the affected skin and the enlarged glands, which show numerous and well developed giant-cell systems such as are commonly found in tubercle, in fact, the glandular tissue is almost entirely replaced by them (see figs. 1 and 2). But when injected into a guinea-pig, neither skin nor gland has caused any reaction whatever. I am unable to place the case with certainty in any known category, but I suggest that the condition more nearly resembles a case of benign lymphogranulomatosis than anything else, although I am well aware that the absence of rarefaction of the bones is much against that view.

Discussion.—Professor GEOFFREY HADFIELD said that the histological changes in the subcutaneous tissue and lymphatic glands were identical, and consisted of a chronic inflammatory infiltration affecting their histiocytic elements, giving rise to the production of large numbers of non-caseating giant-cell systems. No acid-fast bacilli, granules or "splitter" had been demonstrated in either tissue, nor did the inoculation of comparatively large amounts of material produce any lesion in guinea-pigs. Cultural experiments were negative, special attention being directed to the isolation of pathogenic yeasts. No spirochætes were found. In spite of the failure to discover a definite infective agent, the lesions were so typical in every histological detail of those produced by the tubercle bacillus, that one was driven to the conclusion that they were the result of tuberculous infection of a low degree of virulence, or that the patient was possessed of a high degree of immunity. The striking absence of caseation, and the large numbers of unaltered multinucleated giant-cells supported the latter view. The blood showed no noteworthy alteration.

Dr. KINGSTON BARTON said that, as a general practitioner, he was often interested in the fact that a case might apparently have physical signs of tuberculosis in some form without the pathologist being able to find the bacillus to prove the diagnosis. Two or three years ago a patient—who eventually died—had every sign pointing to pulmonary tuberculosis, but in the sanatorium in which he was living, so much doubt was felt, that skiagrams were taken and the shadows seen were regarded as those of malignant growths. Post mortem it was clear that the shadows represented tuberculomata free from caseation. All the microscopic sections showed intense giant-cell formation. That patient had had copious sputum, but no bacilli had been found in it.

Dr. A. M. H. GRAY said this case bore a resemblance to one that he had shown before the Section five or six years previously. The patient was a child, under Dr. Still's care, at Great Ormond Street Children's Hospital. He had an eruption which he (Dr. Gray) described as an unusual type of acne scrofulosorum. The lesions were on the extensor aspects of the limbs, face, and scalp. They were indolent papules which were undergoing pustulation. They were grouped together, forming large red infiltrations on the extensor aspects of the limbs; on the scalp they produced permanent scarring and loss of hair.

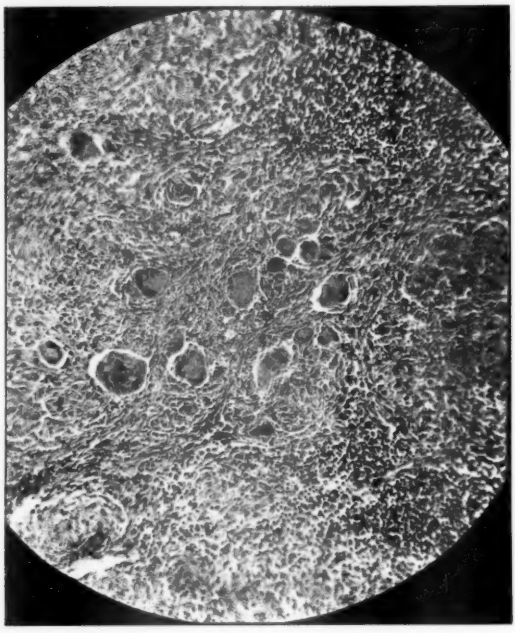

FIG. 1.—General view of section of lymphatic gland from groin. Note giant-cell systems but no caseation. An emulsion of this gland produced no lesion when injected into a guinea-pig.

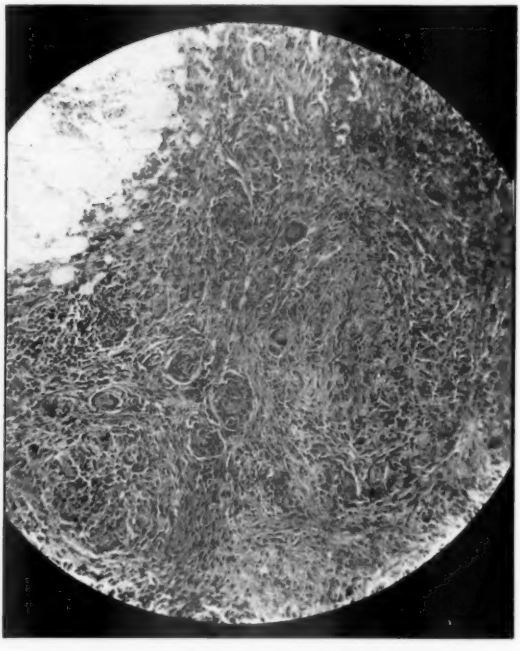

FIG. 2.—General low-power view of skin lesion. Nodule of histiocytic and fibroblastic cells with many multinucleated giant-cells.

Nævus Anæmicus (Ischæmicus).—F. PARKES WEBER, M.D.—This woman, aged 32, has a group of irregular "blotchy" areas of pale skin on the dorsal-radial aspect of the middle of her right fore-arm, which has been observed since childhood and is doubtless a birthmark. There is no other cutaneous abnormality or disease. The Wassermann and Meinicke reactions are negative. At first sight the birthmark appears to be a simple hypochromic nævus (the opposite of a simple pigmentary or hyperchromic nævus), but it can be no longer recognized (its outlines completely disappear) when the skin of the whole region is rendered anæmic by the pressure of a glass slab. On the other hand, by contrast, it shows up well when, by the application of gentle friction, the surrounding skin becomes flushed. When the outlines of the birthmark have been obliterated by the pressure of a glass slab they always re-appear in exactly the same pattern on removal of the pressure. The condition is obviously not due to any local deficiency of cutaneous pigment, but is a *nævus anæmicus* (H. Vörner), that is to say, the opposite condition to that of ordinary capillary angiectatic nævus ("port wine mark"). I was, however, surprised to find that on rendering the skin of the region exceedingly hyperæmic by sponging it with hot water, the nævus anæmicus became red, just like the skin surrounding it, the resistance to vasodilatation being evidently overcome by the hot application. According to H. Vörner (*Arch. f. Derm. u. Syph.*, Leipzig, 1906, lxxxii, p. 391, and *ibid.*, 1912, cxi, p. 149), the anatomical cause of a nævus anæmicus is that the cutaneous blood-vessels of the affected area are all capillaries that do not dilate. One now knows, however, that normal capillaries, like other blood-vessels, can actively dilate, and the present case shows that even the capillaries in a so-called nævus anæmicus can dilate, as a result of the application of hot water to the skin of the part; in fact, the outline of a nævus anæmicus may be completely lost (at all events, in this present case) by the application of intense warmth, just as it is lost by pressing a glass slab over it so as to render the surrounding skin anæmic.

Acanthosis Nigricans.—DOUGLAS HEATH, M.D.—Patient, a single woman, aged 63. Right breast removed for carcinoma in 1916, and left breast for same disease in 1921. Uterus removed for fibroid tumour in 1916.

In 1925 the patient was treated by diathermy for bleeding papilloma of the bladder. She did well for a time, but in 1926 blood was again noticed in the urine, and it has been frequently present during the past six months. A recent examination (December 7, 1928) showed pus, blood and squamous epithelial cells in the urine, but no fragments of papilloma were found.

In January or February, 1928, she began to have severe itching of the skin in all parts of the body without any eruption being visible. This pruritus is still present.

In June, 1928, she noticed that the skin of the axilla, anal region and back of neck was becoming rough, harsh and dark in colour, and further patches have developed during the past three months on the abdominal wall, epigastrium and flexures of the arms and legs (slight).

Present State.—Patient, a thin, anæmic woman, has been losing strength and weight for the last twelve months. She declines cystoscopic examination. The skin of the face, ears and front of the neck is practically normal in colour and texture. On the back and sides of the neck, from the hairy margins of the scalp downwards to the shoulders, the skin shows diffuse, warty thickening and some degree of greyish-brown pigmentation. More marked warty thickening and a greater degree of pigmentation of a dark brown to black colour can be seen in the anterior axillary, anal and genital regions. Similar warty patches of a light brown colour can be seen on the epigastrium, abdominal wall and flexures of the limbs. Many small, unpigmented warts, sessile and pedunculated, are dotted over the trunk, limbs and backs of the hands, but none are to be seen on the face. There is a slight uniform diffuse keratosis of the palmar surface of the hands and fingers and some thickening over the knuckles on the dorsal surface of several of the fingers. The soles of the feet do not show a similar diffuse keratosis. The nails of

the hands and feet are unaffected. The whole of the hard palate is covered by a thick, pale, warty overgrowth, and the inside of the cheeks shows a papillary hypertrophy, which has the effect of throwing the mucous membrane lining the cheeks into folds. The soft palate is not affected. The tongue is covered with long filiform papillæ and the whole of the organ is probably, for this reason, larger than normal.

I think this is a rather earlier stage of the condition than that in the cases previously shown to the Section. Dr. MacCormac showed one eight years ago, and Dr. Graham Little and Dr. Nixon have also shown cases. In all these instances the condition was secondary to abdominal carcinoma, and I think that here it is a similar sequel. The patient had a papilloma of the bladder two or three years ago, and it is likely that malignancy has now set in. She does not yet show the affection of the face and the backs of the hands to the extent depicted in Politzer's Atlas; but if she lives long enough she may yet do so.

In the literature one sees the statement that juvenile cases of the disease occur; but I have not myself seen it unassociated with carcinoma. It is said that it may follow long exposure to light and high temperatures.

Dr. J. M. H. MACLEOD (President) said that in nearly thirty years' experience he had only seen a few cases of this rare affection, and in each of those cases abdominal cancer had co-existed.

Poikilodermia Atrophicans Vasculare (Jacobi).—S. E. DORE, M.D.—Patient, a woman, aged 43. The onset of the complaint was insidious, about 20 years ago. The present distribution of the eruption is mainly on the front of the trunk, the breasts, hips and groins being specially affected. The lesions consist of patches of various sizes, from a few inches to 15 in. in diameter. Those of recent development are brownish-pink in colour, with a well-defined margin, and slightly scaly, with marked accentuation of the skin furrows. In the older patches the outline is less defined, and there are pigmentary and atrophic changes. The pigmentation is especially marked on the right breast where there is a large area consisting of telangiectases, pigmentation and atrophy, with a reticular configuration. On the left breast the affected area is smaller and less retiform, but with definite areas of atrophy. In the fold of the right groin there are large pigmented patches with white central atrophy. There is no sclerodermic thickening, and nothing suggestive of lupus erythematosus, the face, scalp and extremities not being affected. There is no leucoplakia, but patches of "Fordyce's disease" are present on the buccal mucous membrane. The Wassermann reaction in the blood is negative, and a differential blood-count shows no abnormal features. Itching is not a marked feature, but occurs at times, especially when the patient becomes over-heated.

A biopsy has been made, and the pathologist's report is as follows: "The skin shows some acanthosis and parakeratosis, with a very dense round-celled infiltration of the superficial half of the corium, with a slight excess of deposit of pigment. The appearances do not confirm those of the retiform variety of parapsoriasis, but suggest rather those of poikilodermia atrophicans vasculare."

Discussion.—Dr. J. M. H. MACLEOD (President) said he considered that this case conformed to the type described by Jacobi, and was quite different from those cases which Civatte had published. Civatte's cases occurred only in women, and the lesions were manifested about the face and neck and were less pronounced in appearance and much more limited in distribution than those in Jacobi's cases. There were two distinct phases in the histology of Jacobi's poikilodermia, namely, an inflammatory phase which was that seen in the skin, and a later, atrophic stage.

Dr. J. A. DRAKE said he believed that all Jacobi's cases were in males.

Acquired Epidermolysis Bullosa.—H. MACCORMAC, C.B.E., M.D.—Patient, a boy, aged 14, presents a very rare form of a rare and unusual disease—non-congenital epidermolysis bullosa. There is nothing of note either in the family history, or in the previous personal history of the patient. His three brothers and

both his parents are healthy and free from any form of eruption. The first manifestations in the present case developed on the face five months ago. About the same time blisters were observed in the mouth, in which situation they have continued up to the present time. Subsequently and at intervals, the axillæ, groins, buttocks, forearms, hands and feet, were affected, and contents of the bullæ being in most cases clear, but in some, definitely blood-stained from the first. The patient is aware that any trivial injury is followed by a blister, and the distribution of the eruption corresponds with those regions of the body most liable to slight degrees of trauma. This case appears to belong to the second or dystrophic form of epidermolysis bullosa, characterized by atrophic changes in the affected skin, and by milium-like cysts both of which are well-marked in the present case. Thus the skin on the elbows and knees is thin and wrinkled and is dotted with very numerous milium-cysts. It is interesting to note that, where the acquired form of the disease has been observed, it has also been seen in association with this dystrophic type, as pointed out by Wise and Lautman. The blood-picture reveals no unusual features except a slight degree of eosinophilia (4 per cent.). A section of the skin showing the milium-like bodies supports Adamson's view that they are implantation cysts and not derived from blocked sweat-ducts.

Pityriasis Lichenoides et Varioliformis Acuta (?). Case for Diagnosis.—W. KNOWSLEY SIBLEY, M.D.—The patient, a healthy-looking boy aged 16, with no previous history of illnesses, when first seen (October 25 1928), had had a diffuse macular rash on his trunk for two days. He said he was quite well and free from irritation or any symptoms. His temperature was 99·2. I made a provisional diagnosis of early varicella. A week later the appearance had changed, the rash was papular and extensive, and somewhat resembled an acute lichen planus, but there was still no irritation or other symptom and the boy said he felt quite well. Another week later the condition had again considerably changed in appearance, a good deal of the rash had cleared up, and there was some general desquamation and slight irritation. The distribution was that of a pityriasis rosea, the rash being limited to the trunk. In fact, the whole condition that week was not unlike a fading pityriasis rosea. Yet a week later, on November 28, 1928, he was shown at the St. John's Hospital Dermatological Society. There were then only a few scattered papules situated more especially round the flanks and axillæ. There was some irritation. On that occasion Dr. W. N. Goldsmith diagnosed the condition as pityriasis lichenoides et varioliformis acuta. On December 13, 1928, the rash had become much more marked again, was deeper in colour, and had extended as far as the elbows and from the thighs nearly to the knees; there was also some extension on to the sides of the neck. There was an absence of rash on the buccal mucosa and the genitals, and there was no adenitis. *Pathological Report* (Dr. Muende).—"The von Pirquet reaction is negative. A microscopical section shows a plateau with slight irregular downgrowth of the interpapillary processes and some parakeratosis. The corium shows evidence of a reaction of a chronic irritative nature." *Blood-count.*—Red-blood cells, 5,960,000, hæmoglobin, 80 per cent., colour index 0·7, white blood-cells, 11,400. Differential (per cent.).—Polymorphonuclears, 63·5; small lymphocytes, 22·0; large lymphocytes, 8·5; large hyalines, 1·0; eosinophils, 4·5; basophils, 0·5.

Twelve cases have been described since 1916, one by Mucha, three by Rusch, two by Oppenheim, one by Kruger, two by Habermann, one by Almkvist, and two by Dr. A. M. H. Gray shown to this Section, one in November, 1926, and one in October, 1928. Some points of similarity between my case and that shown by Dr. Gray in November, 1926, are: (1) Both the patients were healthy adolescent boys living in a charitable institution; (2) in both the condition was first diagnosed as varicella; (3) in both the rash tended to remain round the anterior folds of the axillæ; (4) itching was not a marked feature in either case and in both the general health was unaffected. On the other hand, I have not yet noticed the pitting described in the other case, nor have I seen any papules with definite central necrosis; in fact,

it is impossible to describe the eruption in my case, as at each weekly interval, when the patient has been seen, the character has been quite different. It has, however, been more or less papular all the time.

Dr. W. N. GOLDSMITH said that the history of lesions looking like varicella, followed by papules, many of which showed a little central necrosis, had made him think this might be a case of pityriasis lichenoides et varioliformis acuta. Very few such cases had been described. The affection generally began acutely like varicella, but afterwards rather resembled pityriasis lichenoides chronica (of which it may be an acute form). He did not feel at all sure about the present case: the papules were smaller than in the cases shown by Dr. Gray and the necrosis less marked. This patient now had itching and scratched the papules. But when he had seen him previously there was no itching and he thought the central scabbing was spontaneous.

Lichen Obtusus Corneus.—G. B. DOWLING, M.D.—A. B., male, aged 63.

History.—Nine years ago a fall on the feet from a height was followed by great swelling of the feet, ankles, and lower parts of the legs, for which the patient was laid up for six weeks. On getting up his feet began to itch, and in the course of months lumps developed on the ankles and leg. These increased in number and size for several years, particularly on the right leg, but they have remained in a stationary condition during the past two or three years. At some time during this period of nine years an itching eruption also developed on the extensor aspects of the elbows and on the buttocks.

Examination.—A large number of cutaneous nodules, dome-shaped and violaceous in colour, are present on the lower two-thirds of the right leg and on the ankle. Some of them are isolated, others are densely aggregated to form convolutions separated by deep sulci. If the convoluted patches are compressed laterally, drops of pungent-smelling cheesy matter can be expressed. A small confluent patch is present on the inner aspect of the left ankle. On the buttocks and elbows the lesions are circumscribed patches of lichenification. No lichen planus papules are present, and there are no buccal lesions.

Histology.—Excised nodules reveal the following changes: No epidermal changes of importance except slight hyperkeratosis. A circumscribed layer of densely packed cells is present in the corium, just beneath the papillary layer, which itself appears to be normal. The cells, many of which are fusiform, appear to be chiefly connective tissue cells in the process of laying down new connective tissue. They are present only over a very limited area which I think may represent the summit of the dome and the point of maximum irritation and friction.

Diagnosis.—The history suggests that the evolution of the lesions was from damage to the skin (resulting from the fall), to pruritus, and from friction to the development of cutaneous nodules.

The development of circumscribed lichenification elsewhere and the complete absence of lichen planus papules excludes the diagnosis of hypertrophic lichen planus.

I regard the condition, therefore, as a type of circumscribed lichenification, and I consider that it corresponds to Brocq's definition of lichen obtusus corneus.

Discussion.—Dr. KNOWSLEY SIBLEY said this case did not give him the impression of lichen obtusus corneus, two or three cases of which he had shown before the Section in 1916 and 1919. In that condition the lesions were isolated, they never amalgamated to form a mass, and the irritation was very acute. This case did not present that degree of irritation, nor were the lesions at all conical.

Dr. PARKES WEBER said that Dr. Dowling's explanation of the condition, as the result of repeated rubbing and scratching, seemed to be confirmed by the recent development of a kind of prurigenous lichenification, notably on the left forearm. But the circular arrangement and hyperplastic nodular appearance of the lesions on one leg hardly admitted of such an explanation.[1]

[1] I have seen similar (non-hæmorrhagic and non-pigmented) lesions supervene in a case which began as one of undoubted so-called "idiopathic multiple hæmorrhagic (pigment) sarcoma (Kaposi)."—[F. P. W.]

NOTE.—The reports of Dr. W. N. GOLDSMITH'S case (Nodules on Scalp) and of Mr. H. CORSI'S Sections from his Case of Punctate Melanoderma will be published in the next issue of the *Proceedings*.

Section of Epidemiology and State Medicine.

[*January* 25, 1929.]

DISCUSSION ON VACCINATION AGAINST SMALLPOX IN THE LIGHT OF RECENT EXPERIENCE.

Professor J. C. G. Ledingham: The title of this discussion implies, I think, that the vaccination problem in its various aspects, scientific, medical and administrative, requires review in the light of recently acquired knowledge. It will be my task to explain as briefly as possible what, to my mind, constitutes the essence of this new knowledge, and how far, in my view, it can be relevantly employed as a lever in influencing the general outlook, or in modifying opinions on this subject which responsible and informed persons currently hold. What is the gist of this new knowledge? Much of it, and perhaps the most important part of it, has emerged from comparatively recent experimental research, not only on vaccinia and smallpox, but on virus diseases generally. For many decades since Jenner's time the study of vaccine virus was largely confined to improvements in the technical procedures necessary to secure lymph of good quality for purposes of vaccination. I do not forget the controversies between the unicists and dualists, which arose from the apparent success of the former and the failure of the latter to effect the postulated transformation of smallpox to vaccinia by variolization of bovines. While the victory is now admittedly with the unicists, the problem still retains its interest and should, indeed, retain its interest for some time to come in view of the remarkable principle it enshrines. It is a principle which is paralleled only by the transformation which "street" rabies virus of canine origin undergoes by repeated passage through the brains of rabbits. While the canine virus has the power to produce rabies in man, the "fixed" or rabbit-passed virus is held to be practically innocuous both to man and dog when administered by the subcutaneous route.

One result of this detached preoccupation of lymph-producers in maintaining potent strains of lymph by aid of many and curious variations of animal passage—by no means yet fully intelligible—was that lymph manufacture came to be regarded even by workers in other fields of bacteriology, who ought to have known better, as a sort of mystery cult. Many important questions remained unanswered until viruses became the object of the intensive study that is now being devoted to them.

Considerations regarding Viruses.

Let me deal first with a few of the scientific problems germane to this discussion and begin with the origin of cowpox, a subject with which this Section has, in the past, not infrequently occupied itself. A few years ago Reece (1922), in this Section, expressed the view that outbreaks of cowpox were then non-existent, but I note that in the Section of Comparative Medicine, recently, Male (1927) stated that such occurrences were extremely frequent. If this is really so, it is unfortunate that no attempt is made to investigate them in a scientific manner, so that not only may their real nature be placed beyond dispute, but their epidemiological features carefully investigated. It is not always possible to refer such outbreaks to an origin in smallpox or vaccinia in man, but the available evidence undoubtedly points most clearly to contact with human vaccinees. In a quite recent German outbreak reported by Winkler (1927), and involving about fifty cows, human smallpox was definitely excluded, but not so human vaccinia, as children whose mothers acted as milkers had been vaccinated shortly before the outbreak among the cows. An origin from human smallpox should not, however,

be excluded where such is possible, simply because difficulty in the experimental variolization of bovines is the rule, for we know that such variolization of bovines has sometimes been accomplished with remarkable ease, e.g., in tropical countries where outbreaks of smallpox have necessitated the securing of lymph stocks at short notice by variolization of the local bovines. What Jenner really meant by horse "grease" I do not know and probably it does not matter, but there is experimental evidence that horse-pox is like cowpox, due to the same modified variola virus and occurs, though with admitted rarity, under precisely similar circumstances.

With regard to the sources of lymph strains employed for the inoculation of calves, Dr. Kaiser, of Vienna, informed me only a few weeks ago that he still uses a strain which traces its descent from a sample of cowpox sent to Austria by Jenner himself. There is no doubt, however, that the majority of strains used in lymph-producing institutes have been derived initially from human smallpox by a procedure which can, with fair success, be realized at will. We are, indeed, on sure ground when we start in our experimental work with human smallpox and attempt to pass it to lower animals, such as the monkey, rabbit and calf. Accommodation is often difficult, sometimes it succeeds easily. Only the other day I received from Professor Sobernheim, of Berne (1928), a sample of variola-vaccine, derived from the prevailing mild smallpox in England, the method employed being that of repeated skin passage on rabbit previously developed and tested by Blaxall (1923). This lymph when titrated by me on the rabbit was no longer recognizable as smallpox. Whether placed on the cutaneous scarified surface or introduced into the dermis, it ran out to the same titre. Smallpox virus obtained direct from man produces regularly a dermal response in the normal rabbit (Ledingham, 1925, McKinnon and Defries, 1928), but never when rubbed into the scarified skin. Repeated passage is necessary. We know also that smallpox virus has, after accommodation to the rabbit, behaved like vaccinia when transferred back to man, so that, for the realization of this modification of smallpox virus, which manifests itself by innocuousness for man and loss of contagious power, the calf is not essential. Initial passage through the monkey undoubtedly facilitates accommodation to rabbit and calf, but chiefly, I think, by providing a large supply of fresh potent virus. It does not appear that the complete change to vaccinia can take place in the monkey as it is certain that monkeys which have responded to smallpox virus by exhibition of an exuberant rash, have given smallpox to unvaccinated attendants inadvertently admitted to their presence.

Technical details associated with the subject of passage are not suitable for discussion here. The impression it leaves on one is that in the accommodation of smallpox virus to the rabbit or calf, some element responsible for producing generalized lesions, as distinct from general invasion, is lost in the process. This element may conceivably be another virus. The change on the other hand may be simply one of general virulence-loss combined, however, with increased affinity for skin tissue. Fortunately we know that loss of virulence for man and loss of contagious power are not accompanied by loss of immunizing power. Indeed there is good evidence that vaccinia affords better protection against smallpox than smallpox does against vaccinia. What we may call the "attenuated virus" in the Jennerian or Pasteurian sense, remains the most efficient immunizing agent. It is possible, however, that variola-vaccine of known recent origin may be still more effective than vaccinia as ordinarily propagated. In any case the vaccinia variant cannot be classed with those "rough" variants of bacteria which have little or no virulence and little or no immunizing power against virulent mother strains. I should add, however, that there is no more pressing problem than that of the standardization of the immunizing powers of viruses and especially of vaccine strains. I have not referred to other animal pocks. In my opinion there is really no satisfactory evidence that sheeppox and fowlpox have any intimate relationships with variola or its variant vaccinia

that can be detected by cross-immunity experiment. They seem certainly to be more remote from the variola-vaccinia stem. While retaining their own special pock diseases, one should note that both sheep and fowl are quite susceptible to experimental vaccinia. A problem in which the detection of differences between viruses associated with similar clinical features appears to be determined almost solely by special animal adaptations, presents enormous difficulties, but when we reflect that no precise knowledge has yet been gained of the relationships that exist between the various types of tubercle bacilli, the melitensis-abortus-tularense trio and the pasteurellosis types, one need not be surprised by delay in solving a similar problem involving viruses.

I pass to another question. Jenner recognized that vaccination produced a disease syndrome exhibiting general as well as local symptoms. We now know from experimental research that the vaccine virus when introduced into the skin of animals does not merely proliferate locally, but invades the whole body, though, so far as we know. it does not proliferate and produce macroscopic lesions elsewhere than in the skin. In this respect it behaves like many pathogenic bacteria which may invade the system from a local vegetative focus. The experimental demonstration of diffusive powers possessed by vaccine virus merely illustrates what is to be expected of a living virus, but it may conceivably, to some minds, give cause for apprehension that serious action at a distance is at least possible. This point requires some elaboration. If the living virus is the only really effective means of giving immunity to smallpox over a fairly long period, and experimental results hitherto with killed virus certainly support this assumption, then it is important to be reasonably sure that in the otherwise healthy person the risk of action at a distance is minimal. I am concerned of course only with pure virus effects. Is there any evidence of such action in rabbit experiment? Vaccination by the ordinary cutaneous route demands first consideration, and here one can state with confidence that so far as ill effects or death of animals following cutaneous vaccination with calf lymph or other lymph of dermal origin are concerned, the records are practically silent and I know that in the unwritten experience of lymph-producers serious illness or death following vaccination are extremely rare events. It is necessary, however, to qualify this statement somewhat, in the light of certain phenomena which have recently been observed and recorded in connexion with the vaccination of rabbits. Some forms of virus, particularly those from cerebral or testicular sources, are liable to give rise to generalized skin lesions especially after inoculation into the vascular system. Why these types of virus should exhibit enhanced powers of generalization is not very clear, but there is experimental evidence (Reynals, 1928) that the addition of tissue extracts, testicular or renal for example, to vaccine virus has a very definite enhancing action on the virus response following intradermal inoculation. The inflammatory reaction is enormously greater, develops more rapidly, is more diffuse in character and exhibits a tendency to early necrosis.

Reynals has stated and it has also been the experience of others, including my colleagues, that occasionally rabbits may become ill and emaciated and finally succumb under such conditions. At autopsy nodular necrotic lesions in the lungs or elsewhere may be found. The view has been taken that visceral lesions of this nature are due to the vaccine virus (McIntosh, 1928), but in my opinion the occurrence of illness or death of animals showing visceral lesions at autopsy, depends solely on the stimulation by vaccinia of a latent pasteurellosis infection in the rabbits concerned. That carrier pasteurellosis infection can be so stimulated, not only by vaccinia (Pette, 1928), but also by injection of other materials such as toxins, foreign tissues, etc., appears to be well recognized, especially in American laboratories where such infections are commoner than they are in laboratory stocks in this country. I may say that I have seen at least two cases recently in which a severe snuffles syndrome appeared after vaccination and *B. lepisepticum* was

recovered from each. In the last four or five years I can recollect only one rabbit death on the fourth day following vaccination by the dermal route and, curiously enough, this animal's tissues were found to be invaded by anthrax, a virulent strain of *B. anthracis* being readily recovered. No work on anthrax was in progress at the time. These mutual interactions between viruses, particularly the stimulation of a latent bacterial infection by a virus are of great significance. I shall comment again on the matter presently but I would just say here that claims which have been made by certain writers suggesting that vaccine virus can itself produce remote visceral lesions of a necrotic character and death of rabbits, require revision in the light of these phenomena due to combined virus action.

Considerations Regarding Types of Smallpox.

So far I have dealt with recent research on the virus itself in so far as seems relevant to this discussion. I pass now to an entirely different subject, viz. the kind of smallpox we have had in this country during the past seven or possibly nine years. This Section has in the past listened to admirable accounts of the alastrim in Trinidad by Seheult (1908) and in Australia by Armstrong (1915) and Cleland and Ferguson (1915). It is, I think, rather remarkable that the continued prevalence of mild smallpox in this country has induced so few health authorities or physicians to furnish us with systematic accounts of it in all its bearings. I do not forget the excellent account given by Dr. R. P. Garrow to the Society of Medical Officers of Health in 1924, but the profession at large is not adequately informed with regard to this disease, and it ought to be. One would think from the paucity of published records of English alastrim that there was a conspiracy of silence on the subject. It is unfortunate that this should be so, as the disease is one of unusual interest. Dr. Garrow has given to it the name of *Parasmallpox* and he believes it to be a disease *sui generis*. I prefer the designation "alastrim" because, so far as I have been able to contrast what records we have of it with those of the Brazilian, West Indian, American and South African types, I have failed to note any outstanding differences. While I would use the name alastrim for convenience of nomenclature, I do not ignore the fact that the disease is a smallpox variant. Let me enumerate the experimental evidence that the virus of alastrim is a variant of smallpox. I have discussed the matter in detail elsewhere (Ledingham, 1925). It is readily communicable to the monkey by scarification. In the rabbit it produces typical vaccinia-like vesicles only after repeated passage (Blaxall, 1923, Sobernheim, 1928), but when inoculated intradermally in the rabbit, material direct from man produces a characteristic reaction which is neutralizable by anti-vaccinia serum (Ledingham, 1925; McKinnon and Defries, 1928). On the rabbit's cornea the virus usually gives a positive Paul's test and Guarnieri bodies may be found. Paschen's elementary bodies are present in smears from early vesicles, and a precipitin reaction has been obtained with alastrim material and antivaccinia serum (Gordon, 1925). The evidence from cross-immunity experiment is possibly not so decisive, simply by reason of the fact that it is difficult to secure anything like comparable conditions as to the amount of antigen employed and degree of resultant reaction, both of which must affect the immunizing response. It would seem that in experimental work vaccinia gives more certain protection against alastrim than alastrim does against vaccinia. Some point has been made of the fact that after recovery from alastrim the period during which vaccination is impossible is short. This may well be the case when we reflect on the generally benign character of the disease. I should say, however, that this has by no means been the experience of all who have performed experiments of this kind on alastrim convalescents. Doubtless variations in the extent of the alastrim lesions influence the degree and duration of the protection it affords against subsequently inoculated vaccinia. The trouble is that we are not too well informed with regard

to the amount and degree of protection which the virulent type of smallpox affords towards either vaccinia or a second attack of smallpox. We have to remember that some persons are so constituted that they do not, like the majority, respond to infection by producing an adequate immunizing response. There are many recorded instances of this phenomenon in smallpox literature, which I need not quote. It is recognized, for example, that the black races are unusually susceptible to smallpox, against which they develop only a feeble immunity on recovery.

It is difficult to evade the evidence that alastrim is a variant of smallpox. Is it a fixed and stable variant or, let us say, a mutant? I interpret what evidence we have on this point in the sense that Garrow has interpreted it, viz., that it is, in all probability, stable. When it has been alleged that case-mortality has risen to a significant percentage in a country in which it has previously been well under unity in spite of thousands of cases, I think it will be found that sufficient consideration has not been given to the possibility that the virulent form of smallpox has been somewhere incident alongside the alastrim but without definite transitions. In England we have had in recent years a few localized outbreaks of the virulent type, and it only leads to confusion to add deaths arising therefrom to the general mortality from smallpox without reservation. I note that in Calcutta in 1927 there were 3,809 cases with 2,853 deaths, in Mesopotamia (Hallinan 1928) in 1926 there were 1,062 cases with 312 deaths, in 1925 832 cases with 329 deaths, in 1924 459 cases with 235 deaths, while in the U.S.A. in 1927 there were 32,592 cases with two deaths, and in England at the time of writing (December, 1928), there occur about 200 cases a week with no deaths. My reading of Rajchman's recent analysis (1928) of smallpox incidence and case-mortality in many countries would seem to justify the conclusion at which he arrives that two entirely distinct types of smallpox prevail which apparently breed true. The virulent form has a mortality-rate which fluctuates in long waves of six or seven years' duration, between 10 and 30 per cent. The mild type on the other hand has minimal figures ranging from 0·1 to 0·3 per cent. There was no evidence that minimal periods in case-mortality of the virulent type were simply transitions towards an alastrim period with its negligible mortality. The result is important and goes at any rate some distance towards the controverting of arguments based on a reputed increase of case-mortality rates in countries where alastrim is widely prevalent.

Post-vaccinal Encephalitis.

The third subject to which I must refer, though very briefly, is that of post-vaccinal encephalitis. This has been so fully treated in the recent Vaccination Report (1928) that I need discuss here only the more salient features. These cases have shown a decided tendency to occur in groups, particularly in remote country districts. There have been instances both here and in Holland of familial cases. The incidence of such cases cannot be brought into numerical relationship with that of poliomyelitis or of encephalitis lethargica. In time they have corresponded sometimes with the autumnal prevalence of poliomyelitis and sometimes with the spring prevalence of encephalitis lethargica. The period after vaccination at which nervous symptoms begin in such cases is remarkably constant and much has been made of this feature by the very few (Jorge 1928, M'Intosh 1928, Lucksch 1927), who regard the vaccine virus as alone responsible. I take the view that the apparent constancy of this period may fit equally well with a theory which presupposes some latent neurotropic virus in the vaccinees to be activated by vaccinia. I have already said that there is no good evidence of vaccine virus producing lesions of the central nervous system unless it is inoculated directly into the brain, and that when visceral lesions have been reported in rabbits and attributed to vaccinia, sufficient cognizance has not been taken of the possibility of latent bacterial infection in stock rabbits. Further research on this particular point may throw much light on the mechanism of concurrent infection and incidentally on that of post-vaccinal encephalitis.

The histology of the nervous system is characterized by one very important lesion, that of perivascular demyelination, which is common to the encephalitis following measles, to the myelitis occurring at times in the course of antirabies treatment and to certain forms of acute encephalomyelitis to which attention has recently been drawn by neurologists (Redlich, 1927, Petto, 1928, etc.).

These latter cases are of great interest at the present juncture when we are searching for some form of acute nervous disease that might conceivably be activated by vaccinia. Neurologists inform us that neither in acute poliomyelitis nor in encephalitis lethargica do lesions of the perivascular demyelination type occur. This fact does not necessarily exclude these diseases from consideration as possibly playing a part in post-vaccinal encephalitis, as we are unaware whether the histology of these infections might or might not be modified by a concurrent vaccinia. Opinion recently expressed in the Section of Neurology on the ætiology of measles encephalitis favoured the theory of activation and a somewhat similar view has been taken of the neuroparalytic accidents in the course of antirabies treatment. It will be realized, therefore, that within a comparatively short time, attention has been focused on quite a series of myelitic or encephalomyelitic accidents incidental to virus infection, which either have escaped notice in the past or which have not until recent years secured the conditions necessary for their development

Fortunately these cases of post-vaccinal encephalitis are rare, as also are those of postmorbillar encephalitis. Practically all of them have been primary vaccinees and the great bulk of them have been children of school age. The question has been asked why a primary vaccination in infancy is apparently a protection against such accidents in later vaccinations. We have not yet sufficient data to be sure of the truth of the premiss, but if we assume its truth, the explanation may rest simply on the fact that in the allergic or accelerated reactions exhibited by partially immune subjects, the virus is precluded from invading the system and thus from inducing those nervous disturbances to which the subject might perhaps be liable if the vaccination were primary.

While we have no means of detecting *ante factum* those persons who may possess this peculiar liability, their extreme rarity cannot, in my opinion, be allowed to exercise any unwarrantably depreciatory effect on the practice of vaccination. The new experience should, however, have the effect of furthering infant vaccination and of inculcating perhaps greater circumspection and caution in the primary vaccination of older children and adults.

The Practice of Vaccination in Relation to Recent Experiences.

I think I have now dealt with the new knowledge and the new experience, and it remains for me to indicate lines on which, in my view, a discussion involving the practice of vaccination in all its bearings might usefully run.

In the first place, is the mild smallpox or alastrim which we have now, worth preventing by vaccination? Where the disease is prevalent we are told that many would rather have alastrim than vaccination. This expression of preference may indeed have some justification when the benign character of the bulk of the cases is considered, but I understand that a residuum of cases characterized by troublesome symptoms and profuse and occasionally hæmorrhagic rash would rather be avoided than not, apart altogether from the lack of risk of lethal issue. Moreover, it is recognized that an attack of alastrim may indirectly produce death in persons of feeble constitution or sufferers from other disorders. On the whole, however, alastrim must for purposes of argument be assumed to be a mild disease. Some have seriously suggested that we should "let it rip" and get what immunity we can from it in the event of the virulent form coming along again. If we had only to consider the individual victim, if that victim had only himself to blame for neglect of vaccination, if his contact victims were equally neglectful, and if he and his chain

of contact victims took all responsibility, financial and otherwise, for their past mistakes of judgment so that the present costs of hospitalization and control would fall on the appropriate shoulders, it might indeed be expedient to let the disease rip and save the public purse. Alastrim is a completely preventible yet not a death-dealing disease when it attacks the unprotected.

What counsel would be given if we had as efficient prophylactics against chicken-pox or measles, the latter a potent forerunner of pulmonary disease and even death? We have gone some distance towards the development of methods of lowering the incidence of scarlet fever and diphtheria. How many enlightened health officers are trying to reduce the incidence of diphtheria by immunization methods? If a health officer can let diphtheria rip he is not likely to give alastrim a thought, and yet he may. Why does he? It is the variola tradition. Variola has always ranked with plague and cholera as a death-dealing disease against which a civilized community must protect itself if it knows how. Consequently all the expensive paraphernalia of smallpox control and hospitalization must be maintained though the disease is practically harmless. If, in a civilized State, the isolation of contacts and hospitalization of alastrim patients are held to be indicated in the interest both of the victim and of the State, which is cheaper, vaccination and revaccination which would abolish the disease altogether, or housing and treatment of cases as they arise and contact control? Surely vaccination. If that is agreed, let us consider vaccination afresh and not as a traditional century-old policy. Our object is to protect ourselves by its aid against the risk of imported virulent smallpox and against the risk of contracting a relatively benign, but on the whole, rather loath-some disease like that now prevalent. I am assuming, of course, that this benign variant is stable.

We know that vaccination and revaccination can "deliver the goods." How are we to spread abroad its merits? I suggest that no serious attempt has ever been made to explain to all and sundry what vaccination is and what it is calculated to do. The man in the street can talk about the planetary systems of an atom, but he has little notion of vaccination except that it has something to do with sore arms and possibly a calf. We want cheap broadcast informative and authoritative pamphlets and the help of an intelligent press. I was rather impressed by a remark in an anti-vaccination organ *à propos* of a recommendation on this point in the Vaccination Committee's Report. The blue-pencilled remark was to the effect that "we do our level best to instruct the public about vaccination every month." Having explained the nature and purpose of vaccination by sympathetic educational crusade and not in the cold-blooded manner of the income-tax form, how are we to make it accept-able in the present state of affairs? The Vaccination Committee recommended that a trial should be made of vaccination in one insertion, as is done in the U.S.A. and Canada, with the proviso that the usual four insertions would be available for those who wanted them. This recommendation has been challenged in some quarters as tending unnecessarily to lessen the general level of immunity resulting from vaccination. Doubtless the amount and duration of the immunity resulting from one insertion will be less than that from four. Further experimental data on this point are desirable.

To compensate for any reduction, revaccination is recommended at school age and again on leaving school. The experience with regard to post-vaccinal nervous sequelæ, rare though these be, offers a strong argument for the recommendation of infancy as the time of election for primary vaccination. I feel also, though we have no definite knowledge on this point, that in the light of these nervous sequelæ it is expedient to reduce the dose to one insertion, especially for primary vaccination at school age, on the chance that there may be some correlation between the dose of vaccine virus and the degree of risk of some latent parasitism being stimulated to activity. I believe also that reduction to one insertion must

indubitably diminish the risk of septic troubles, which, with all their diversity of origin and responsibility, constitute the bulk of post-vaccinal sequelæ. The limitation of vaccination to one insertion can only be fully justified if revaccination in similar fashion and at the time of leaving school is made a matter of routine practice.

One other point remains for discussion, and being concerned with politics, it was not one on which my Committee was invited to express an opinion. On the other hand it might be usefully debated whether compulsory vaccination, such as it is or is not, under the Acts, is justified under present conditions and whether vaccination may not be likely to meet with much fuller acceptation if placed on a purely optional basis and introduced to the people at large with the accompaniment of a sympathetic and informative propaganda. I have been considerably impressed in this connexion by the educational crusade which Dr. Kinloch, now of the Department of Health for Scotland, instituted some five years ago in Aberdeen, and by the success which his modified vaccination procedure, comparable in essence to that advocated in the Vaccination Committee's Report, has achieved. The percentage of conscientious objectors in that city is only eight. By concentration on the education of the public as to the value of vaccination, and by removing what compulsory elements remain in the Vaccination Acts, it is possible that we might secure a much larger total of voluntary vaccinations and revaccinations throughout the country. In conclusion, with the probable transference in the near future, of vaccination control to the health authorities in the country, I anticipate that the environment for effective immunization against smallpox will become far more favourable and will, if accompanied by the frankest propaganda, in no way be detrimental to the success of other immunization procedures such as those directed against diphtheria and scarlet fever.

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Colonel F. E. Fremantle (President) said that it had been with a view to assisting Parliament and the Minister of Health in devising any alterations of policy and procedure that immediately on taking up the Presidency of the Section he had asked the Council and Secretaries to arrange this discussion.

He welcomed the large and representative audience of epidemiologists present, and hoped that the discussion would point the way, if not to a certain policy, at any rate to certain conclusions in their proper proportion with a view to the development of a policy. The alleged hesitation of medical officers and practitioners to speak freely of drawbacks in the present procedure arose not so much from lack of courage as from a deep-rooted sense of the important issues involved. The lay press, in general, was naturally inclined to exaggerate and distort any statements made with regard to defects in the present procedure. Thus, without the balancing factors, truth was distorted, and the half truth was seized on by those who, with prejudiced views, wished to inflame public opinion against compulsory vaccination, regardless of the alternative dangers.

In reference to the possibility of abolishing compulsion and putting vaccination against smallpox on the same footing as other preventive measures, attention

must be paid, not only to the immediate and ultimate effects on individuals and on the general population, but also to the influence which such a revolutionary change would have on international relations, on our commerce and trade, if once it were known that this country had abandoned even a defective system of compulsory general vaccination.

Dr. C. Killick Millard said he agreed that the present strain of smallpox was a distinct and separate entity from old-fashioned smallpox, and he felt strongly that the time had come when the difference ought to be officially recognized. The present position was most unsatisfactory. What value would posterity attach to our smallpox statistics when we lumped together in one column, under one heading, two different affections, one having a fatality of from 10 to 30 per cent., and the other having practically no fatality at all? He much preferred the nomenclature suggested by Dr. R. W. Jameson, viz., "variola major" and "variola minor," to the terms "smallpox" and "alastrim." It mattered not whether they regarded variola minor as a "variant" of smallpox or a "mutant"—all the evidence pointed to the difference being fixed and permanent. There was no evidence forthcoming that the one strain ever merged or changed into the other.

He doubted whether Professor Ledingham was justified in speaking quite so definitely about the efficacy of vaccination. Vaccination and revaccination, performed often enough, would certainly protect the individual, but as applied to the community it would fail, for the reason that it was not practicable to keep all the individuals in a community effectively protected.

Seeing that variola major was virtually banished from the country, that variola minor was a comparatively trifling disease, and that a newly-recognized additional risk attached to vaccination, namely that of serious disease of the central nervous system, he did not believe that compulsory vaccination could be any longer justified.

Sir George Buchanan said that Professor Ledingham had done good service in bringing together these different considerations closely affecting the practice of vaccination and the question of the administrative measures which should be taken to protect against smallpox by vaccination at the present time. It was right that all these considerations should be taken into account.

During the last decade, many new facts relating to smallpox and vaccination had emerged, as well as knowledge which, though not perhaps new, had assumed quite different proportions. He fully agreed that nearly all these facts called for additional work in the directions suggested. We had need, for example, of further studies and better teaching about the clinical character of the alastrim or mild smallpox, which was characteristically occurring in this country, in America, and elsewhere. Further research was required into the pathological processes associated with vaccination, including the way in which the vaccinia virus remained stored in the body or could be recovered from it, and the length of time which it remained there. Another matter for research was the inter-relation between the different animal viruses. Problems such as these were universal and much advantage could be gained by securing some mutual understanding between workers in different countries on the lines of their research and by their interchanging material. Something in this direction had been proposed recently by the Smallpox and Vaccination Commission of the League of Nations, and it seemed to him an important piece of coördinated work to do. The League of Nations Commission had already done some useful work by the collection of data regarding post-vaccinal encephalitis in different countries which was published from Geneva in its report issued in August, 1928. We still required knowledge on the meaning of the apparently localized distribution of post-vaccinal encephalitis. If it had been limited to certain countries, and to certain regions of those countries, because it was only there that the latent encephalitis

virus infection had been present to be activated by vaccination, its occurrence could be looked on as perhaps only an exceptional and passing incident in the history of vaccination, not as a permanent complication which, though rarely occurring, must always be allowed for. On the other hand, it was possible that post-vaccinal encephalitis could be found in some countries from which hitherto it had not been reported, if only a better and more systematic search was made. The Geneva Commission was endeavouring to secure such intensive inquiries in suitable regions, especially in Scandinavia, and results should be instructive.

The occurrence, though rare, was important in relation to Professor Ledingham's thesis that vaccination should be promoted primarily by convincing the individual. It should not be difficult to persuade people to have their children vaccinated at an early age and to repeat the process at suitable intervals if the operation itself and its results caused so little inconvenience that no one would think twice about them. The argument in favour of such vaccination, even as a preventive of alastrim or mild smallpox, would have weight, and it must be remembered that invasions of the severe form of variola were by no means infrequent and that people were at any time liable to travel in countries where it was prevalent. The real difficulty in relying on convincing the individual was more likely to lie in answering the question, What assurance could be given that vaccination was free from risk ? From this point of view, post-vaccinal encephalitis should continue to be investigated from all points of approach.

Dr. S. Monckton Copeman said that, dealing more particularly with the scientific aspects of the subject, he was naturally interested to learn that Professor Ledingham had been able, in all respects, to confirm his (Dr. Copeman's) pioneer work carried out many years ago, at a period when many of the advantages of modern laboratory technique were non-existent.

When working at the question of the bacteriological purification of vaccine lymph, which resulted in the devising and introduction of the anti-smallpox vaccine which he had termed "glycerinated lymph," he had realized the necessity of using laboratory animals—if any were found to be susceptible to smallpox or vaccinia—for testing purposes. Eventually he discovered that monkeys (*rhæsus*) could, unfailingly, be inoculated both with smallpox of human origin and with vaccinia, by means of skin inoculation. Rabbits, on the other hand, could be vaccinated on the denuded skin, but could not, apparently, be variolated. Coloured drawings illustrating the results on the skin of the monkey's arm of both variola and of vaccinia were published at the time in the *Journal of Pathology and Bacteriology*.[1]

Attempts to convert smallpox into vaccinia by passage through the calf alone having failed, the monkey was then used as intermediary, with the result that a number of strains of variola-vaccine were obtained, which afforded excellent results *quâ* vaccinia, on children, and from which the Government stock vaccine was regenerated on several occasions. Definite information as to the inter-relationship of variola and vaccinia was thus obtained for the first time in this country, and the results of this investigation were reported to the Royal Society in 1902.

With regard to the origin and prevalence of cowpox, Professor Ledingham had laid stress on apparently divergent views of Reece (1922) and Male (1927). But these were probably to be accounted for by the fact that this disease, like smallpox, tended to recur in cycles. In his (the speaker's) opinion the special prevalence of cowpox in Jenner's day could be explained by the fact that variolation as a protection against ordinary smallpox was much in vogue at the time, while the fact that milking was carried out by men who also worked in the stables, might account for the simultaneous appearance of horse-pox and cowpox. That true cowpox was identical with vaccinia he had not any doubt, and he had always given special attention to the scientific demonstration of this point in connexion with a number

[1] "Variola and Vaccinia: Their Manifestations and Inter-relations in the Lower Animals." A Comparative Study. May, 1894.

of outbreaks of cowpox as to which, from time to time, he had had to make official inquiry during his service in the Medical Department of the Local Government Board and Ministry of Health.

In default of a satisfactory killed virus being available it was still necessary to use the living virus for purposes of vaccination, with the very occasional result of inducing an attack of generalized vaccinia (in which it was possible, by appropriate methods, to demonstrate the presence of the living virus in the secondary vesicles) or, as had been seen during the past few years, of waking into activity some latent affection of the central nervous system, resulting in what is known as post-vaccinal encephalitis. Generalized vaccinia must be regarded as the direct effect of multiplication of the living virus in infants possessed of but slight immunizing capacity. On the other hand, with regard to post-vaccinal encephalitis, no definite evidence had thus far been obtained pointing to the vaccine lymph, as such, being directly responsible. Indeed, as indicated by Professor Ledingham, the evidence obtained from experiments on animals seemed to point in the opposite direction. It was improbable that any such accessory virus, even if present in the lymph, as collected, would be able to survive the continued action of the glycerine with which the vaccine lymph was incorporated; but, so far as he was aware, experimental evidence on this point was not available.

With reference to the kind of smallpox that had come under observation in this country during the past few years, he (Dr. Copeman), as a member of a small Departmental Committee, had had exceptional opportunities of studying its ætiological and clinical manifestations. In addition he had made official inquiry into certain isolated outbreaks of the disease, including one of somewhat similar characteristics at Cambridge so long ago as 1903. Some account of this outbreak and of a later one at Beccles, in Suffolk, was given in a paper read by him before this Section in June, 1910; as well as in an official report to the Ministry of Health, entitled: "The Relationship of Smallpox and Alastrim."[2] In that report he definitely expressed the opinion that this mild disease, notwithstanding certain clinical divergencies, in reality constitutes a variant of smallpox, and, from the practical point of view, must be treated as such.

With regard to administrative measures in connexion with vaccination, these had, in his opinion, always suffered from having originally, and necessarily, to be entrusted to the Boards of Guardians with their Poor Law association. It was all for the good in this respect that, under legislative enactment, transference to the health authorities would shortly come about throughout the country, thus bringing vaccination into line with other measures for prevention of disease.

Sir William Hamer said: In considering whether the occurrences of vaccinal encephalitis in 1922 and 1923 were fortuitous or not, it is obviously desirable that the tests applied to "control populations" should be so far as practicable identical with those applied to "recently vaccinated children." The Andrewes Committee tried to effect this by obtaining death returns from the Registrar-General, and they consulted Professor Greenwood, who analysed the returns, but could merely deduce therefrom that the observed excessive incidence upon "recently vaccinated" over that upon "controls" was "moderately improbable." Recourse was, thereupon, made to cases (as opposed to deaths), but here it was not possible to obtain the particulars respecting the "controls" that had been obtained, by *special inquiry*, in the "recently vaccinated." The question is discussed on pp. 102, 119 and 170 of the Vaccination Report, and against the cases of "encephalitis" (which has the wide connotation defined in a footnote on p. 98) first poliomyelitis and polio-encephalitis and then later encephalitis lethargica are placed; but while, on p. 170, it is further admitted that "other acute nervous disease" should also be added, precise particulars concerning the last named were not available. Thus, the tests applied to the two populations still differed materially. Indeed, if

[2] Appendix II to the Chief Medical Officer's Report, 1919-20.

in addition to this consideration, due account be taken of age-incidence, of seasonal prevalence and of the unusual manifestations of both cerebro-spinal types of influenza and smallpox, in the areas under consideration (in November-December, 1922, and June-August, 1923), the case becomes a very strong one for regarding the small number of cases of so-called " vaccinal encephalitis " as merely instances of fortuitous overlapping (in time) of vaccination with prevalence of disease attributable in the main to the familiar neurotropic virus of influenza. No importance can be attached to an argument which has been based upon " incubation period," for the reason so carefully set out in the Vaccination Committee's Report (see p. 120 and elsewhere).

Dr. S. P. Bedson said that with regard to the problem of finding a better method of prophylactic vaccination against smallpox, it would be difficult to popularize a method of vaccination which was attended by even a minimal number of fatalities. It had been shown in the case of other diseases due to filtrable viruses—foot-and-mouth disease, distemper, yellow fever and fowl plague—that a degree of immunity could be conferred by the inoculation of a formalized virus, and further, that this partial immunity could be consolidated by the inoculation of living virus. He (the speaker) thought that a procedure consisting of two inoculations, a formalized or otherwise treated vaccine virus, followed later by the inoculation of active vaccine virus, might prove an effective prophylaxis against smallpox. Experimentation along these lines should be actively pursued.

Professor Arthur Ellis said he strongly supported the suggestion of Dr. Bedson that experimental work should be directed towards improvement of the method of prophylactic immunization against smallpox. As one who had seen a number of cases of post-vaccinal encephalitis, including four deaths from the condition, he spoke with feeling of the seriousness of this tragic, though fortunately rare complication. The condition had been recognized at the London Hospital as a definite clinical and pathological entity as long ago as 1922. It was now more than six years since the occurrence of such cases had been established, yet no important modifications in vaccinal procedure designed to obviate these fatalities were in use. The importance of the problem was further enhanced by the fact that there appeared to be an unfortunate tendency for vaccination to become more an individual than a universal procedure, a tendency which, if developed, would necessarily lead to more primary vaccinations at school age. It was in such circumstances that post-vaccinal encephalitis had been shown to be most likely to occur. He hoped that, as a result of the meeting, some public authority would be stimulated to originate or support work on improved methods of vaccination along the lines suggested by Dr. Bedson. The suggestion was particularly apposite in view of the recent results in immunization with killed or modified virus in other diseases, notably foot-and-mouth disease, distemper and yellow fever.

Dr. R. P. Garrow said that with regard to the absence of systematic clinical descriptions of so-called " mild smallpox," he agreed with Dr. Ledingham's suggestion that there appeared to be a conspiracy of silence in this matter. It did not pay the medical official to express views which were not strictly orthodox. Dr. Ledingham had come to the conclusion that so-called " mild smallpox " or alastrim was a *fixed variant of smallpox*. He (the speaker) entirely agreed with this view which he regarded as the dualist conception of smallpox.

He was satisfied that alastrim was not a disease worthy of the preventive measures appropriate to smallpox. He (the speaker) had been completely cured, after his experiences in Chesterfield, of any opinion he had ever held as to the value of compulsory vaccination.

He strongly disagreed with Dr. Millard's contention that vaccination protected the individual from smallpox but did not protect the community.

Dr. R. W. Jameson said: Variola major (classical smallpox) and variola minor (alastrim, etc.) are separate, stable, and easily distinguishable forms of smallpox with marked differences of virulence. The central authority refuses to subdivide smallpox with the result that: (1) The public is coming to think all smallpox trivial; (2) differing treatment suitable to the two forms is not considered, much less adopted; (3) lack of definition results in frequent alarmist reports, that variola minor is becoming more severe. Time always proves these to be groundless.

Post-vaccinal encephalitis causes an appreciable number of deaths, though its causation is obscure. It is admitted that in the post-vaccinal encephalitis cases encephalitis would not have developed if the patients had not been vaccinated, therefore post-vaccinal encephalitis deaths must be attributed to vaccination. Variola minor is a disease without a death-rate (the Registrar-General's death-rate is admittedly misleading); there is therefore no justification of the use of vaccinia as a protection against it, for vaccinia, though rarely, does cause death.

Infant vaccination is advocated because fewer cases of post-vaccinal encephalitis have been reported in infants than in those of older years. It would have been surprising had other been the case, seeing how great is the difficulty in the diagnosis of disease in infants; to determine whether vaccination is, or is not, deleterious to infants it would be necessary to include the vaccinal condition on the death certificates of those under one year of age and analyse the results.

General vaccination and revaccination is not a practical possibility in this country; its advocates seem to forget how ably variola major outbreaks are controlled, how few they are, and therefore what a ridiculous insurance proposition, at the present time, in this country, is non-epidemic vaccination against variola major.

The orthodox do not know enough about the infectivity of smallpox, about the protective powers of vaccinia, or about its dangers, to justify them in continuing to press their views on the community; there is an obvious need for the issue of a questionnaire on what actually does happen to vaccinated and unvaccinated contacts: we should not remain content with the inconclusive though suggestive hospital returns of vaccinated and unvaccinated cases.

One-mark vaccination of contacts and possible contacts, of variola major, a dangerous disease, is to be commended; the dispersion of energy entailed in times of epidemic in the attempt to confer a lasting immunity by vaccination on the general community is to be deprecated.

Dr. Carr's Presidential Address to the Medical Society of London in October, 1928, gave such an eminently common-sense view of vaccination that one hopes it may reach and influence the stereotyped.

Professor Major Greenwood said: As a teacher of epidemiology I am naturally interested in this subject, the history of which does not, perhaps, do much honour to our sense of logic or candour. Even to-night one has heard strange doctrine. Dr. Killick Millard has said that in the world in which we live vaccination may protect the individual but not the community. His reasons are (1) that it is not possible to vaccinate efficiently 100 per cent. of the community. (2) That smallpox modified by vaccination may pass undetected until, in the unimmunized, serious cases have occurred. Hence, on balance, the advantage of the *individuals* may be the detriment of the *group*. Dr. Garrow is obviously entitled to object, on material grounds, to the premises, but if he accepts the premiss, he has not, I think, any right to dissent from the conclusion. What he ought to have said was that Dr. Millard's conclusion would not have followed from premises which Dr. Millard had specifically excluded. Indeed, quite apart from this controversy, it is surely a truism that the properties of a *group* are often in fact usually different from the sum of the properties of the *individuals*, or even of the subgroups making up the group.

Taking the historical controversy as a whole, I suggest that both parties to it have attempted and still attempt to defend the indefensible.

Take the anti-vaccinist case as presented by that great man, the late Charles Creighton. It was essentially this :—

(1) That Jenner's "classical" paper was very poor stuff, and its author of doubtful veracity. (2) That it was ridiculous to try to "protect" against one "disease" by means of the products of another "disease." (3) That the *prima facie* advantages of vaccination—as said to be demonstrated by statistics—could all be explained in some other way. The whole argument was to show that vaccination was worthless for the *individual*.

But, with respect to (1) and (2), laboratory work done since Creighton's day has surely proved that, even admitting Jenner to have guessed, we must agree that his was a lucky guess, for the cross-immunization results of many workers show that there is a very close biological affinity between "vaccinia" and "variola." Hence, although Creighton was fully justified in using his arguments, as he was fully justified in putting forward a theory of plague *now* obsolete, nobody is now justified in using them. With regard to (3), I do not think I am likely to over-rate the cogency of medical statistics, but I do feel that the evidence in favour of the view that, given equal conditions of exposure to risk, the attack-rate of the vaccinated exposed is less and the fatality-rate of the vaccinated exposed greatly less than the corresponding indices for the unvaccinated, is so strong that it should carry conviction. It will be found, I think, that other explanations of the *prima facie* facts violate flagrantly the law of parsimony.

On the orthodox side, I think, the fallacy has been committed of generalizing from experience of individuals or subgroups, and, claiming that because individuals or subgroups are advantaged, *therefore* the group history of smallpox has been modified. Again, it has been suggested, if not actually asserted, that without vaccination smallpox *must* ravage a community. Neither proposition can be established. The epidemiological history of the nineteenth century has not only been written tendentiously, but we really do not know enough about the laws of herd-immunity to interpret the facts. Suppose we maintain under constant environmental conditions a herd, n per cent. of whose members are immunized against a particular disease, what will be the epidemiological history of that disease in the community? Only for the case $n = 100$ can we answer. Years of experiment will be required. I do not believe that we know enough to lay down the law about the epidemiology of smallpox, even under constant environmental conditions, *a fortiori* we cannot lay it down for changing conditions.

Lastly, as to change of type. I think that those who, like myself, believe in the individual value of vaccination, have felt this difficulty: May not the present epidemic constitution of variola change so suddenly that, in a largely unimmunized community enormous havoc may be made before widespread protection can be given? It will be remembered that—in some important respects—the character of influenza did change suddenly in 1918. It will also be remembered that the scarlet fever of the first quarter of the nineteenth century was very different from that of the second quarter. My own feeling is—but the subject surely deserves investigation—that in retrospect we exaggerate the rapidity of these changes of type and that, always provided that the means of adequate vaccination can be mobilized without delay, the danger is not serious.

Dr. Knowsley Sibley said that as a dermatologist he was frequently asked to diagnose rashes in infants, which the mothers attributed to vaccination, and some undoubtedly were due to that cause, though many were not.

He had never been able to convince himself that there was such a condition as "generalized vaccinia" nor could he see why there should be, if vaccinia had been inoculated, as cowpox was a local disease of the udder, and not a constitutional disease like variola.

He was surprised to hear Professor Ledingham dwelling on the often published statistics supposed to prove that two, three, or four vaccination pustules formed a progressive immunity for smallpox. For on the same principle, one chancre would not give rise to as virulent an attack of the great-pox (syphilis) as two or more chancres, if such were possible, which was not correct.

He had always been interested in the vaccination question, since the time, about 1888, when he was working in the laboratory at King's College under the late Professor Edgar Cruickshank, when he was writing his books on "The History and Pathology of Vaccination," in which he had summed up the position as follows: "though in this country, vaccine lymph is generally taken to mean the virus of cowpox, the viruses in use have been derived from several distinct and severe diseases in different animals." Would Professor Ledingham inform him as to the origin of the lymph as supplied by the Lister Institute; was it from cowpox, horsepox, sheep-pox, cattle plague, or from variola itself, and were we after all variolating and not vaccinating the public?

Dr. J. A. H. Brincker, representing the Public Health Department of the London County Council, said he believed the present existing type of smallpox to be a fixed variant of a mild type which was not likely to alter its character so as to become again a severe disease. Many of the cases now occurring were so mild that, but for the existence of previous cases, they would be missed, even by expert diagnosticians. Was vaccination as a means of controlling the present type of smallpox possible or even worth trying? London at present was an unvaccinated community; more especially was this true of the child population. Accurate figures were not available, but at least 75 per cent. of school children were unvaccinated. Even in those schools where smallpox cases occurred it was difficult to get the contacts vaccinated, and it was true, as Professor Ledingham had stated, that the majority preferred an attack of the present smallpox to an attack of vaccinia. This was partly because of the mildness of the disease, but partly also because the English working man had the inveterate betting instinct and so would prefer taking the sporting chance of escaping the disease, even though he might have been a close and continuous contact of a previous case. As Professor Ledingham had said—it was the variola tradition which made us adhere to this method of attempting to prevent smallpox. Since we would never think of advising vaccination when varicella was prevalent, why resort to vaccination to prevent the present type of smallpox, which was at least as mild and free from dangers as chickenpox? It would be time enough to recommend vaccination when the severe or Mediterranean type of the disease was introduced and implanted upon the present type, which was likely to remain with us for a long time to come. The medical officer of health of a London borough, although primarily responsible for the control of smallpox as one of a group of communicable diseases, had no control over vaccination and had no authority over the public vaccinator in his district. The medical officer of health of the London County Council had even less authority than his colleagues of the metropolitan boroughs. The functions of the L.C.C. medical department in connexion with smallpox control were both limited and obligatory and could be stated under three headings, namely:—

Firstly, in regard to diagnosis.—For years past the Council had by arrangement provided an expert in smallpox diagnosis on its staff, and for these reasons. Variola was up to recent date a disease of rare occurrence in London, in fact the majority of physicians in London had never had the opportunity of seeing a case of the disease; they could, therefore, not be expected to recognize it; hence the necessity for providing a consultant who was expert in the diagnosis of variola and had the variola viewpoint foremost in his mind. For various reasons it had been considered advisable that the diagnosis of a suspected case of the disease should be confirmed before the machinery of removal to hospital was put into action—it was more

satisfactory for the patient and more economic for the community. The Council's expert was available for the whole of London and consultations might be arranged with him through the medical officers of health of the boroughs. This scheme of consultation had generally given such satisfaction that the health officers in adjoining counties were, by special arrangement, also availing themselves of his services.

Secondly, the L.C.C. was a centre for the distribution of information.—By an arrangement made with the twenty-nine metropolitan borough medical officers of health and those of the County Councils covering a large area round London, the medical officer of health of the London County Council acted by agreement as an agent for the collecting and distributing of information regarding smallpox cases and their contacts. Reports were furnished on every case of smallpox as soon as it became known and the information was forwarded to all the health authorities in the prescribed districts in and around London. The presence and distribution of variola were, therefore, known to the health authorities, who could take the necessary action to prevent its spread.

Thirdly, the school medical officer controlled the spread of communicable diseases in schools.—In addition to inspecting and supervising scholars, the medical officer was ready to assist the public vaccinator to carry out his duties more effectively. When parents so desired, he arranged for the public vaccinator to vaccinate groups of children on the school premises, and he would provide the assistance of school doctors or school nurses ; after vaccination a school nurse would keep the children under observation and deal with them as cases of minor ailments, requiring inspection, dressings, etc., until they were fit.

Since May, 1928, when variola first affected the elementary schools in London, public vaccinators had performed about 6,000 primary vaccinations in about thirty-six affected schools, all with satisfactory results.

In January, 1929, one death from post-vaccinal encephalitis was reported in a school child, who was one of 106 cases vaccinated from the same vaccine and of 149 primary vaccinations carried out in the same school—the remaining 148, including a brother, had shown no untoward symptoms after vaccination.

Professor Ledingham (in reply) said that the discussion had not been particularly useful in eliciting concrete proposals that might guide future policy. For some speakers, post-vaccinal encephalitis dominated the situation, and informative propaganda was doomed to failure in face of this risk. Others considered alastrim not worth preventing by vaccination, while no one, except possibly the Chairman, was in favour of compulsory vaccination. Alternative measures, such as costly hospitalization, apparently troubled nobody. In fact, the general impression was that vaccination might now be safely abandoned. Should virulent smallpox reappear, Professor Greenwood would have it that little damage would ensue, provided the machinery of vaccination was quickly mobilized. It was a relief to have his assurance that vaccination protected against both risk of and death from smallpox. In his (the speaker's) view it would be disastrous to destroy what continuity still remained in the practice of infant vaccination. Informative propaganda to secure the highest percentage of infant vaccinations was urgently needed; the risks could be declared minimal. Abandonment of infant vaccination would only intensify those risks from post-vaccinal nervous sequelæ should a period of virulent smallpox necessitate wholesale primary vaccinations at all ages. It was highly probable, though not proved, that the activation mechanism in post-vaccinal encephalitis depended on the living nature of the vaccine virus. While every effort should be made to secure an efficient killed virus, or some modification of procedure like that recently advocated for distemper prevention, experimental investigations towards this end had not been particularly promising.

Section of Laryngology.

[*December* 7, 1928.]

The Use of Ultra-Violet Rays in Diseases of the Nose and Throat.

By Albert Eidinow, M.B., B.S.

(*From the National Institute for Medical Research.*)

The biological action of light is primarily due to photo-chemical reaction. Waves of light are absorbed by living cells and produce photo-biochemical reactions which influence the whole organism. All living cells are sensitive to the ultra-violet rays from 3100-2500 A.U.; furthermore, the living cells can be made sensitive to the infra-red, visible and long ultra-violet rays, by the addition of certain dyes and chemical substances called "Light sensitizors." When a normal tissue, such as the skin, is exposed to a source of light emitting numerous waves of light, each group of waves penetrates tissue to a different depth and has a different biological action. The near infra-red, visible red and yellow rays penetrate beyond the blood-vessels in the skin and heat the tissues, thus producing vasodilatation and hyperæmia. The green, blue, violet and long ultra-violet rays penetrate to a smaller extent and are absorbed by the hæmoglobin pigments of the cutaneous blood. The long ultra-violet rays excite a greenish fluorescence of the skin and are selective in their behaviour to the light sensitizors. The ultra-violet rays from 3100-2500 A.U. penetrate to the superficial layers of the living epidermal cells. They excite a green fluorescent light in the horny layers of the skin, and, having a rapid lethal action on the living cells, produce a characteristic erythema of the skin at the site of irradiation.

The erythema dose of light produces a marked local hyperæmia and excites a local white cell infiltration of the epidermal tissues. The endothelial cells are swollen and enlarged and the transudation of fluids through the walls of the capillaries is altered. The exudation of plasma produces an intradermal œdema of the tissues at the irradiated area. The minimal erythema dose of ultra-violet rays applied to the skin, the peritoneum, abdominal viscera, or subcutaneous tissues, increases the bactericidal properties of the shed defibrinated blood tested *in vitro*. The maximum effect is obtained by irradiation of 20-30 sq. cm. per kilogm. weight. This bactericidal effect originates locally at the site of irradiation and is carried by the blood-stream into the general circulation. These results support the theory that ultra-violet rays have a photo-biochemical action on the living cells and produce a substance which has bactericidal properties. The local effect of ultra-violet rays is to increase the immunity of the skin; this may explain the beneficial action of the local irradiation of lupus lesions and is due to the increased leucocytic infiltration and the production of a bactericidal photo-biochemical substance.

The Copenhagen Light Institute has developed the use of the short-flame carbon arc and has successfully treated many cases of lupus vulgaris of the skin and mucous membranes. The ultra-violet rays emitted from this source are mostly long rays. In this technique the whole surface of the body is exposed to light for twenty minutes to two hours twice or three times a week. It is only suitable for chronic types of disease and in non-febrile conditions. Although many excellent clinical results have been obtained by various clinicians, the experimental evidence obtained from the observations of the immunity of the skin following irradiation, and the study of the hæmobactericidal response to light, suggest that overdosage and excessive exposure have been frequent. The application of an erythema dose of rays renders the irradiated area immune to further reaction for three to four days; this is due to the protective screening action of the exudates in the inter-epidermal tissues. During the stages previous to desquamation the skin is

still immune to radiation, since the dead epidermal cells screen off the biological active rays. During desquamation the exposed epidermal tissue is hypersensitive to light and an irregular action results, demonstrating immune and hypersensitive areas. From these observations it seems right that a period of from ten to fourteen days should elapse between the exposures of any given area of skin. In this way the skin can be kept sensitive to ultra-violet rays for a long period and excessive heating of the body by prolonged exposure is avoided.

The Finsen Light Institute at Copenhagen claims as good results as Rollier obtains by open air and sunlight. The possible defects of the short-flame arc technique are that: (1) The area of skin exposed is too large. (2) The skin is rendered immune to ultra-violet rays. (3) The body is overheated by prolonged exposures. (4) It is unsuitable for febrile diseases. (5) It is laborious and expensive.

From these observations I have adopted a special technique of general treatment, with lamps emitting ultra-violet rays shorter than 2970 A.U., which I have called "short-ray" therapy. (1) The source of light is a quartz mercury vapour lamp, and at later stages, when pigmentation has developed, metal-cored thirty-ampere carbon arcs. (2) The surface area of skin exposed is roughly equal to one-sixth of the surface area of the body. (3) An interval of from ten to fourteen days is allowed between irradiation of any given area of skin. (4) Irradiation is conducted two or three times a week. (5) The minimal erythema dose is applied and the skin is kept in a "light sensitive" state during the whole period of treatment.

The "general" irradiation of the normal skin improves the health and body resistance. Early cases of lupus of the skin, naso-pharynx, palate, and larynx have been healed by these means. The cases in which chronic ulceration and hard scar fibrous tissue are present are more resistant. Many catarrhal and inflammatory conditions of the mucous membrane are due to general failures of health, associated with a secondary anæmia and vasomotor disturbances. Light has often proved a valuable help and has improved these conditions. The healing of septic wounds and the repair of granulation tissue are accelerated by general irradiation. In cases of subacute disease and acute inflammatory conditions the cautious use of irradiation of the skin may be helpful. In such conditions the application of the minimal erythema dose and exposure of a small area of skin to light is a safe technique. General light treatment has been successfully employed as a post-operative measure and accelerates the healing of septic wounds and young granulation tissue.

The "local" treatment of a lesion with correct dosage of light accelerates repair. Some early investigators have maintained that a direct bactericidal action was obtained with the water-cooled Finsen-Reyn arc lamp or the mercury Kromeyer lamp, but this is an erroneous impression, for serum and lymph greatly impede the penetration of the bactericidal rays. Excessive dosage produces necrosis of the tissues and extensive œdema, which may cause a spreading infiltrating cellulitis. This applies mainly to subacute conditions when the granulation tissue is vascular, but it is uncommon with chronic fibrous ulceration, such as is seen in old-standing conditions of lupus vulgaris. The careful dosage in local irradiation, producing a mild erythema reaction and avoiding excessive damage to the tissues, will hasten the healing and repair of many types of septic wounds.

The normal mucous membrane is about three times as sensitive as normal white skin to light. Many cases of tuberculous disease of the mouth, throat, or nose can be healed by general light treatment of the skin; as a rule they react more favourably and quicker than the skin, since the granulation tissue is more vascular and ulceration does not contain hard, old, fibrous tissue. Strandberg has claimed good results in the treatment of tuberculous disease of the larynx. Local treatment of the mucous membrane accelerates repair and regeneration of granulation tissue. The application of local light through the mouth to the pharynx and larynx

necessitates an apparatus by which the effects of radiation are rapidly obtained. Irritability and pain are prominent symptoms and only a rapid form of treatment is practical.

The use of quartz rods and applicators to transmit the light to the site of infection is unsatisfactory as the intensity of radiation is reduced and the apparatus becomes cumbersome and impossible to use. The same criticism applies to the use of reflected light from mirrors. The water-cooled throat lamp is based on the same principles as the water-cooled Kromayer mercury lamp, but it is small enough to be readily inserted into the mouth, pharynx and naso-pharynx. The intensity of the ultra-violet rays emitted is sufficient to produce an erythema reaction after 60 seconds' exposure of the mucous membrane. A quartz envelope surrounds the burner and can be easily removed to clean the lamp.

Local anæsthesia with eucaine and cleansing of the irradiated area are necessary before light is applied to the mouth. The reactions of the mucous membrane are similar to those seen when the skin is irradiated; erythema and œdema are evident and massive exposure produces necrosis and blistering of the mucosa. Since the mucous membrane is more sensitive to light than the skin, the dosage must be regulated with great care. Excessive dosage may cause a spreading infiltration and acute inflammation which will produce a general reaction of the body with increased temperature and malaise. In tuberculous disease of the larynx and palate, the lungs are often involved and the irradiation of the local lesion in the mucous membrane may cause reaction of the lung infection. Careful supervision of the lung is imperative. The irritability and pain associated with ulceration of the lesions are rapidly diminished after treatment.

Local treatment is applied every ten or fourteen days. The combination of exposure of areas of the normal skin to radiation twice or three times a week and local treatment every ten or fourteen days to the lesion have proved successful in resistant cases of lupus and tuberculous disease of the palate, pharynx and larynx.

Further research on treatment by means of sensitizors and ultra-violet radiations emitted by high-frequency current are in progress.

Discussion.—Mr. H. BELL TAWSE (President) said that two or three years previously one of these lamps had been tried in Nottingham, and he did not think that any harm had been caused by its use. If, however, the erythema occurring on the skin were to occur owing to improper applications to the mucous membrane, that might be a serious matter. He himself had had applications of the rays, and was much better after erythema had been caused than when it was not. Pain from herpes zoster was relieved after four applications of the rays, when every other measure had failed. He would like to know whether, in using the rays, it was better to cause erythema or to stop just short of it.

Mr. SOMERVILLE HASTINGS said that at Copenhagen he saw the excellent results which were obtained in cases of tuberculosis of the larynx. There, he believed, the applications were made to the whole surface of the body every second day, a procedure which did not accord with Dr. Eidinow's theory. An exposure only every fourteen days could hardly produce results better than those seen at Copenhagen.

Mr. O. POPPER said that when house-surgeon to the President, he had been instrumental in installing an ultra-violet ray outfit (Kromayer quartz mercury vapour type) in the Ear, Nose and Throat Department of the General Hospital, Nottingham. He was warned of the harmful effect of over-dosage on the comparatively delicate mucous membranes. Accordingly, it was used with care and with good effect. He understood that, in applications to the larynx, a shield to protect the fauces was used, while getting as much irradiation on to the larynx as possible. No attempt was made to estimate the quantity of rays reaching the larynx. That did not seem a scientific regulation of dose. Would it not be better to have direct laryngoscopy, and use a lamp of known power?

Mr. NORMAN BARNETT said he had used mercury vapour and Kromayer lamps for both local and general applications. The rays were applied every other day, and there had been no

bad results owing to inflammation of mucous membrane or skin. From the aspect of disease of the parts the results had been good. He had been much pleased with the effect of the rays in nose, throat and ear cases. He would like to know how much of it was attributable to the effect on the cells, and how much to that on the circulating blood.

Dr. EIDINOW (in reply) said that the dosage which gave the best response, judged by the bactericidal power of the blood, was the minimal erythema dose. It was best to expose 20 to 30 square centimetres to a kilogram weight. Excessive exposure was harmful; it lowered the blood resistance. The lamp used in Copenhagen technique was poor in biologically active ultra-violet rays. It contained much visible light, and long ultra-violet rays, and only few short ultra-violet rays. In this country, mercury vapour lamps were being used; these were powerful in ultra-violet rays. The skin was a growing organ, and was rapidly undergoing changes. What was probably happening under the Copenhagen technique was that some cells which had received light were probably immune for a time, while other cells which had not received stimulation reacted. The plan was to give light twice or three times a week, but each time to a different area of the body. To produce a tonic effect on the blood there was no need to use ultra-violet light at all. The ordinary filament tungsten lamp, giving off much visible light, might produce as good a tonic effect as the mercury vapour lamp. When a patient was suffering from infective disease and had a low resistance, it could only be improved by giving a minimal dose to produce erythema. The blood would not respond to a massive dose of either these rays or light. By means of this technique there have been very successful cures of lupus of the skin and in other types of infection by using the short-ray technique. Massive exposures were found to be useless.

When there was present an acute or subacute condition, with definite pyrexia, it was necessary to limit the rays still more, exposing a still smaller area of skin. During the last two years it had been found clinically, and from the bacteriological examinations, that the treatment seemed as good as the Copenhagen technique, saved much time and labour, and appeared more scientific.

The question of direct action of the light on the skin and on the circulating blood was a difficult one; it had been studied for a long time, but not much progress had been made, as it was difficult to make extracts of skin tissues. He had ground skin up and exposed it to light, in the hope of finding a bactericidal substance but he had failed to do so. If, however, he took a quantity of blood and exposed it to light, the blood was damaged, some of the red cells were hæmolysed, and the blood lost its power to kill microbes; but when he injected that blood into animals he obtained a rise of bactericidal power. Ultra-violet rays did not seem to penetrate as far as the blood, though they might reach the most superficial blood vessels. When irradiation was done it was shown that a bactericidal substance was produced. It must be assumed that the effect of the light began where the light was placed. The action must occur in superficial living skin cells.

He would be interested to try the laryngoscopic method which had been mentioned. He had placed his lamp as far down the pharynx as possible, the patient being anæsthetized.

Hyperplastic Laryngitis and Pharyngitis; ? Myxœdema.[1] (Shown a year ago, but not reported.)—Sir JAMES DUNDAS-GRANT, K.B.E., M.D.—The patient, a girl aged 20, was originally sent up as a case of tuberculosis. There is hyperplasia of the uvula and of the epiglottis and aryepiglottic folds. A microscopical section of tissue from uvula showed the lesion to be simple chronic inflammation with no evidence of tuberculosis or rhinoscleroma. She was subjected to light treatment but no definite improvement took place until thyroid extract was administered, since which the subjective discomfort has been enormously diminished, though the change in the appearance is slight.

She is taking 8 gr. of thyroid (fresh glandular substance), and the question is whether an increasing dose is likely to do good. The diminution of the swelling is comparatively slight, but the general improvement is so great that she has gone to work.

1 The title of the case as originally furnished was as given above (" ? Myxœdema "), not as on the agenda paper, " with myxœdema."

Discussion.—Mr. HAROLD BARWELL asked whether Sir James's diagnosis was that the swelling was the result of myxœdema. The speaker had seen a certain amount of thickening of the laryngeal mucosa in myxœdematous patients, but never such a degree of swelling as in this case.

Mr. HERBERT TILLEY said that cases of a similar type had been described by Brown-Kelly in 1901 (*Lancet*), under the term sclerotic hyperplasia of the pharynx and nasopharynx. He (the speaker) thought he had seen a similar condition following pneumococcal infection.

Mr. NORMAN PATTERSON asked whether the Wassermann had been tested in this case.

The PRESIDENT asked whether there were any other signs of myxœdema in the patient.

Sir JAMES DUNDAS-GRANT (in reply) said the Wassermann reaction was negative. The condition was supposed to be tuberculous but the sputum was negative. The diagnosis rested to a considerable extent on the extraordinary improvement immediately following thyroid treatment.

Specimen: Œsophagus from an Elderly Man, treated by Radium for Carcinoma.—V. E. NEGUS, M.S.—The specimen shows an extensive carcinoma, which has perforated the trachea (fig. 1, p. 14). The original growth was treated over twenty times with radium and shows disappearance of the carcinomatous cells and the formation of fibrous tissue in their place. Owing to defective distribution of radium the growth has spread upwards and downwards and has thus caused death.

The radium applicator consists of a bougie, perforated to slide down a thread as in Plummer's method. Above this is a capsule for the radium, to which is attached a whalebone staff. The bougie is guided through the stricture by the thread and is kept in position for as long as required (fig. 2, p. 15).

Specimen: Œsophagus from a Middle-aged Man, showing a Congenital Opening into the Trachea.—V. E. NEGUS, M.S.—The patient was aged about 45, and died from general weakness, due to a carcinoma which was supposed to be primarily in the œsophagus.

On post-mortem examination, a primary growth was found in the lung, with secondary deposits around the œsophagus; owing to infiltration of the walls of the latter, the appearances were given on œsophagoscopy of a primary growth, and the symptoms were those of œsophageal stricture.

On post-mortem examination, a congenital opening was found, but it had not produced any symptoms in life, and had not given rise to septic pneumonia. The opening led in a downward direction from the trachea to œsophagus, and on the œsophageal aspect, there was a small valve-like hood of mucous membrane, which must have prevented regurgitation of food into the trachea.

Carcinoma of the Œsophagus treated by Radium.—V. E. NEGUS, M.S.—F. J., male, aged 63.

February 24, 1928.—First seen. History of dysphagia for one month. Skiagram shows narrowing of the œsophagus below the eighth dorsal vertebra; no evidence of aneurysm.

May 1, 1928.—Œsophagoscopy (local anæsthesia). Jackson's 9-mm. tube. Roughness on the right lateral wall, 26 cm. from the incisor teeth. A piece of this was removed for biopsy, and was reported to be epitheliomatous tissue, with some superadded inflammation.

June 22, 1928.—Gastrostomy. This was owing to absolute obstruction to both solids and fluids of four days' duration, following a period of rapidly increasing dysphagia. The patient was discharged in ten days after an uneventful recovery.

October 15, 1928.—Œsophagoscopy (local anæsthesia). Jackson's 9-mm. tube. There was a nodular, friable bleeding mass 26 cm. down. It was dilated with Jackson's bougie to 8 mm.

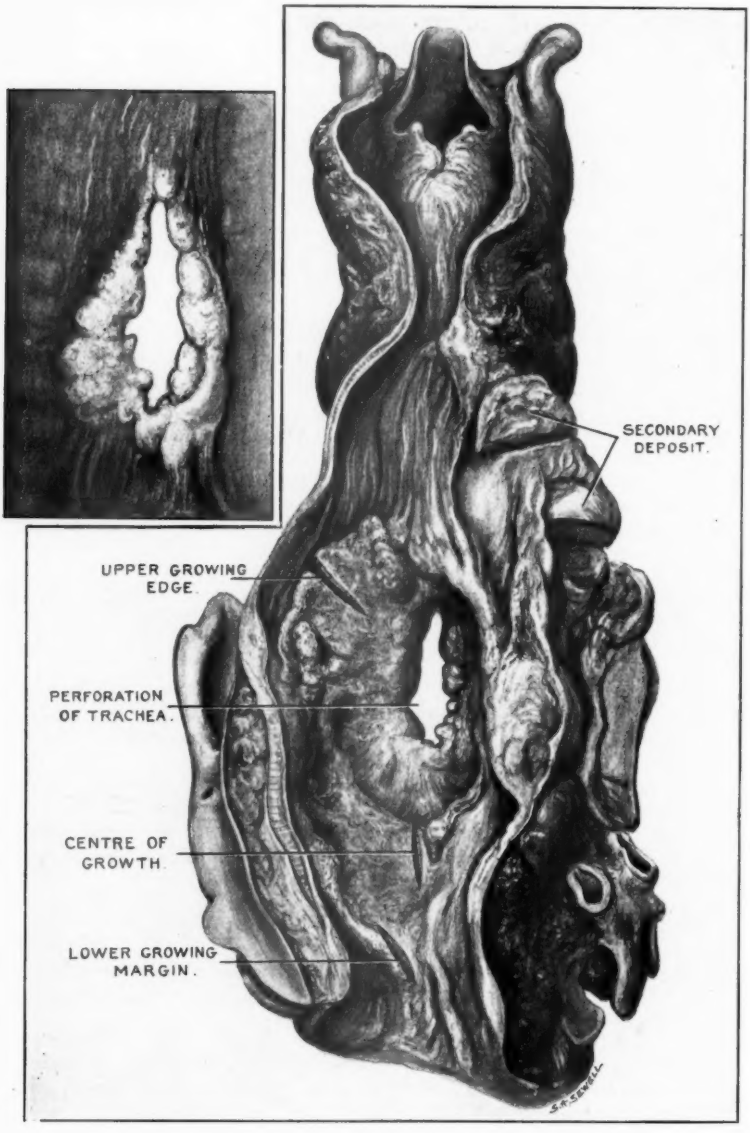

FIG. 1.—ŒSOPHAGUS OF PATIENT TREATED WITH RADIUM.

There is active growth of squamous-celled carcinoma at the upper and lower growing margins and at the edges of the perforation into the trachea. The centre of the growth has been irradiated efficiently and all progress of malignant tissue-cells here has been arrested. The small drawing shows the tracheal aspect of the perforation.

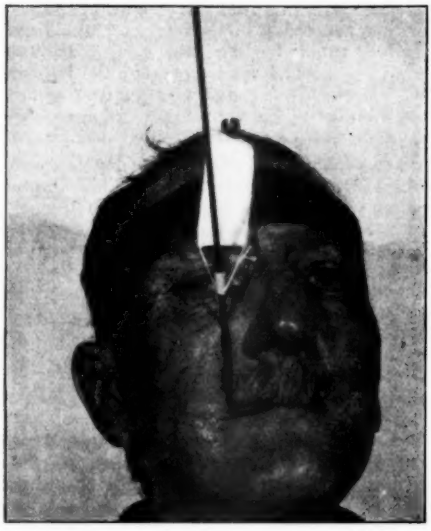

FIG. 2.—PATIENT DURING TREATMENT.

A whalebone staff projects from the mouth; slight upward pull on it is maintained by an elastic strip fixed to a band on the patient's head. The whalebone staff has at its lower end a radium container lying accurately in the stricture, and below the bougie-container there is an olivary bougie, of which the shoulders press on the lower margin of the stricture, thus preventing the radium from slipping out of position.

October 30, 1928.—One hundred milligrammes of radium passed down to growth in special container attached to a railroad bougie. Repeated on the two succeeding days, and on the 6th, 7th, 8th, 13th and 14th days of the nexth month.

November 15, 1928.—Œsophagoscopy. Growth as before. Easily dilated to 8 mm.

November 19, 20, 21, 1928.—Fifty milligrammes of radium applied just distal to growth for two hours.

November 22, 23, 24, 1928.—Fifty milligrammes applied just proximal to growth for two hours.

Remarks.—After the second application of radium the patient could swallow liquids, which he had not done for over five months. At the end of the course he could swallow light solids, such as mince, fish and chicken.

Discussion.—Sir JAMES DUNDAS-GRANT said Mr. Negus had shown a very ingenious and elegant way of applying radium with considerable accuracy, using a thread as a guide. Fibrosis occurring in the stricture after treatment was a very real difficulty. He had now under care a middle-aged man in whose case radium was applied on several occasions; there was definite enlargement of the passage, but after a time there was some narrowing; it was very low down. That patient could not take even liquids without some difficulty. Gastrostomy was performed with the best result, for the patient could now swallow through his œsophagus more easily. The speaker hoped to be able to dilate the fibrosis, so as to give considerable comfort, though probably the process would have to be repeated at intervals for the remainder of life.

Mr. H. V. FORSTER said he had often wished to see such a demonstration as this. He had been asked to put radium into the œsophagus in a case of carcinoma, but he had thought that there would be difficulty from the stilette irritating the arytenoids and fauces.

He had recently introduced radium into the œsophagus of a patient who was having lead treatment too, and he had then adopted a method which had allowed the radium to be kept *in situ* for seven hours. A long rubber feeding tube was used; it was not unlike Symond's original œsophageal catheter, but it had a winged end like the self-retaining urinary catheter. This long tube he had used occasionally for some years for feeding purposes when gastrostomy had been refused. He had been disappointed in the use of Soutar's short tube and Guisez's winged funnel; his observations with these tubes had been confirmed at autopsy.

Mr. E. WATSON-WILLIAMS inquired whether Mr. Negus felt certain that the recurrence was to be ascribed to a faulty placing of the radium. He had himself on several occasions had a similar experience after using radium. Apparently the original growth had healed, but within three or four months later recurrence had taken place an inch or more higher up the œsophagus. Even when special precautions have been taken to irradiate the œsophagus widely above and below the visible growth, the same thing had happened. He instanced a case for which a special container, with a central tube for the purpose of swallowing, was inserted with radium extending along 7 cm. of the œsophagus; although the irradiated area extended a full 2 cm. above the upper limit of visible growth the same melancholy sequel resulted.

But it was not only in irradiated cases where this happened. He had had cases treated by himself with selenium injections, and another was treated by Mr. A. T. Todd with injections of lead-selenium,[1] and here again with exactly the same result. No question could arise in these latter cases as to the proper placing of the medicament, and one was forced to conclude that the growing margin of the neoplasm was relatively little affected by radiations or other anti-neoplastic agents which had power to destroy the older part of the growth.

Mr. NEGUS (in reply) said he also had difficulty in getting the thread through in some patients. In such cases it was better to put the thread through with forceps, or on the end of a gum-elastic bougie, under direct vision by œsophagoscopy. The patient could keep the thread down two weeks or more if necessary.

Tuberculosis of Larynx with Cyst.—E. A. PETERS, F.R.C.S.—Female, aged 21. Hoarse three months. A growth, the size of a walnut, is seen attached to the left aryepiglottic fold. The growth has a solid rounded fibromatous appearance, and is probably a cyst.

Radiogram of chest.—Report: Consolidation of right apex with suspicion of cavitation.

Discussion.—Mr. HERBERT TILLEY said that he did not think this swelling was a cyst, because one could see a continuous swelling from the arytenoid upwards. He regarded it as tuberculous and suggested an examination under efficient local anæsthesia, so that the swelling could be punctured, and, if it were solid, galvano-puncture could be applied.

Dr. PETERS (in reply) said that when he had first seen the patient she was very hoarse and the mass rolled about with the laryngeal movements of respiration and presented the appearance of a cyst.

The patient had now nearly recovered her voice, and the mass appeared more like a solid infiltration of tuberculous character. He would puncture with the cautery and report the result.

Tuberculosis of the Larynx with Outgrowth from Ventricle.—Sir JAMES DUNDAS-GRANT, K.B.E., M.D.—Patient, a man aged 31. Former disappearance of outgrowth under galvano-cautery treatment. Recent recurrence. Removal of a portion of ventricular band which, on microscopical section, showed tuberculosis.

Discussion.—Mr. HERBERT TILLEY said that many years ago Dr. Jobson Horne in his work on tuberculosis of the larynx had shown that often the first deposit occurred in the mucous glands of the ventricular band, and was first noticeable when it emerged from beneath the band on to the surface of the cord. Recently he (the speaker) had seen a paper by Dr. Heyninx, of Brussels, reporting such cases of tuberculous laryngitis originating in the mucous glands of the ventricular band.

[1] *Vide* "Chemo-therapeutic Researches on Cancer," A. T. Todd, M.B., 1928.

Mr. HAROLD BARWELL said he agreed with Mr. Tilley that this was a tuberculous infiltration; there was a flask-like swelling of the arytenoid, right up the aryepiglottic fold; it could be punctured with the galvano-cautery as part of the treatment.

Mr. E. D. D. DAVIS said he thought an ulcer would be found on the anterior surface of the arytenoid, and agreed that the condition was a tuberculous infiltration.

Tuberculous Ulcer of the Septum Nasi with Infection of Sinuses of the Affected Side.—MILDRED WARDE, F.R.C.S.—Mrs. W. G., aged 39, came to hospital on October 19, 1928, complaining of discharge from the left ear, of three weeks' duration. She had suffered from left frontal and vertical headaches and from thick nasal discharge intermittently for fifteen months. Examination showed swelling of the left membrana tympani with a small perforation discharging watery fluid.

Nasal examination revealed a bright red polypus blocking the left nasal passage. This bled freely when touched but did not look malignant in type. It was removed with a snare and cheesy matter was found behind it, lying in an ulcer on the upper part of the septum. The edge of the ulcer consisted of pulpy granulations, and the whole picture suggested tubercle. Section of the polypus (slide shown) shows many giant cells but no tubercle bacilli have been found in the section nor in any of the specimens of discharge sent for examination.

A radiogram shows invasion of the left antrum, the ethmoid and, in my opinion, the left frontal sinus. Proof puncture of the antrum showed first a gelatinous mass with mucopus besides, and, on the second occasion, mucopus. On culture a mixed growth of staphylococci and streptococci occurred.

Up to the present the patient has been having ultra-violet ray treatment with menthol ointment and inhalations, and drops for the ear, the condition of which is much improved.

Discussion.—Mr. E. WATSON-WILLIAMS asked why the exhibitor called this condition tuberculous. It was true that the section showed giant cells, but no endothelioid cells or other elements of a tubercle, or tubercle bacilli. He had himself twice seen a similar condition, in each case in rhinitis caseosa: the core was surrounded by granulation tissue, quite obviously different from a nasal polypus and bleeding readily on touching. He regarded this as a chronic inflammatory condition involving the ethmoid and possibly other sinuses.

Mr. T. B. LAYTON said he thought Miss Warde had proved her contention that this was tuberculous. On looking at the mucous membrane one saw the edge of an ulceration very suggestive of a tuberculous ulceration such as might be seen in any part of the body. Beyond it was a cavity of caseous material. The section exhibited showed something characteristic of certain types of tuberculosis. A number of tubercles could be seen running together, with the giant cells beginning to break down into a caseous mass; endothelial cells were surrounding the giant cells, and outside were numerous leucocytes. This was sufficient to confirm the clinical diagnosis, and a section, after all, was meant to confirm a diagnosis, not to make one. If then there were tuberculosis of the nasal fossa one might suspect that the conditions within the maxillary sinus were also tuberculous. He suggested that Miss Warde should make another attempt at cultivation, and that whatever material was brought away should be divided into three parts. One part should be examined directly for tubercle bacilli; another should be cultivated in the ordinary way, and the third should be cultivated anaerobically. In antra he had examined, other than in cases of suppuration, cultivations had been made aerobically without success, but anaerobically had given a mixture of organisms.

If he did not think this was tuberculous he would have suggested operating on the deviated septum to relieve the pressure in the middle turbinal region, as he believed it was the first thing to do with certain cases of sinus disease in the nose. That he did not advise it here was because he would not operate across what he regarded as a tuberculous mucous membrane. Nor did he recommend a major operation on the maxillary sinus, either intra-nasally or by the sub-labial route. He did not find that operative treatment on the sinuses affected by non-suppurative inflammation gave the same good results as when the maxillary sinus was full of pus. Much mucous membrane might be scraped away, but the symptoms remained the same. He suggested that ionization of this maxillary sinus might be tried. Probably other organisms than tubercle bacilli were present in this case. He had had several cases of non-suppurative maxillary sinus disease, which had distinctly improved after ionization, and in this case he thought it might benefit the non-tuberculous factor.

Mr. NORMAN PATTERSON said that, failing the finding of the tubercle bacillus, it would be worth while to sacrifice a guinea-pig. In a case of tuberculous laryngitis which he had under investigation, he received three negative reports on the sputum, but a guinea-pig inoculated with material obtained direct from the larynx was found, when killed, to have developed tuberculosis.

Dr. JOBSON HORNE said his impression was that the nasal condition was not due to tuberculosis. Years ago, when he had examined under the microscope a large number of growths removed from the nose, he had met with giant cells in cases in which there was no evidence of tuberculosis. Before the case under discussion could be regarded as tuberculosis further evidence of that disease was required.

Miss WARDE (in reply) said that the diagnosis here was merely presumptive. As to trying the material on a guinea-pig, the pathologist said there were too many pyogenic organisms present to make the test of much value. There was a deep ulcer on the septum, and on passing a probe over the edge of the tissues one discovered a pit, from which caseous material could be scraped. The polyp had not the appearance of an ordinary nasal polyp. She had tried to find a foreign body, thinking it might be carious bone giving rise to giant cells, but she found only polypoid granulations. The antrum culture the first time was sterile. She would follow Mr. Layton's suggestion and give a further report. The aural discharge was thin and watery, and there was practically no pain.

Congenital and Hereditary Nasal Deformity.—L. GRAHAM BROWN, F.R.C.S.—W. S., female, aged 11, first seen November 16, 1928, for nasal obstruction and attacks of tonsillitis.

The nasal deformity is congenital. The right naris is normal, but the left is stenosed with excoriation. Tonsils hypertrophied and cervical adenitis present.

The patient's father (shown for comparison) has a similar nasal deformity, and there are other features in common between parent and child, viz., irregular dentition with poorly developed face and jaws.

I propose to remove the tonsils, which are causing much obstruction and repeated tonsillitis. Apart from that there is the question of operation on the nose. Something ought to be done, not only for cosmetic reasons, but also to improve the health and give better nasal breathing.

Discussion.—Mr. HERBERT TILLEY said he would suggest submucous resection and that the father also be advised to submit to a similar procedure.

Mr. NORMAN BARNETT asked what was the pathological cause of the condition. Was it due to the deformity? Had syphilis and tuberculosis been excluded?

Mr. GRAHAM BROWN (in reply) said that a septal operation was said to have been performed at Middlesex Hospital on the father in 1910. The result, it would be agreed, was not very satisfactory. He intended to leave the father alone. He had not gone into the matter of syphilis and tuberculosis in the child. The condition in both father and child was congenital, and seemed to be due to mal-development of the facial bones. The father was said also to have been operated on for cleft palate at an early age, but there was no trace of this.

Specimen of Bilateral Abductor Paralysis.—H. BELL TAWSE, F.R.C.S.—F. B., male, aged 5. April 21, 1928.—Guillotine enucleation of very large tonsils and removal of adenoids under ethyl chloride anæsthesia on account of nasal and pharyngeal obstruction. Breathed badly when asleep. Larynx not examined. May 12, 1928.—Breathing worse at night and occasionally, even during the day, makes a peculiar croaking noise. Mother suggests that "one of the tonsils has been dropped into the throat." Admitted to hospital.

Larynx shows bilateral abductor paralysis. Glottis a mere chink and varies only in the direction of getting less at times. Voice good—complete adduction on phonation—breathing impeded. Some recession of the intercostal spaces; occasional attacks of urgent dyspnœa and inspiratory stridor. *Physician's Report.*—Negative. *X-ray Report.*—Negative. *Wassermann reaction* of patient and mother.—Negative

May 23, 1928.—Very difficult tracheotomy under ether and chloroform. Trachea small, soft and difficult to find and, on palpation, gave the impression of a soft tube. It was obscured by a large thymus which had to be retracted. Insertion of tube did not give the relief anticipated and suggested obstruction lower down. June 3, 1928.—Sudden death from hæmorrhage through the tube.

Post-mortem (Specimen shown).—Large thymus apparently enveloping cricoid, trachea and continuous with similar looking tissue around roots of lungs and heart. Hæmorrhage came from a tear in the right innominate vein.

Pathological Reports.—(1) Typical thymoid tissue with large Hassall's corpuscles; no evidence of malignancy. (2) Inflamed lymph gland tissue.

Papilloma of Larynx.—E. BROUGHTON BARNES, F.R.C.S.E.—R. S., male, aged 34. Husky since February, 1927. Admitted to Creaton Tuberculosis Sanatorium, February, 1928. Said to have had tubercle bacilli in sputum before admission. No evidence of tuberculosis of lung found since admission. June, 1928: Semi-pedunculated tumour beneath anterior end of right cord: supposed to be tuberculoma. Removed through a tube. Report on section shown: The fragments from the vocal cord are composed of stratified squamous epithelium arranged round a fine fibrous tissue reticulum like a papilloma in transverse section.

The tumour now extends widely on both sides of the larynx, chiefly in the subglottic space.

Discussion.—Mr. A. WYLIE said he thought Mr. Barnes should get a good view by direct laryngoscopy and try again to remove the growth thoroughly.

Mr. A. J. WRIGHT said that in some cases of papilloma of the larynx recurrences would take place, whatever type of operation was carried out. At relatively long intervals throughout her life—she was now over 70—recurrences had taken place. The late Dr. William Hill had removed these growths several times and had cauterized the base after removal. He (the speaker) had twice during the last few years removed them by the direct method. He knew of no means of checking these recurrences. He had been watching for some years a case in which the patient, an old lady, had, during adolescence, undergone thyrofissure, performed by Sir Henry Butlin for the removal of papillomata from the larynx.

Mr. HERBERT TILLEY said he had had a similar case, that of a patient who had had papilloma removed from the anterior commissure on five occasions by the direct method. She was then about 40 years of age. He had advised that she should obtain another opinion regarding the question of laryngo-fissure. A colleague suggested that the growth was malignant. He (the speaker) persuaded the patient to allow him to open the larynx by thyrofissure; there had been no recurrence during the last four years, and the voice had remained normal. He advised Mr. Barnes to make one more attempt in this case by the direct method, and if he had any doubt about having removed all the growth, it would be wise to split the larynx, and effect complete removal by cutting deep into and around its base of origin, and then cauterizing this region. In this patient the growth is largely subglottic, and it extends to the other side. The right cord did not appear to move as freely as the left, and therefore it would be well to have everything ready as if for the removal of a malignant growth.

CORRIGENDUM.

Proceedings, 1928, xxii (Sect. Laryng.).—The illustrations facing p. 2 *et seq.* refer to Mr. COLLEDGE'S remarks on pp. 1, 2.

Section of Odontology.

[*January* 28, 1929.]

Some Notes on the Origin and Prevention of Gingivitis.

By G. B. Pritchard, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

Abstract.—Gingivitis is a disease of local origin primarily, although in many cases a constitutional factor is concerned.—The predisposition arises during the eruptive period of the second dentition.—The developmental predisposing cause of most importance is a crowded condition of the permanent dentition.—Of acquired predisposing causes, calculus deposits on the teeth are the most important.—Others are those which either cause stagnation around the teeth, or produce definite mechanical injury to the gum margin.—They can take effect at any age.

Orthodontic treatment directed towards the arrangement of the optimum number of teeth in well functioning order is the ideal prophylactic.

Other preventive measures are to employ such methods of artificial cleansing as brushing and the use of floss silk.—These are the adjuncts to automatic cleansing best carried out by efficient mastication of a suitable diet.—This is only possible when the arrangement of the teeth is such that stagnation of any kind of food is reduced to a minimum.

I shall deal only with that type of gingivitis which arises as a primary local disease of the mouth. I would exclude cases occurring as manifestations of a general systemic intoxication. I think that most cases are manifestations of local disease. On the other hand, some cannot be accounted for as a local disease only. These are seen at all ages—chiefly after early middle life—and often one finds no abnormality in the mouth to account for the incidence of the disease.

Gingivitis is a disease of youth. It may begin during the eruptive period of the permanent dentition, or if it does not begin during this period, the predisposition to it does so, and it is only a matter of time before infection of the tissues takes place. Next to dental caries a chronic gingivitis is the most common disease of the mouth. In its earliest stages the pathological changes are so slight that they may be missed altogether, and as the symptoms are so vague as to be almost negligible, the condition tends to become firmly established and widely spread before the patient has any idea of its presence. The recognition of this is most important, because an established gingivitis insensibly merges into definite pyorrhœa with bone inflammation which cannot be cured except by extraction of the teeth. In this connexion, however, H. K. Box has demonstrated the possibilities of regeneration of bone and re-attachment of gum. Conversely, it is my opinion that pyorrhœa can be aborted by judiciously applying expectant treatment which should be directed firstly against the incidence of a gingivitis, and secondly, against localized areas of a commencing gingivitis. I want to stress the importance of beginning treatment, both anticipatory and remedial, at the earliest possible moment. Gingivitis in its early stages is essentially an extremely localized inflammation, and the inevitable spread is usually slow. When it has become generalized, pyorrhœa pockets have already begun to form. A chronic gingivitis is rarely seen in the intact mouth of an average healthy child of six years, or younger, and this is not due to the shortness of time available for its development, whereas I have seen it well established around the permanent incisors of children of 8 to 10 years of age. Active growth is in progress, and, as the teeth pierce the gum and are moulded into position and the gingival margin forms around them, there is always adequate room for each of them. Even the canine—the only tooth to erupt between those already in position—comes into line without difficulty. On the eruption of the second temporary molar there is ample room for it to take up its proper position, and also—and this is most important—for the gingival margin to form normally on all sides of it. The same phenomena are seen during the eruption

of the permanent dentition if normally articulating arches are developing and bone growth is adequate. On the other hand, the eruption of the permanent dentition and the development of the gingival margin around individual teeth may present a totally different picture. In the first place, owing to the eruption of the teeth in successive groups, their ultimate position is partly governed by that of the milk teeth left standing, and that of the permanent series already erupted, so that sharply defined areas, only, are available for them, more or less adequate in accordance with the amount of translation undergone by the milk teeth. This translation, of course, when complete being evidence of normal growth. If they cannot take up their proper alignment, the subsequent development of the gingival margin is seriously interfered with. The thin gingival crest cannot be protected by the convexity at the neck, and the sharp angle at the junction of the superficial epithelium with the enamel becomes rounded off and is not moulded closely around the necks. It is in this abnormal gingival margin, I consider, that the most important predisposing factor to the incidence of gingivitis lies.

THE DEVELOPMENT OF THE ABNORMAL GINGIVAL MARGIN.—Gingivitis is seen beginning at definite age periods during the growth of the jaws and the eruption of the teeth, and in definitely localized areas of the gums, until the third molars are properly established in position. After this period of growth has ceased and the disease so far escaped, the tendency is much less marked. I will now consider these age periods and their relationship to the formation of the gingival margin.

(1) *The Incisor Teeth Eruption—age* 7-9.—On the eruption of the central incisor teeth, as they pierce the gum the usual forces operate to mould them into their normal position. While this happens the dental epithelium blends with the oral epithelium to form the crest of the gingival margin and this lies close to the tooth, protected by the convexity of the enamel. On the eruption of the lateral incisors, as with all the teeth, the same process is repeated, and the mucosa gets heaped up on the interdental septum to form the papilla.

If, however, owing to limitation of space, the normal alignment does not result, a very different condition is seen. On the anterior aspect of the instanding tooth and the posterior surface of the outstanding tooth the margin, instead of falling away sharply from under the protection of the enamel convexity, is heaped-up in a blunt, rounded surface and a definite sulcus is formed at the point where the primary injury will occur, as the result of trauma or food stagnation. In the region of the interdental papilla the condition is more marked still; and a triangular area, bounded on two sides by enamel and on the third by the heaped-up gum margin, is seen with its deepest point at the enamel contact points. Direct injury may be inflicted on this area when biting, and stagnation on the injured surface is inevitable.

(2) *The Premolar and Canine Teeth Eruption—age* 10-13.—The eruption of these teeth is comparable with that of the incisors, a definitely restricted area is given to them. For reasons not connected with the subject of this paper this area is often insufficient. The resultant deformity varies; I will give two examples and their results. If one tooth, the last to be erupted, lies partly or completely inside or outside the arch, the gum margin gets heaped up just away from the amelo-gingival angle and invites direct trauma to form a localized pocket. On the other hand, if the last tooth to erupt—and I now refer especially to the canine—gets forced into the arch, the effects are felt throughout the whole of the incisor area. The arch gives way, horizontally in the upper to form a superior protrusion and vertically in the lower to cause "fanning". This can only be brought about by the approximation of the roots and the alteration of the contact points and results in compression of the interdental papilla.

(3) *The Third Molar Eruption.*—It is exceedingly rare for the gingival margin behind the lower third molars to develop as it does elsewhere. In man I consider it normal for a pocket to be present in this region. The gum is loosely attached to the periosteum and without the slightest injury this pocket can be opened widely. This is a potential area of infection, and in accordance with variations in the shape, size and depth of this pocket is the likelihood of infection. There is another aspect of the eruption of this tooth and this concerns the pressure it may exert if its movement is impeded. The results of this pressure can be transmitted widely. I have seen a first premolar squeezed completely out of the arch during the eruption of the third molar. Also I have come across corrected cases of anterior crowding which relapsed completely long after correction and entirely as a result of third molar eruption. Clinically, during the prolonged eruption of this tooth, one can see a congestion of the gum with serous exudate as far forward, perhaps, as the first premolar. The congestion is at its maximum during the eruptive spasms and is least during periods of quiescence. From this observation I conclude that this is a result of pressure transmitted through the teeth to the interdental papillæ, although it is rather difficult to understand how this is possible when the contact points are all established, although occasionally one sees the contact points shifting laterally. Alternatively, it may be due to a spread of inflammation from the congested gum at the site of eruption. Whatever the cause, if this condition persists long enough, calculus is bound to occur at the gingival margin.

Having described the formation and development of an anatomically abnormal gingival margin, which I consider the most important factor in the predisposition to gingivitis, and also some of the pathological sequelæ of this abnormality, I will tabulate the essentials for prevention of the primary injury which, as James and Counsell have demonstrated, takes place at the junction of the gingival and subgingival epithelium at the neck of the tooth.

These are :—

(1) *A normal gingival margin.*
(2) *The maintenance of the integrity of that margin.*

In connexion with this and assuming that the abnormal margin is the developmental predisposing cause of gingivitis, one must consider what acquired predisposing causes there are, and they seem to be as follows :—

(1) *Calculus.*
(2) *Malocclusion.*—(*a*) Established causes producing abnormal gingival margins as described above, which in time undergo permanent organic alteration in structure. (*b*) Traumatic occlusion. (*c*) Deformities resulting from mouth-breathing.
(3) *Mouth-breathing* from habit, nasal and pharyngeal obstruction.
(4) *Dental caries* resulting in loss of contact points, disuse and mechanical injury to the gum margin.
(5) *Restorations* to, or below, the gum margin.
(6) *Prosthetic* and *orthodontic* appliances.

The responsibility for the formation of a normal gingival margin falls on nature or the orthodontist. To maintain its integrity the function of the mouth must be carried out as perfectly as possible. This entails regular arches conforming to each other, and the result is self-cleansing by efficient mastication. For efficient mastication to clean thoroughly, the diet must be suitable. The contact points must be established, and kept in contact. When this is not possible the spaces must be adequate for self-cleansing. The tooth-bearing area must not encroach on the base

of the ascending ramus. Restorations must reproduce the form of the normal tooth and should not encroach on the gum margin. Prosthetic appliances must be kept clear of the gum margin where possible. This is not always practicable, but it is obviously the lesser of the two evils to have a plate with, say, two abutments than to have many functionless teeth. The same applies to orthodontic appliances, but I think it best to do without them whenever possible. The application of many of the factors on my list of acquired predisposing causes is self-evident. A few I will discuss further.

Traumatic Occlusion.—In certain cases of traumatic occlusion, e.g., when a lower incisor, usually outstanding, strikes the back of the upper on closure, a definite destruction of tissue is taking place. At an early stage a zone of marginal congestion is seen, then pocket formation, which eventually extends towards the apex with extensive destruction of the alveolus, and all confined to the anterior aspect of the tooth.

Interstitial Caries and Restorations involving the Gum Margin.—If these conditions are of long standing, definite permanent changes in the papilla, of an inflammatory hypertrophic nature, are seen and nothing short of extraction will alter the condition.

Calculus.—This is most important. A variable amount of deposit appears in all mouths. It seems to me that there is a sharp line of demarcation between the two main varieties of calculus found on the necks of the teeth. The clinical variety known as salivary calculus starts depositing on the enamel immediately superficial to the gingival margin. At first it does no damage, but as it thickens it forms a ledge under which food débris and secretions stagnate, and as it spreads downwards towards the cementum it produces a progressive atrophy of the gum margin and, in time, of the alveolar crest and periodontal membrane. It is indeed surprising that, in many cases, this is all that does happen. Frequently the gum presents an ulcerated margin in contact with the deposit, and then there is superadded the irritation produced by the newly deposited serumal calculus. On the removal of this deposit the ulcerated margin heals rapidly, but the teeth remain with their gingivæ permanently altered and open to trauma. When serumal calculus is found on the necks of teeth its presence must be viewed with the utmost suspicion. It always occurs as a gingival deposit and is therefore produced in association with a marginal pocket. In its earliest form it seems to be deposited as a result of a congestion of the gum margin, the leptothrix, which, as J. G. Turner and Bulleid have pointed out on various occasions. is always associated with all tartar formation, getting its calcium from the exudate. Once formed, it is a very definite mechanical and infective irritant to the already damaged tissues. In many cases it persists as a subgingival deposit only, and generally stops short of the bottom of the pocket. Indeed, in long-standing cases of pyorrhœa with deep pockets, there may be an appreciable distance between this ring and the limit of the tissue destruction.

The Prevention of Gingivitis.—As the majority of the cases of gingivitis appear to arise, or rather the predisposition arises, during the growth period of the jaws, while the gingivæ are forming around the erupting permanent dentition, measures to prevent its onset should be taken as early as possible and should be orthodontic. These should provide adequate room for each group of teeth before they erupt, and, within reasonable limits, as long as possible before eruption, if such anticipation is feasible, so that the forming gingival margin can mould itself normally around the necks of the teeth as they erupt. If the natural growth of the jaws has not provided this room then orthodontic interference should take place. I have pointed out earlier that a gingivitis starts around the anterior teeth in the

child and the posterior teeth in the young adult. In the case of the civilized child of to-day it does not seem always possible to make adequate room for the incisors, or—and this is equally important—to keep adequate space for the ten anterior teeth, without the sacrifice of at first some temporary teeth and later a tooth on each side in the premolar region. I am now referring to the type where a definite anterior crowding seems inevitable. Without trespassing on the purely orthodontic aspect of these cases I think such treatment has important effects both immediate and remote.

The immediate effects are to allow the lateral incisors to erupt into line, and if the first premolar can be removed well before eruption of the canine, the latter moves bodily backwards as it erupts and provides that most important relief of pressure on the roots of the incisor teeth. The remote effects are not seen until the eruption of the third molars. In addition to the relief of pressure on the anterior teeth there is the general forward movement of the molar teeth, and this is beneficial in two ways. It brings the third molar further away from the areolar tissue at the base of the ascending ramus, and it lessens the probabilities of its impaction and the damage to the tissues which may result from its attempts at eruption. In the case of the adult with a commencing marginal inflammation around erupting, or erupted, third molars, and because of the extreme probability of a definite tissue change becoming established, it is important to decide as early as possible if these inflammatory manifestations are likely to subside on completion of growth. If one decides in the negative, the tooth should be extracted and very shortly a firm margin is seen all round the second molar, and natural, or easy, artificial cleansing is established. I do not suggest that extraction of the first premolars is a panacea for the prevention of gingivitis. The making of sufficient space for the number of teeth to be retained, fully functional, in well shaped arches is the essential, and the manner in which it is carried out is almost immaterial. On the other hand, I suggest that, when gingivitis is threatened because of irregularities due to underdevelopment of the jaws, or any other cause, making thirty-two teeth too many for the jaws to hold, prolonged orthodontic treatment definitely increases the liability of the gum margin to infection, because of the unavoidable use of apparatus. Also, that the third molar, as a result of such treatment, is most likely to develop and erupt so badly placed with regard both to the soft tissues and to the other teeth, that infection is inevitable. Regarding mouth breathing as a cause of gingivitis, it is most important that it should be cured before it becomes an intractable habit, continuing long after its cause has been removed. The local irritation produced by mouth-breathing is often sufficient to start and perpetuate a most intractable anterior gingivitis. The importance of treating caries so early that simple fillings are enough for perfect restorations without encroaching on the gum margin is so obvious that it needs no further mention. Artificial cleansing is usually required in the mouth of modern man although the necessity is reduced to a minimum if the gingivæ are normal. In such a mouth, the gum margin, by virtue of its anatomical relationship, is largely protected from direct trauma. Salivary calculus deposits also tend to be small and are kept down by proper functional activity, provided the diet is suitable and dietetic habits are as they should be. Tooth brushing must not be overdone. Gentle brushing with a brush of medium stiffness and an impalpable dentifrice and cleaning the interdental areas with floss silk are all that is required. Tooth brushing is frequently overdone and one constantly sees cases in which the excessive use of very hard brushes and abrasive dentifrices has so damaged the soft tissues that all indications of a normal margin have disappeared.

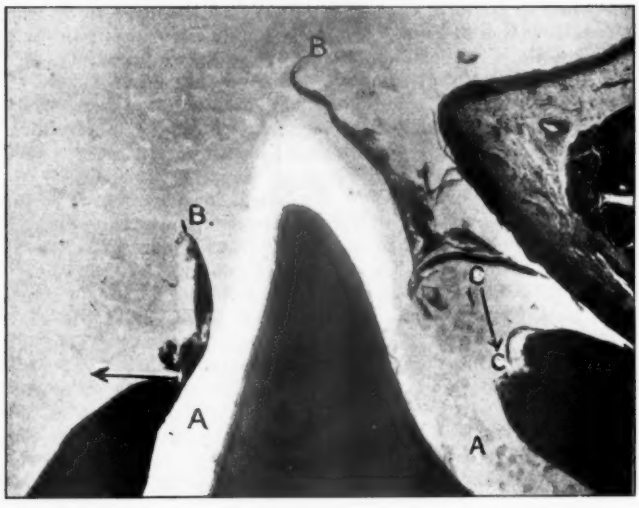

FIG. 1. × 60 (approx.)

Erupting lower carnassial tooth of stray kitten in very poor condition (transverse section of jaw). Much oral débris stagnating on Nasmyth's membrane which has become detached from the gingival crest on right of figure. A, Space for enamel removed in decalcification. B, Nasmyth's membrane with deposit. C, Tip of gingival crest.

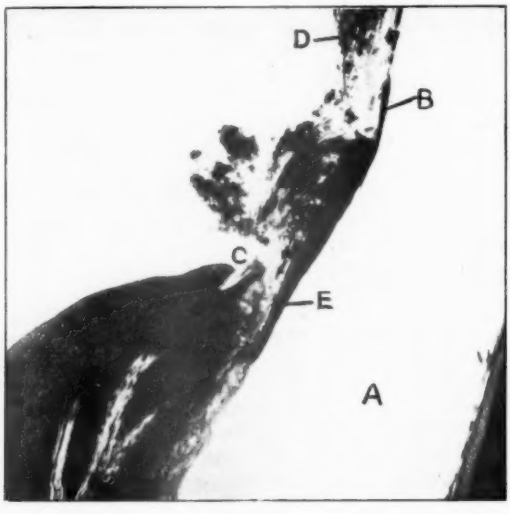

FIG. 2. × 400 (approx.)

Gingival crest from left side of fig. 1. Débris containing many leptothrix on Nasmyth's membrane impacted in the gingival crevice. A, Space for enamel. B, Nasmyth's membrane. C, Tip of gingival crest. D, Leptothrix. E, Débris impacted in gingival crevice.

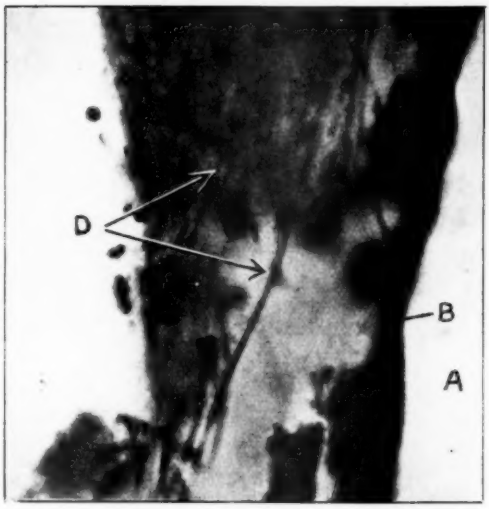

FIG. 3. × 1200 (approx.)

Leptothrix on Nasmyth's membrane. A, Space for enamel. B, Nasmyth's membrane. D, Leptothri

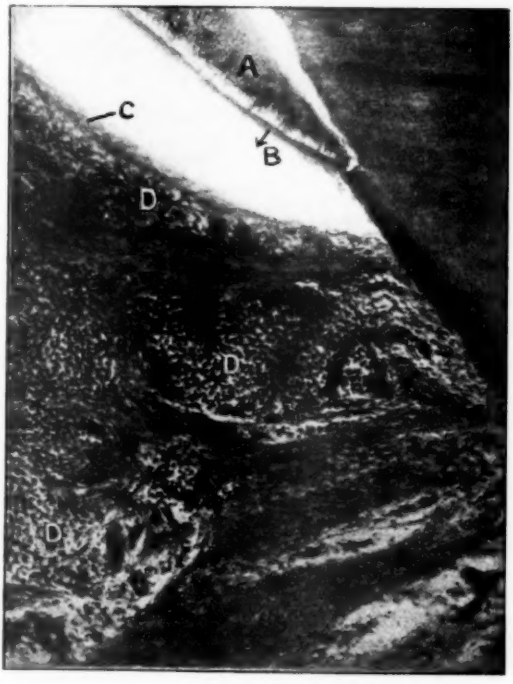

FIG. 4. × 400 (approx.

Interdental papilla upper molar. Chronic inflammatory infiltrations of corium. Some enamel débris with Nasmyth's membrane *in situ*. The separation between the latter and the rest of the subgingival epithelium is an artefact at the time of extraction. A, Enamel débris. B, Nasmyth's membrane. C, Subgingival epithelium. D, Lymphocytic infiltration.

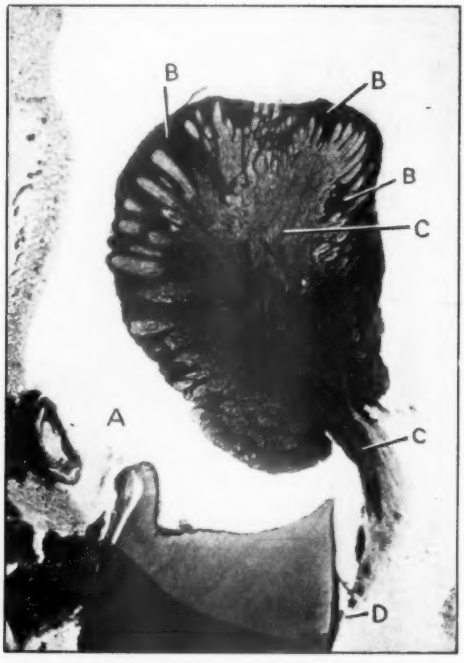

FIG. 5. × 15 (approx.)

Chronic inflammatory hypertrophy of interdental papilla in relationship with a carious cavity. A, Carious cavity. B, Proliferated epithelium. C, Fibrous tissue much hypertrophied extending from superficial fibres of periodontal membrane D.

Discussion.—Mr. ARTHUR BULLEID inquired if Mr. Pritchard had used mercurochrome in the local treatment of early cases.

Mr. DAVIES THOMPSON stressed the importance of breathing exercises for children who had had tonsils and adenoids removed. He had obtained good results in cases of gingivitis by simple massage of the gums.

Mr. ST. J. STEADMAN agreed that gingivitis was a disease of the young, and that loss of function and crowding were among the main features of the condition.

Mr. LEWIN PAYNE said that during the last two or three years a good deal of attention had been drawn to the anatomy of cases of gingivitis, and he was glad that Mr. Pritchard had dealt with the clinical aspect in his paper, and had referred to some of the changes that occurred during the eruption of teeth. He supported Mr. Pritchard's view that the majority of cases of pyorrhœa arose as a result of local disease, although as factors, general systemic infections could not be ignored. With regard to the important point of the connexion between gingivitis and the eruption of the third molar, the reason for this seemed to him to be threefold: (1) the smallness of the present-day jaws in the third molar region and the consequent obstruction; (2) the extreme difficulty in keeping the region clean; and (3) the pocket during the process of eruption. He referred to the frequency with which gingivitis began during young adult life, and said that many cases could be traced to the period when the third molar began to erupt. In cases in which gingivitis developed in the anterior part of the mouth in children, the condition was probably due to the habit of mouth-breathing. In place of the term "traumatic occlusion," he would

prefer that of "abnormal occlusal stress." Finally, he urged the need for more careful charting of the cases. Great care was taken to record every detail in connexion with the teeth themselves, and it was equally important to record the actual conditions in the various parts of the mouth.

Mr. C. H. HOUSDEN referred to the importance of maintaining the eruption of the first permanent molars in the normal way.

Mr. G. F. CALE-MATTHEWS said that restoration of function was one of the most important conditions in the mouth, and if vertical growth in the posterior part of the mouth could be induced, the automatic cure of gingivitis would be secured. He regarded the pre-molar teeth as sacred; they maintained occlusion. In regard to the prevention of gingivitis, the removal of posterior teeth should be considered to a greater extent than was the case at present.

Mr. PRITCHARD (in reply) said that the toothbrush could not be altogether abolished, but it must be employed as an adjunct to other methods of cleansing the teeth. He stated that Warwick James had, in his paper, stressed the importance of the fact that pyorrhœa and gingivitis were not essentially septic diseases. With regard to mercurochrome, he had not yet had experience of it, but hoped to use it later. He considered the use of a dentifrice most important, because it gave patients something with which to massage and had a stimulating effect upon the gum. He agreed with Mr. Payne as to the need for recognizing that there were many cases of gingivitis which were not of local origin. With regard to the point raised by Mr. Housden, if the permanent molar was not encouraged to move forward, there would be the extreme probability of infection occurring behind the tooth.

Section of Orthopædics.

[*November* 6, 1928.]

A Case of Kohler's Disease Presenting Unusual Features.—E. HENRIETTA JEBENS, F.R.C.S.—D. B., a boy, first seen in January, 1928, when aged $2\frac{1}{2}$ years, on account of pain and swelling of left foot.

January 18, 1928.—Left foot was swollen, especially over dorsum. Wasting of left leg $\frac{3}{4}$ in., left thigh, $\frac{1}{4}$ in. Knee-jerks normal. General health good. Movements of ankle-joint free and painless, of tarsal joints slightly limited, but not apparently painful. *Skiagram.*—Left foot: Scaphoid very dense but good shape. Slight general rarefaction in tarsal bones. Right foot: Scaphoid ossified, but normal in density and larger than on left side. *Treatment.*—Immobilization in plaster.

February 15, 1928.—Foot still swollen. *Skiagram.*—Left foot: Scaphoid larger and less dense.

March 14, 1928.—Foot still swollen. *Skiagram.*—Left foot: General rarefaction. Some destructive changes in scaphoid. ? fragmentation.

April 11, 1928.—Foot less swollen. *Skiagram.*—Left foot: Now hardly any trace of scaphoid.

May 9, 1928.—Slight swelling still present; plaster removed. *Skiagram.*—Left foot: Hardly a trace of scaphoid visible. Right foot: Scaphoid normal.

June 26, 1928.—Slight swelling over dorsum of foot; no pain. Child walking normally. *Skiagram.*—Left foot: Scaphoid beginning to reform.

August 15, 1928.—No swelling. Walking normally. *Skiagram.*—Left foot: Scaphoid fragment slightly larger and denser.

October 10, 1928.—Foot normal. *Skiagram.*—Left foot: Ossification of scaphoid increasing.

The case presents certain anomalous features, namely, the youth of the patient, and the process of fragmentation, disappearance and re-formation of the scaphoid nucleus.

Age.—An investigation of the literature reveals only two cases of patients so young as this. In 1913, Pfähler [1] reported the case of a boy, aged $3\frac{1}{2}$, who had had three attacks of pain and lameness during the preceding year, making the age of onset $2\frac{1}{2}$ years. Fassett [2], in 1914, reported Köhler's disease in a girl aged $2\frac{1}{2}$. In 1919, O'Brien [3] described a case in a girl, aged 3, and Cockayne [4] reported one where the patient complained of pain at the age of $3\frac{1}{2}$ years.

Kidner and Moro [5], in 1924, were able to collect 120 cases of Köhler's disease from the literature, but they give no bibliography. They remark that the disease occurs between the ages of 3 and 9, so presumably they found none younger on record.

Fragmentation.—In 1920, Moreau [6] described the case of a boy, aged 7, whose scaphoid showed a clear zone in the middle, as though it had been split in two. Kidner and Moro say that there is sometimes a tendency to fragmentation of the nucleus, but that it never breaks up into separate elements. In Bartolini's case, quoted by Kidner and Moro, there were three distinct centres, but the American observers think that these were anomalies of ossification and not true Köhler's disease. Lane's [7] two cases were boys aged 8 and $4\frac{3}{4}$ years who also suffered from pseudo-coxalgia; he describes the scaphoids as having the form of "thin discs, much fragmented." Christie [8] says that in Köhler's disease—unlike Perthes' disease—there is no fragmentation. Hodgkinson and Duke [9] remark that the nucleus may be fragmented, but they give no references and no specific cases to support the statement.

Summary.—The first point that distinguishes my case from the majority is the youth of the patient. Christie and others have suggested that Köhler's disease is a disorder of growth affecting the scaphoid at the time of its maximum development. Yet the nucleus of ossification of this tarsal does not usually appear before the fourth year. Spalteholtz [10], Poirier and Charpy [11], say that ossification

commences in it between the ages of 3 and 5. Kidner and Moro remark that it does not normally appear in X-rays until 3½ or 4 years of age. It is curious that Fassett does not remark on its early appearance in his patient aged 2½. Frazer gives the age of ossification as 4 to 5. I have not been able to find any record of earlier appearance of the nucleus. My case seems, at any rate, to dispose of Hoenisch's theory that the cause of the disease is delayed ossification.

The second point which, so far as my investigations go, is quite unique, is the curious disappearance of the scaphoid, followed by its re-appearance in a shape which suggests that it is re-forming around some nucleus not opaque to X-rays.

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Fracture of the Neck of the Femur in a Girl of 12 Years.—EDWIN A. LINDSAY, F.R.C.S.—The patient, O.P., was in the fever hospital during February and March, 1928, with diphtheria, and was discharged on March 28. She was "weak on her legs" after coming home, and about the middle of May definite limping was first noticed. Any possibility of injury was stoutly denied by mother and girl and they stated that the condition was not becoming worse.

The patient was first seen by me on June 28, 1928. She then complained that the limp was not getting any better, and that although there was no pain in the hip there was aching after walking for an hour.

FIG. 1.

On examination there was ¾ in. shortening of the right leg, ¾ in. wasting of the thigh, the calves were equal, the hip was prominent and the buttock flattened and broadened. Abduction, internal and external rotation were all limited by about one-third to one-half. Flexion, extension and adduction were not affected. There was no pain on jarring or movement. She limped on the right leg and walked as though she had a stiff hip.

The skiagram (fig. 1) revealed a fracture of the neck of the femur at the junction with the shaft, gaping above and incomplete below, the neck forming about a right angle with the shaft.

In view of the time which had elapsed, and the apparent firmness of union, attempt at correction of the deformity was not considered advisable, so the patient was provided with a patten-ended walking calliper and a three-inch patten on the left boot.

A skiagram on November 1 (fig. 2) showed the line of fracture filled in by sound bone.

A reference to the literature shows that fracture of the neck of the femur in childhood is less common than statements in some of the text-books suggest.

Bland-Sutton, in his Presidential Address to the Section of Surgery, in November, 1918, mentioned as a rare specimen one of a fractured neck of femur of a child aged about 15 years, which he had found in the Museum of the Middlesex Hospital. He doubted if there were more than five specimens of this type of fracture in all the museums of the United Kingdom, and the only living case he had seen was in a boy, aged 12 years, who had hurt his hip while playing a rough game.

Following this, Greig, in the *Edinburgh Medical Journal*, of February, 1919, stated that the accident was not common, and, as it was not fatal, there were few specimens. He quoted three cases which had come under his observation.

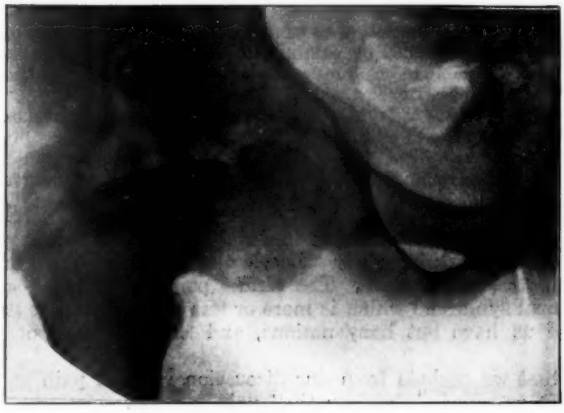


FIG. 2.

Previous to this, in the *Clinical Journal* of September 3, 1913, Telford (Manchester) referred to the latency of symptoms in fracture of the neck of the femur in adolescents. He quoted two cases of boys, aged 15 and 17, each of whom sought medical advice only after three months.

He distinguishes between separation of the epiphysis and fracture of the neck, and considers that the fracture is more usually at the base of the neck. If there is complete separation the symptoms are at once obvious, but if incomplete weeks or months may elapse before they seek advice. In this second type he thinks impaction frequently occurs, while others are of a greenstick variety.

In the *New York State Journal* of November, 1917, Taylor discusses fracture of the neck of the femur in children, and states that it was not until 1890 that a case was reported by Whitman.

He notes three different types of fracture :—

(1) In young adults aged from 18 to 20, usually not differing from that in advanced life, a break through the middle of the neck or near the head with upward displacement and external rotation and complete disability. Such cases often fail to unite and require some form of operative treatment.

(2) In adolescents, from about 12 to 16, the usual form is the slipped or separated epiphysis.

(3) The characteristic injury in children under 13 is a fracture at the base of the neck at its junction with the shaft, without, or with only angular, displacement, in adduction. He suggests the name of "hinge fracture" for this type, as the periosteum under the neck acts as a hinge. The line of fracture may be gaped open at the top, either by the original injury, or by walking on the injured limb, and is one of the causes of coxa vara. He cites seven cases of this type of fracture, four in boys and three in girls. One of his illustrations is very like the skiagrams here reproduced, and it is to this group that the present case belongs.

[*December* 4, 1928.]

DISCUSSION ON THE PAINFUL SHOULDER.

Mr. George Perkins.

ABSTRACT.—Three or four definite types are separated from among the many examples of painful shoulder and their treatment is debated:—

(1) *Adhesions around the Joint.*—Characterized by limitation of movement at the shoulder-joint, through the outer half of its range. Curable by manipulative surgery.

(2) *Tendonitis.*—Characterized by painful movements through a small arc in the middle of the normal range. Curable in the hyperacute patient by operation, in the acute by rest in partial abduction with the assistance of time.

(3) *Osteo-arthritis of the Shoulder-joint.*—Characterized by painful extremes of movement. Incurable but capable of alleviation by physiotherapy.

(4) *Subacute Arthritis of the Shoulder-joint.*—Characterized by muscle spasm at the commencement of movement. To be treated by rest on an abduction splint, and the eradication of septic foci.

THE subject for discussion has an ill-defined title, but I do not think anyone will have difficulty in defining for himself the limits of the problem which it is intended to probe and explore. The term "painful shoulder" conjures up the vision of a clinical syndrome which is more or less constant, about the pathology of which some of us have but hazy notions, and the treatment of which is often empirical.

I suggest that we exclude from our discussion referred pain at the tip of the shoulder. This so-called "shoulder tip" pain has few mysteries. The pain is accurately localized to the tip of the shoulder and felt in the distribution of the descending cutaneous branches of the third and fourth cervical nerves. The pain does not extend down the arm for more than an inch or so, and is accompanied by hyperæsthesia. It may occur whenever the phrenic nerve is irritated anywhere in its course; it is thus common in gall-stones, diaphragmatic pleurisy, subphrenic abscess, etc. Often the pain is increased during deep inspiration. The pain appears spontaneously and the shoulder-joint on examination is normal.

We shall deal with the painful shoulder proper. The symptom is pain, usually felt a few inches above the insertion of the deltoid and often continued down the outer side of the arm as far as the hand. The patient diagnoses the complaint as a neuritis; although it is, of course, a neuralgia and not a neuritis from which he is suffering. There is little or no muscle wasting, no reaction of degeneration, no alteration in sensation or in the reflexes, and the main nerve trunks of the arm are not tender on palpation. Movements at the shoulder-joint increase the pain and the weight of the arm dragging on the shoulder causes a constant ache in the limb.

But in spite of a general similarity in the symptoms in every patient, it is possible, I think, to separate and identify different types. I have tried in vain to find a good classification among the literature, and so I propose for the purposes of this discussion to disentangle certain well defined groups from the medley.

Type 1. *Adhesions around the Capsule.*—This type frequently follows trauma, although the damage to the shoulder may be incidental. Following a fall on the outstretched hand, the traumatic force passes up the arm—causing, perhaps, a Colles' fracture or a fractured neck of the humerus or a fractured clavicle, and in its travels the shoulder-joint is sprained. The hæmorrhage which takes place around the capsule organizes into fibrous tissue and this contracts into scar tissue, so that when the shoulder-joint is moved the scar tissue pulls on the healthy surrounding structures, stimulates the nerve endings, and pain is felt. Clinically the patient gives a history of trauma, often followed by a period during which the shoulder has been kept immobile in a sling. He complains of stiffness of the shoulder and pain on movement. On examination some or all movements are limited, particularly rotation and abduction, but *only in the outer half of their range.* This is the important fact. The inner half of the range is painless and free of muscle spasm ; muscle spasm does not occur until the movements are checked by the painful adhesions.

Treatment.—I firmly believe that this type of painful shoulder is preventible and, when it occurs, curable. If, following an injury such as a fall on the hand, the shoulder-joint is kept mobile from the commencement, irrespective of whether or no it appears to have been damaged, painful adhesions will not occur, because the fibrous tissue resulting from the hæmorrhage will be kept stretched during the time it is contracting down into scar tissue. Once the adhesions have formed they can be broken down by mobilization of the shoulder under anæsthesia. I prefer to mobilize repeatedly, in stages, not doing too much at a time, and under nitrous oxide anæsthesia so that the patient can begin active movements immediately without waiting twenty-four hours to recover from his anæsthetic. If the manipulation has been too forcible a period of rest will be required to recover from the trauma of the manipulation, and it is essential that the patient should follow the manipulation by active exercises so as to keep stretched the new fibrous tissue which is bound to form as a result of the breaking down of the adhesions. I suggest that physiotherapy for this type of patient without previous manipulation under anæsthesia is valueless. The patient after years of discomfort and disability and after the expenditure of much time and money on massage, heat and electricity, can be quickly cured by mobilization under anæsthesia.

Type 2. *Tendonitis.*—I apologize for the title. Whitman writes that Duplay [1] in 1872 first described a type of painful shoulder, under the name scapulo-humeral periarthritis, induced by injury and dependent on an inflammation of the subdeltoid bursa.

The symptoms of this type of painful shoulder were accurately described by Codman [2] in 1906, under the title of "subdeltoid bursitis." I have purposely refrained from using that polemic title because I do not think it consistent with the pathology. Many surgeons deny the existence of subdeltoid bursitis, but few will deny that the syndrome described under that name exists as a clinical entity.

Often after unusual exertion—starting up a motor car with the starting handle—often apparently spontaneous, severe pain is complained of over the outer side of the lower half of the deltoid, sometimes also round the external epicondyle of the elbow and down the hand. The pain varies in intensity and may be as severe as that of biliary colic; it is worse at night and the patient cannot lie on the affected side. On examination there is a localized tender spot over the head of the humerus just below the acromion, over, in fact, the greater tuberosity of the humerus. As Dawbarn pointed out (and it is often called Dawbarn's sign) this spot of tenderness disappears when the shoulder is abducted, presumably because the greater tuberosity disappears under the cover of the acromion. There is no increased warmth or swelling of the shoulder-joint. *Motion at the joint is full but through a small arc movement is painful*; this is the characteristic pathognomonic sign. The painful arc of movement is from about 75° to 90° of abduction with the shoulder slightly forward of the

plane of the body and slightly internally rotated. The patient can abduct the arm to 75° painlessly, then follows 20° of painful movement after which the arm can be stretched up above the head to its full extent without giving rise to any more pain. Pain is felt through this same arc of movement when the arm is lowered to the side. Some patients learn to dodge the painful arc of movement by rotating in or out the arm just before the painful stage and rotating back again when the painful arc is passed.

The symptoms and disability to the patient vary enormously. In its commonest form the condition is a mere nuisance; the patient forgets all about his complaint until he moves his shoulder through the painful arc of movement, sudden severe pain is felt and the patient breaks off from what he is attempting to do, and is apt to drop anything he is holding. Providing the patient's occupation does not necessitate him moving the shoulder through the painful arc the condition does not greatly inconvenience him. But often the predisposing or exciting factor is over use of the arm, and if this is caused by the patient's occupation he is rendered unfit for work. Providing the patient avoids the movements giving rise to the pain the condition gradually quietens down in a few days or weeks of its own accord, only to recur again a few weeks or months later on undue exertion of the shoulder.

Occasionally the attack is severe, and the patient may suffer the most intense agony suggesting to the doctor that there must be a fracture. The severity of the symptoms following an apparently trivial cause is remarkable. Sometimes morphia in large doses is needed to give relief. In these hyperacute cases the patient will not tolerate any movement of the shoulder, and the arm is hugged to the side wall of the chest.

Curiously enough, the muscles of the shoulder do not go into protective spasm and prevent movement at the joint, as occurs for example with an arthritis of the joint. This is readily understood if one accepts the view of the pathogenesis to be referred to later.

I submit that this condition forms a clinical entity. What is its explanation? Duplay and Codman considered it due to an inflammation of the subdeltoid bursa; this is a large bursa which lies between the capsule of the joint on the one side and the acromion process and the deltoid muscle on the other side. Many authors—Painter [3] the first—have called attention to the presence of calcareous deposits shown by radiograms around the joint in patients suffering from this complaint. He assumed that these calcareous deposits were in the wall of the bursa, and it is interesting to recall that it was Codman who first showed this to be fallacious; watching Painter operate, he pointed out to him that the deposits were beneath the floor of the bursa. This truth is now generally accepted. The calcareous deposits are beneath the bursa, the floor of which has to be divided before they can be reached. They are situated, in fact, in the tendon of the supraspinatus muscle at its insertion into the greater tuberosity of the humerus.

The calcareous deposits vary in consistency from soapy cheese to stony hardness and are composed of calcium phosphate and carbonate. They are not always visible in the radiograms when symptoms are present, and when visible they are not always associated with symptoms. Stereoscopic views should be taken to reveal them, as they may be hidden by the shadow of the head of the humerus. In my experience they are only seen infrequently; roughness of the greater tuberosity on the other hand is common. I hope we shall hear the opinions of radiographers on this point. The deposits undergo spontaneous disappearance with or without treatment.

Regarding pathogenesis, Moschowitz [4], after microscopical examination of tissue removed from Bickner's patients, concluded that the lesion was a tendonitis. Cultures of the bursa and the tissues deep to the bursa were sterile. He believed that, following trauma, small areas of tendon necrosed and underwent calcareous degeneration. The trauma might be due to excessive tension on the tendon caused

by the vigorous contraction of the muscle, or to the pinching of the insertion of the tendon between the head of the humerus and the acromion. I incline to the latter view. It seems plausible to imagine that the painful arc of movement is due to the head of the humerus impinging on the beak of the acromion process. If this view is correct, the term subdeltoid bursitis is a misnomer; in fact, if there was a bursitis there would be a cushion of fluid interposed between the head of the humerus and the acromion, and the impact would be avoided, and symptoms would be absent. I suggest that there is no bursitis, and that if there was, the symptoms associated with this type of painful shoulder would not exist. Moreover, some explanation has to be found for the fact that movement is only painful over a small arc, which, to my mind, is suggestive of a temporary impinging of painful surfaces. It is easy to understand, also, why protective muscle spasm is absent; contraction of the muscle would only pull the painful greater tuberosity towards the acromion, and cause an increase of pain.

I do not believe that sepsis is an important factor in the ætiology.

Treatment.—Every conceivable form of treatment has been tried and every method has its advocates. The fact that must not be forgotten is that the symptoms disappear in time, with or without treatment; hence any prolonged form of treatment will result in cure.

In the subacute ordinary type it is sufficient to avoid making those movements which give rise to pain. The tender inflamed area, freed from repeated trauma, will return to normal and become painless; and the patient will lose his symptoms.

In the hyperacute type treatment is a problem. Many surgeons favour immediate operation, and it is stated that this method of treatment never fails. In 1924, Carnett [5], professor of surgery in Philadelphia General Hospital, reported having operated on nineteen patients. He split the deltoid and incised the thin wall of the subdeltoid bursa, the walls of which were rarely abnormal; he then cut through the floor of the bursa over the calcareous deposits, which could be felt or seen as a greyish patch, on to the insertion of the supraspinatus tendon. The major portion of the calcareous deposit could be lifted out with a blunt spoon, leaving a small cavity. The walls of the cavity were trimmed and obliterated with sutures. Usually he did not sew up either the floor or the roof of the subdeltoid bursa. Following the operation he rested the arm in moderate abduction on a pillow and slung the wrist to the neck with a bandage. Active movements were encouraged from the first and in ten days full abduction was regained. I have quoted Carnett fully because I have no personal experience of operating on these shoulders.

Carnett declared that in the acute cases there was immediate relief from pain and freedom from recurrence; and in the chronic cases the acute exacerbation subsided leaving the old persistent discomfort. Carnett also reported twenty-five cases which were not submitted to operation. These patients had several acute exacerbations, but their symptoms tended to diminish in severity with time. After reading Carnett's article one is left with the impression that operation is a certain cure, but in the majority of instances is unnecessary.

However, in the hyperacute type I suggest that operation is indicated. The only alternative is complete fixation of the shoulder in mild abduction, or perhaps hyperabduction,—certainly not at right angles to the body, because that is the most painful position. This fixation is difficult to obtain except with a plaster cast; the ordinary abduction splint does not maintain sufficient immobility of the joint to render the patient comfortable. So intense is the pain that an anæsthetic is necessary to apply any form of splint to the shoulder-joint, and my view is—doubtless it will be modified as a result of this evening's discussion—that if the symptoms are severe enough to require anæsthesia for the application of a splint, the correct line of treatment is operation. My recollection of the only two patients I have seen with hyperacute symptoms is vivid. These patients were in agony.

It is difficult to explain why removal by operation of the calcareous deposits relieves symptoms.

As soon as the acute symptoms subside, active exercises and heat, applied as radiant heat, hot fomentations, or diathermy, are beneficial. It sometimes happens that, following a severe attack, the shoulder-joint has been immobilized for a while, and in consequence has become short of its full range of movement. In these circumstances mobilization of the joint under anæsthesia is advisable. But I do not believe manipulative surgery is of value in the ordinary type of tendonitis, in which, although full range is possible, movement through a small intermediate portion of that range is painful.

Type 3. *Osteo-arthritis of the Shoulder.*—The symptoms of this type of painful shoulder may develop insidiously but frequently follow over-use of the joint; trauma is rarely a precursor. The patient complains of a dull ache in the arm and stiffness in the shoulder and pain on moving the joint. On examination there is a diffuse slight tenderness over the joint line, the muscles are slightly wasted, and movement in every direction is less full than on the sound side and painful at its extremes. This type differs from the first type in that the symptoms do not directly date from trauma, and there is not the painful check to movements. *Movement is restricted, but only in the last few degrees.* For instance, the patient may be able to put his hands on the back of his head, but the elbow on the affected side will be more forward than on the sound side, and pressure on the elbow to force it backwards is painful and resisted.

The radiogram reveals no abnormality except perhaps areas of rarefaction indefinitely suggestive of cavity formation in the head of the humerus. These apparent cavities are common in the bone-ends of other joints suffering from osteo-arthritis. Lipping is but rarely seen.

I assume the pathogenesis and treatment are the same as for osteo-arthritis elsewhere. Those who believe in a toxic theory will search for and eradicate foci of infection and, in my humble opinion, not do a single jot of good to their patient's shoulder. Even if one is ignorant of the pathogenesis it is pleasant to delude oneself with an explanation, and I assume that for some unknown reason the capsule of the joint loses its elasticity and undergoes slight sclerosis, and consequently when it is stretched it causes pain. Any treatment directed to improving the power of the musculature and the nutrition of the joint will be beneficial. Over-use of the joint must be avoided.

Physiotherapy is valuable; I pin my faith to two lines of treatment—heat in any form, and graduated faradism to the shoulder muscles, particularly the abductors. Some patients prefer ionization. Manipulative surgery has no success.

Type 4. *Subacute Arthritis of the Shoulder.*—This type may develop insidiously, or may follow trauma or over-use of the arm. The symptoms are pain whenever the joint is moved actively or passively. Providing the patient keeps the joint at rest there is but little discomfort. When attempts are made to move the shoulder-joint the muscles go into spasm and the humerus and scapula move as one. Rotation is naturally more painful than abduction, because the scapula cannot so easily follow rotatory movements of the humerus as it can pure abduction. Muscle wasting is more obvious than in the other types of painful shoulder. Radiograms reveal bone atrophy. This type of painful shoulder is infective, and the signs are those of an arthritis. Many of these patients, in contradistinction to the osteo-arthritic patients, are benefited by the removal of septic foci. Local treatment consists in rest to the joint, preferably on an abduction splint, which should be retained until muscle spasm disappears and the patient regains the power of elevating the arm from the splint. Active movements and faradism to the muscles are encouraged as soon as the patient will tolerate them.

REFERENCES.

[1] DUPLAY, *Arch. gén. de Méd.*, Paris, 1872. [2] CODMAN, E. A., *Boston Med. and Surg. Journ.*, 1906. [3] PAINTER, quoted by Carnett. [4] MOSCHOWITZ, quoted by Carnett. [5] CARMETT, J., *Trans. Coll. Phys., Philadelphia*, 1924, xlvi, 181.

Mr. Alan H. Todd said that in 1919 he investigated the results of the treatment of dislocated shoulders[1]; he found that all the young patients soon recovered normal function, and it made no difference whether they were bandaged, or had massage, after reduction or not; but in persons aged 40 or over there was an important difference. When these persons had their arms bound to their sides, after reduction, the ensuing disability lasted just twice as long as it did when they were treated by an abduction-splint. A far more serious difference (because it was permanent) was that scarcely anyone who had had the arm bandaged down ever recovered the ability to raise it above the horizontal level again Of 24 patients, all but one aged over 50, not one was fit for work after 22 weeks' treatment, and only one could lift his arm above shoulder-level; while at least six were permanently unfitted for any kind of manual labour. This disability meant a grave diminution of earning capacity to many men, and loss of occupation to some. It was the result not of the injury, but of wrong treatment; the profession was not curing disability, but actually producing it. When the arm was not tied down, the limitation did not ensue. Yet this method was advised in every standard English text-book, and was universally taught and practised. Surgeons should scrutinize their results more closely, and they would then be convinced that abduction was the proper method. There was no risk of recurrence of dislocation with this method; no single case had occurred in his series and there was no reason why it should; abduction brought the edges of the torn inferior part of the capsule, into apposition, and rendered tense the muscles that mainly supported the shoulder-joint; it also prevented the lax, redundant folds of the under part of the synovial membrane becoming bunched together into an adherent mass. He was not arguing on any theoretical grounds in favour of the abduction-method of treating shoulder-injuries; it was a fact, which anyone could verify, that this method gave far better results, in far less time, than the stereotyped methods. Not only dislocations, but also all serious injuries about the shoulder, should be treated in abduction, preferably on a splint which took its bearing on the iliac crest, this being the natural way of supporting the weight of the arm.

One reason why the results were bad was that these cases were left to inexperienced juniors, or even to inadequately-supervised masseuses; another reason was that they were not admitted to hospital; general hospitals could not admit them, it was true, but the infirmaries could, and if proper abduction-treatment were there given, the present shocking financial losses incurred in these cases would cease. The ideal was hyper-abduction, in bed; the next-best course was out-patient treatment on an abduction-splint.

It was important to appreciate that the abduction-method was indicated, not only in dislocations, but also in most other injuries of the shoulder. A simple fall on the shoulder, in an elderly or even a middle-aged man, was always a serious injury; every such case should be treated by abduction, or there would be grave likelihood of permanent impairment of function. The curious fact emerged, from a recent study of private cases, that the worst examples of limitation of movement were often the result of comparatively mild injuries; a fall of only 6-8 ft. was a frequent cause of inability to lift the arm above shoulder-level. A slight fall seemed to involve a greater danger than a fall of 30 ft. or so, perhaps because, being regarded as a minor accident, it was treated less assiduously. The practice of tying the arm down, or leaving it lying in a sling, had only one thing to recommend it, namely, that it was the easiest way of giving rest; but for elderly persons *it was always a dangerous method.*

It was a mistake to regard even a coarse grating at the shoulder as necessarily indicating osteo-arthritis. Osteo-arthritis was the commonest cause of such grating,

[1] See *Practitioner*, 1920, civ, 186-196.

but traumatic inflammatory infiltration of the deltoid muscle might produce the same sign. Usually this grating was felt about one inch external to the origin of the muscle, and probably it was interfascicular; the importance of recognizing this fact was that such grating often disappeared completely within a few months of the injury, leaving no disability; if it were regarded as necessarily indicating osteo-arthritis, a wholly wrong prognosis might be given.

Osteo-arthritis in the shoulder, following injury, seemed to show a greater tendency to become progressive than it did in any other joint; also, statistics showed that it was very liable to occur after comparatively mild falls upon the point of the shoulder; the more severe falls produced other injuries, but did not often originate osteo-arthritis, though they might aggravate it, if it already existed.

Dr. Alex. Mercer (introduced by Mr. ALAN TODD), said: As surgeon to a large insurance corporation specializing in workmen's compensation cases, I have during the past twenty years examined 844 cases of stiff shoulders, definitely the result of contusions and strains. This does not include stiffness due to gross lesions such as fractures, dislocations and nerve injuries, or cases in which definite arthritic changes were present. Nor does it include cases that recovered within one month of the accident.

The cases were sent to me at intervals of one month until either recovered or settled on a permanent disability basis.

Out of the 844 cases, only 98 patients (less than 12 per cent.) recovered for full work within three months of the accident; 272 (less than 31 per cent.) recovered for light work within three months; 379, or about 45 per cent., ended in partial permanent incapacity; and 95, or about 11 per cent., ended in total permanent incapacity. Thus over half ended in permanent incapacity, that is, broadly speaking, were unable to raise their arms above shoulder level.

Practically all cases were treated at general hospitals, treatment taking the form of movements, graduated or forcible, under an anæsthetic, massage, electricity, and radiant heat in varying proportions.

As my figures show, the result of such treatment was disappointing. In many cases it appeared to be futile and even retrograde. Much of course depends upon the patient's response to treatment of this kind, but even so one would expect better results. The voluntary and involuntary resistance of the patient is greatest just after the accident, and it is then that drastic measures are likely to succeed and not later when adhesions form a real mechanical obstacle, often of an intractable nature. Too often the feelings of a patient are considered and the treatment is started too casually, or too carefully. The amount of stiffness that sometimes results from a comparatively slight injury is surprising; this is mainly due to overmuch rest treatment. Usually mild exercises of short duration are instituted with long periods of rest, i.e., with the arm lying dependent or resting on a sling.

I suggest that when a case is seen early and definitely diagnosed as a contusion or strain, the treatment should be begun by fixing the arm in as abducted a position as possible. If necessary, an anæsthetic should be given. The abduction should be at the shoulder-joint independently of scapular movement. After a variable period, depending on the amount of pain, gentle movements should be initiated, after which the arm should again be fixed in the abducted position. Even where adhesions are forming or have formed, as elevated a position as possible should be aimed at. The position is not very comfortable and may not be liked by the patient, but this is a detail compared with the discomforts he would experience later from the treatment of contracted adhesions. It is important that external rotation should be combined with the abduction. Movements are more likely to succeed with the patient in the recumbent position, i.e., the fixing of the arm should be against the force of gravity, and the movements should be mainly in the direction of that force.

Mr. John Robert Lee said: In this discussion it is well to have in mind the anatomy of the shoulder-joint. It is a joint which is muscularly strong and freely movable. Some of the muscles in close relation to it are tendinous, and the deltoid has intramuscular septa. In these tissues the nerves terminate by end-organs and there is a comparatively poor blood supply. There is a large cul-de-sac of the capsule at its inferior surface. When the arm is at the side this sac is folded with its surfaces in apposition, but when the arm is abducted and partly elevated, the capsule is tense. I agree with Mr. Todd's method of treatment of dislocation, and since 1919 I have adopted treatment with an abduction splint combined with early massage and movements in all cases. Much better results are obtained because dense adhesions are not formed in and around the capsule, and it is easy for patients to move the joint with the aid of gravity instead of having to overcome marked adhesions in the capsule, and to move the limb against gravity.

It should be the rule in hospitals that all patients are seen regularly by the surgeon in charge. Unfortunately many, especially workmen, are lost sight of for weeks in the massage and electrical departments, and develop "neurasthenic" symptoms—pain, stiffness and impaired movements persist. In many cases suitable work is the best treatment.

Some apparently not severe cases of trauma of the shoulder may have bony damage. A patient slipped and fell on his shoulder; there was swelling and subcutaneous hæmorrhage. An X-ray revealed small areas of "crushed" fracture of the great tuberosity. In another patient swelling of the deltoid persisted for months after the injury. These patients complained of pain in the shoulder, with limitation of movements similar to those described to Mr. Perkins. Many cases of painful shoulder are due to fibrosis affecting tendons, tendon sheaths and intramuscular septa. Histological examination of tissue removed from the latter case above referred to showed the formation of fibrous tissue, in which were giant cells. Two other patients gave a history of aching in the shoulder-joint previous to a comparatively trivial injury. X-rays revealed in one case a spontaneous fracture, with a myeloma at the head of the humerus, and in the other hydatid disease involving the upper end of that bone.

Mr. Whitchurch Howell said he recalled the case of a man who had been unable to sleep resting on the left shoulder owing to severe pain in it. The position most comfortable to him was with the arm abducted and externally rotated. A skiagram showed calcareous plaques, nodular, not facetted, elliptical in distribution, lying on the floor of the subdeltoid bursa or immediately beneath it. Their presence was proved by operation, and a complete cure resulted. Microscopical sections showed a calcareous deposit and no sign of tubercle.

With regard to pain in other regions of the shoulder-joint, that in the region of the tendon of the biceps in the bicipital groove was fairly common, and was sometimes associated with subdeltoid bursitis, probably because of the closeness of this tendon to the supraspinatus tendon. Manipulation generally cured this, the manœuvre being that of complete abduction and hyperextension of the shoulder, together with extension and supination at the elbow.

Rest often cured minor pains and injuries of the shoulder-joint. But not always; for example, there was sometimes pain after a Colles fracture: in one variety this occurred twenty-four to thirty-six hours after the injury, and was due to bruising of the glenoid cartilage; in the other the pain appeared about three weeks afterwards, and was due to adhesions, the result of immobilization of the joint.

Manipulation cured certain cases. In his opinion, nitrous oxide anæsthesia did not secure sufficient muscular relaxation, and a general anæsthetic was essential. Even a dislocation of the shoulder-joint might not be reduced except under general anæsthesia.

In progressive arthritis of the shoulder-joint the problem was even greater. With increasing pain and stiffness should one perform an arthrodesis or an arthroplasty ?

Mr. McCrae Aitken said that the President and himself had served their apprenticeship in a seaport, where they saw sailors who, when rounding Cape Horn, had received injuries to the shoulder, and had been lying in their bunks ever since, and doing no work. Hence, when seen in this country, they had a "stiff shoulder." And that matter—the shoulder stiff from disuse—had been raised in to-night's discussion; it was not the painful shoulder to which Mr. Perkins alluded. All orthopædic surgeons had been puzzled by shoulder cases, and he (the speaker) thought it would be valuable to return to the subject in about three years' time, when, perhaps, the sub-division now submitted would not be considered adequate. He did not know the meaning of "osteo-arthritis" or the exact meaning of "subacute arthritis." In studying out-of-the-way painful shoulders which he saw, it was strange how often there was only one movement, or only one section of a movement, which was painful. Just as a bricklayer who ceased work for a time and then resumed, contracted inflammation in those tendons of the thumb used for the bricklayer's peculiar motion, so it was possible for there to be inflammation round the tendons of the shoulder.

He thought the word "tendonitis" should be "tendinitis." He was not sure that a tendon which contained only a few blood-vessels was a tissue in which a true inflammatory reaction would occur. He agreed that curious little calcareous deposits occurred. There was no proof that they were in the tendon; his view was that they were in the sheath, hence the word to employ was teno-synovitis. There were internal and external rotators, etc., all of which might be subjected to sudden strain and injury, and deposits might occur.

He was sorry to see the acromio-clavicular joint relegated to such a subordinate position, as he was not assured in his own mind about what happened to it, and to the triangular meniscus which hung inside it. These points might well be discussed on a future occasion.

He remembered a Canadian lumberman, with magnificent muscles, who, four years ago, when at work with the logs, wrenched his shoulder badly. The pain was definitely situated at the origin of the deltoid, opposite the tip of the acromion process. The skiagram showed spiculation along the origin of the deltoid, where he thought there was a tear. If such could occur, could not there equally well be similar injuries about the numerous other shoulder-joint muscles ?

One kind of case not included in the paper was the painful shoulder manifest a few weeks after an injury. When working with Sir Robert Jones he had seen a number of cases in which there had been a fall on the elbow or wrist, and perhaps a Colles' fracture had resulted. Recovery seemed to take place within a fortnight, but three or four weeks after the injury the patient had begun to have pain, stiffness, and inability to use the shoulder-joint. He did not think that kind of case had found its way into the books. The explanation given by Sir Robert Jones and himself was, that it was, so to speak, like the case of the corneal ulcer; at the original fall, on the outstretched hand, the joint cartilage was contused, and not until a new vascularization had crept into the injured cartilage did the joint become painful or tender. In the case of the injury of the cornea which caused the ulcer not much attention was paid to it at first, but when more vessels were obvious there was pain and a blurring of vision. There was a type of contusion of articular cartilage which caused a late or diffused pain and disability, and the right treatment for this was absolute rest until the disability disappeared.

Dr. James F. Brailsford said: For the purpose of this discussion radiographs were examined of 347 patients who have come for X-ray examination during the past three years on account of pain in the region of the shoulder-joint.

RADIOGRAPHIC FINDINGS IN 347 PAINFUL SHOULDERS.

	The Queen's Hospital, Birmingham	The Royal Cripples Hospitals, Birmingham	St. Chad's Hospital	Pensions Hospitals	Total
Total	**151**	**52**	**53**	**91**	**347**
Normal	90	20	19	48	177
Arthritic changes (acromio-clavicular joint)	23	8	10	10	51
Arthritic changes (shoulder-joint)	6	3	4	11	24
Loose bodies	2	4	4	1	11
Old fracture	11	6	7	(not included)	24
Unreduced dislocation (acromio-clavicular joint)	4	—	4	,,	8
Unreduced dislocation (shoulder-joint)	2	—	2	,,	4
Tubercle	4	8	1	16	29
Cyst of humerus	—	1	1	—	2
Carcinoma	2	—	—	—	2
Sarcoma	2	1	—	1	4
Myeloma	2	—	—	—	2
Exostosis	2	—	—	—	2
Osteomyelitis with arthritis	1	1	—	5	7

Patients attending casualty with recent injuries are not included, also many patients in whom the symptoms appeared to be due to toxic absorption. Many of these were sent for radiography of the teeth, sinuses, etc., and definite septic foci were found in a large number. The chart shows that about 50 per cent. of the painful shoulders showed no abnormality on the radiographs. The commonest lesions shown were arthritic changes in the acromio-clavicular joint. The "cavities" in the humerus head are normal varieties of the cancellous structure often found in joints in which there is no suggestion of arthritis and not to be considered as indicative of osteo-arthritis. Only one patient showed marked osteo-arthritis of the shoulder-joint as evidenced by sclerosis of the subarticular bone and erosion and eburnation of the articular surfaces. In every case in which loose bodies were seen, there was definite evidence of severe trauma: in 5 cases the shoulder-joint had been dislocated. Rice bodies are usually not shown on the radiograph. The fractures and dislocations enumerated were unsuspected until radiographs were made as the shoulder pain and stiffness persisted. No evidence of inflamed bursæ is obtained from the radiograph until calcium has been deposited. In two cases, one of calcified cysticerci, and the other of trichinosis, the patients complained of rheumatic pains.

Mr. W. Rowley Bristow said he understood Dr. Mercer to say that of 844 cases of trifling injury to the shoulder-joint, apart from fracture, 45 per cent. resulted in permanent partial incapacity, and 11 per cent. in permanent total incapacity. This fact, if substantiated, should be broadcast, and an investigation made into this high percentage of incapacity following trifling injuries.

Most Members of the Section would agree with Mr. Todd that shoulder injuries required treatment in abduction.

Dr. Kingston Barton said he spoke as a general practitioner who had had to deal with injuries in the days before X-rays. The practitioner was frequently unable to make a correct diagnosis. He (the speaker) remembered many cases of the kind to which Mr. Aitken had referred, in which trouble in the shoulder did not begin until two or three weeks after an injury.

In the old days the surgeon used to keep the limb quiet for too long. The general practitioner saw many cases which never went near a surgeon, and many of them had been merely cases of acute inflammations of the numerous fibrous branches in the deltoid muscle, or acute rheumatic inflammations in the long tendon of the biceps. These had been quickly cured by the homely mustard plaster, and by Sir William Gowers' treatment for lumbago, which consisted of large doses of salicylate of soda, with double doses of bicarbonate of soda.

Another class of case was that in which there were injuries or neuritis of the brachial plexus, particularly a single neuritis of the circumflex nerve. Within from three to six weeks muscle wasting would be evident. They recovered under massage treatment.

With regard to osteo-arthritis, he agreed that the orthopædic surgeon would be well rid of these cases except when only one joint was involved, when the surgeon could no doubt prove a special cause. If more than one joint was affected, the case came rather into the province of the balneologist.

The Section should insist on the importance of early diagnosis, and disseminate the appropriate treatment.

Mr. Naughton Dunn (President) showed two skiagrams of patients who complained of pain in the shoulder.

He said he did not know whether Members had clear ideas on the subject of stiff and painful shoulder; he certainly had not. Painful shoulder was the greatest bugbear of the orthopædic surgeon. Some cases might seem trivial to the surgeon, but to the patient the condition was very important. Was it inflammatory or was it due to adhesions, perhaps following a previous inflammation? Whatever the diagnosis, the need was to recognize the state of affairs and so to treat it as to relieve disability. One of the points to be satisfied about was whether the patient could lie with comfort on the shoulder. If he could, the condition probably was not inflammatory. Were all the joint movements free? If not, there might be adhesions or there might be inflammation. Was movement gradually improving? If so, recovery would be helped by forcing movement.

Mr. Perkins mentioned tenderness in the line of the joint. The shoulder was a difficult joint to palpate. Tenderness in front seemed to be so commonly associated with inflammation of the joint that he thought the commonest cause of painful shoulder was subacute arthritis, which was frequently toxic in origin.

Very few patients were relieved by the breaking down of adhesions; he found that procedure effective only when the patient could lie on the shoulder comfortably and when certain movements only were limited.

With regard to subdeltoid bursitis and calcareous deposits, he had operated on one or two patients who had had acute severe pain, and the results had been very good. The cases on which he had operated were those in which X-rays had shown a deposit in the area. He did not agree that if fluid was present it would lessen the pain.

Mr. Perkins (in reply) said the condition which he described in one part of the paper as a "tendonitis" had to do with the tendon, not with the tendon sheath. One could not have a tenosynovitis where there was no sheath, and he suggested that there was no sheath over the attachment of the supraspinatus muscle. He accepted Mr. Aitken's correction of the word "tendonitis" to "tendinitis."

He agreed with Mr. Aitken that in many cases the lesion was in or around the acromio-clavicular joint. An interesting point was that X-rays had shown the possibility of marked changes in the acromio-clavicular joint, with apparently no symptoms.

He thought it would have been possible to limit the discussion to painful shoulders, following Colles' fracture, because there seemed to be a variety of painful shoulders which could result from a Colles'.

He did not agree with Mr. Whitchurch Howell about the general anæsthetic. In many cases one could not get full range of movement by one manipulation, and if partial movement was successful at one trial under nitrous oxide, it was not difficult to persuade the patient to have another seance; whereas he might refuse another dose of ether or chloroform.

JOINT DISCUSSION No. 1.

Section of Therapeutics and Pharmacology, with the Section of Tropical Diseases and Parasitology.

[*November* 13, 1928.]

DISCUSSION ON THE SPECIAL USES OF ANTIMONY.

Dr. J. B. Christopherson: Though we need not discuss uses which have become obsolete, it may be of interest to indicate some of these discarded practices in a short historical survey.

It is interesting to inquire—how did antimony get into pharmacy at all, and when?

Although in England antimony was of little account as a medicinal agent until the sixteenth century, it has played an interesting and not unimportant part in the evolution of medicine in all epochs since remote times.

In *ancient* history—from prehistoric times to A.D. 200—first as a cosmetic, and later a medicinal preparation for the eyes.

In *medieval* times, A.D. 200 to A.D. 1500—it was the lodestar which drew the oriental alchemists onward in the futile pursuit of the philosopher's stone.

In the period of the revival of Medicine—say A.D. 1500 to A.D. 1825—when chemists and physicians competing with each other in their rivalry, and torturing antimony to find the universal remedy, used it empirically in a great number of diseases.

In *modern* times—from 1825 to the beginning of the present century—a period which marked the gradual eclipse of orally administered antimony under the influence of the cultured Victorian physicians.

And finally, from 1905, to the present day, the period of the scientific administration of antimony. One might be tempted to call the present day the period of the apotheosis of antimony in the department of tropical medicine.

It is important to remember that antimony belongs to the same chemical group as nitrogen, phosphorus, arsenic and bismuth, for this appears to be the answer to the question, why has antimony attracted, till recently, so little attention from the biochemist? No doubt the close chemical relationship between antimony and arsenic led to the mistaken assumption that their biochemical reactions were also similar.

The ancestor of all antimony derivatives is the mineral stibnite ($Sb_2 S_3$). The "Encyclopædia Britannica" is open to all who wish to know whence stibnite (antimony) is now obtained, but the place from which the ancients mined it is of real interest to us. The Assyrians found it near Ispahan in Persia, and were probably the first to introduce el Kohel (antimony) as an article of female adornment—pigment for the eyes. It is still so used by Arab women, and it is still called in the East the eye paint, "el kohel," a word whose root is Syriac, denoting the early Assyrian use of the mineral.

Painting the eyes with a coloured mineral was in vogue 5,000 years ago. Amongst the articles found by Mr. C. Woolley whilst excavating at Ur of the Chaldees—1927-1928—was a little shell containing a green substance, which has not yet been analysed, but it is most probably malachite, emerald green, copper carbonate, $CuCO_3$, which Shub-ad, its owner, used for painting her eyes and eyebrows 5,000 years ago. The "queen," who with her retinue, was buried with the king when he died about 3500 B.C., did not forget to take with her her little eye-paint container which would, as a matter of course, be useful to her in the life beyond the grave. The third daughter of Job, who lived about 1520 B.C., was named Keren Happuch, which, translated, is "the little horn of paint," indicating that the "lipstick" of

1520 B.C. was a dearly-loved and well-established article of the feminine toilet. At a later date, red lead, vermilion (Pb_3O_4), the *minium* of the ancients, was used in the East as a cosmetic for the eyes. When Jezebel tired her hair and "put her eyes in painting" on hearing that Jehu was approaching Jezreel, she probably used antimony sulphide (884 B.C.) When vermilion or minium went out of vogue as a cosmetic for the eyes antimony sulphide took its place, and an interesting suggested derivation of the word antimony is anti-minium, anti-monium, the pigment which took the place of minium or vermilion. This is not accepted by all philologists.

Early in the sixth century B.C., antimony (antimony sulphide—el kohel) had taken the place of vermilion (minium). In Ezekiel xxiii. 40, "paintedst thy eyes," "kahalt eynaik," is literally "*koledst* thy two eyes," or "*antimonedst* thy two eyes" (Armbruster). Ezekiel, in his youth, was taken as a captive to Assyria with Jehoiachim in 597 B.C.,[1] and no doubt the Israelites learned from the Assyrians the antimony eye-paint. The ancient use of the mineral antimony for painting the eyes is interesting. It somewhat naturally was the first step towards its therapeutic use, as a remedy for the well-known eastern malady—granular eyelids. Pliny the Elder (A.D. 23-79) describes the seven remedies in use at his period derived from antimony; they were for local use only, as astringents for discharges and ulcers of the eyes, for burns, for recent wounds and dog bites, and, it was thought, to arrest discharges of blood from the brain. In Pliny's time, a preparation used by women for dilating the pupils consisted of antimony. The word "antimonium" is the transcription of the Arabic (uθmud) into Latin and is first found in Latin translations by Constantine the African, who died A.D. 1087. In the "Liber de Gradibus," by Ibn al Jazzar, who died at Cairouan, Tunis, A.D. 1004, antimony is recommended for granulations of the eyelids. A native of Carthage and a Moslem, he came to Salerno about A.D. 1070. When the Normans captured Salerno in 1076, Constantine became secretary to Robert Guiscard. Ultimately becoming a monk he translated Arabic medical works into "poor Latin."

Antimony, however, had not yet attracted the notice of English physicians, and only towards the end of the fifteenth century does the word occur in English literature in the "Ordinal of Alchemy," by John Norton, of Bristol, 1477. In fact the alchemists, who were busy in the pursuit of the philosopher's stone at this period, were now eagerly investigating its chemical reactions. The use of antimony as an *internal* therapeutic remedy dates from the first half of the sixteenth century when the famous German physician, Paracelsus (1490-1541)[2] introduced a complex antimonial preparation for internal use—Mercurius Vitæ. Mercurius Vitæ, which did not contain mercury, marks an epoch in the therapeutic history of antimony. Paracelsus pointed out that diseases had specific remedies, and he strongly recommended Mercurius Vitæ for the "Plague." He also expounded the view that the proper sphere of alchemists was the production of medicinal preparations and not the pursuit of the philosopher's stone. It would be true to say that about this time (of Paracelsus) the alchemists began to influence medicine deeply. They appear at first to have practised medicine independently, however, selling their remedies to whoever would buy them. Mercurius Vitæ was the antimonial of the sixteenth century. It was not, however, until 1604, when a German, John Tolde, of Huss, wrote the work "The Triumphal Car of Antimony" under the pseudonym of Basil Valentin, a supposed Dominican monk, that antimony, as a remedy, attracted any great attention from the physician and passed into the pharmacopœia. There is no doubt that in 1604 antimony was launched on an exciting therapeutic career which lasted 400 years, experiencing vogue, popularity, use, abuse, and, finally, almost total eclipse.

1 J. Taylor, in Hasting's "Dictionary of the Bible."
2 *Paracelsus*, "superior to Celsus," born at Eidseideln, Schwyz, 1490.

The introduction of Pulvis Algarotti (effectively Paracelsian Mercurius Vitæ) followed in 1607. In 1619 the Earl of Warwick's powder, consisting of scammony, antimony sulphide and cream of tartar, the precursor of tartar emetic, appeared in Italy.[3] In 1631 Adrian Mynsicht, physician and chemist to the Duke of Mechlenberg, introduced tartar emetic—the most important of all antimony derivatives. Amongst illustrious patients who benefited by it in its early days was Louis XIV, then aged 19 (1657). Seized at Calais with an illness which baffled his physicians, he consulted an alchemist at Abbeville, who is said to have cured him with tartar emetic. There should have been some difficulty in consulting the quack at Abbeville in 1657 for the parliamentary ban on antimony was at the time in force in France. About the period (1566) antimony had been the subject of violent controversy amongst physicians, especially in France, where the Parisian Parliament, advised by the College of Physicians, banned it for 100 years (1566-1666). In the meanwhile, about 1651, J. R. Glauber produced Kermes mineral, the most famous of antimony preparations. Glauber, a Bavarian chemist, had settled in Amsterdam and made his living by the sale of secret preparations, including the celebrated Glauber salts. He compounded certain drugs prepared from metals such as antimony. Kermes mineral was an orange red powder of not very constant composition, a mixture of the oxides and sulphides of antimony. It had a considerable reputation and is represented in the current B. P. (1914) as Antimonium Sulphuratum and Pulvis Antimonialis, and is still official in P. Française and P. Belgique. When Glauber died, the secret of Kermes mineral was disclosed by his pupil to Dr. Chastenay, who revealed it to the surgeon La Ligirie, who communicated it to a Carthusian monk, Brother Simon, who gave the drug to his brother monks with success, and it became known as Poudre de Chatres, and was the most popular remedy of the time for smallpox, ague, dropsy, syphilis. Fifty years after Glauber's death, Louis XIV (1638-1715), who 63 years previously had been cured by tartar emetic, bought the Kermes mineral prescription from La Ligirie for a considerable sum.[4] In 1802, Proust (Christison's "Dispensatory," 1802) obtained sesquioxide of antimony (Sb_2O_3 pure) which was introduced into the Pharmacopœias of Scotland and Ireland, as Christison states (1842), partly as a good antimonial and partly because it must be made in the composition of tartar emetic and of *James' Powder* (Pulvis Antimonialis). There are two famous antimony preparations which originated about the middle of the eighteenth century: James' Powder and Plummer's Pill. The exact composition of James' powder has never been disclosed. Pulvis antimonialis was the official substitute introduced into the London Pharmacopœia, 1787, and is still in the current British Pharmacopœia, 1914; but Christison wrote that "the substitute was inferior in activity as well as certainty" to the original James' Powder. Dr. Robert James, who died in 1773, was a life-long friend of Dr. Johnson with whom he had been at school at Lichfield. He practised medicine (latterly) at Craven Street, Strand. He patented his fever powder, which acquired a great reputation amongst physicians, in 1747. He disclosed the prescription only to a bookseller named Newbery who lived in St. Paul's Churchyard and who was his partner and sold the patent medicine. It is an interesting fact that the successors of the original Newbery firm are in existence at 27, Charterhouse Square —Newbery, Francis and Sons, Druggists—and they still issue James' Powder in packets of two sizes. Its official name is Pulvis Jacobi Vera. Dr. Plummer[5] was Professor of Chemistry at Edinburgh University when James was practising in London. He published his antimony pill in "Edinburgh Medical Essays," 1751. It is mentioned in Martindale's Extra Pharmacopœia, 1928, and Dr. Martindale tells me it is still in demand, though he did not disclose who was now taking it. In the current

3 In 1619 a Pisan physician named Cornaelini (Marco) published at Florence a work which he dedicated to Sir Robert Dudley, "Earl of Warwick." He says Dudley invented the powder.

4 A. C. Wootton, "Chr. of Pharmacy," 1910.

5 Plummer's Pill—calomel and antimony sulphide.

British Pharmacopœia, Antimonium Sulphuratum, Pulv. Antimonialis and Plummer's Pill may be considered the direct descendants of Kermes Mineral of Glauber.

I will but briefly name the "antimony cups," chiefly used in Germany in the sixteenth and seventeenth centuries but persisting in England until the middle of the eighteenth century. They were made of antimony and tin, wine standing in them became impregnated with tartar emetic—hence the name poculum emeticum.[6] They were used in monasteries for brothers who had dined too well. Vinum antimoniale originated in this way. There was also the "everlasting pill," of about the same period, a family institution. Antimony pills were made of an alloy of antimony which became slightly oxidized when the pill passed through the intestine and acquired a medicinal effect. The same pill could be taken many times and kept in a drawer when not being used. There was also the piece of "glass of antimony" (crude antimony heated in a crucible), a mixture of oxides and sulphides, a German device which was hired out to customers to stand in wine to which it communicated an emetic property. (Angelo Sala quoted by Wootton.)

It is unnecessary to pursue in detail the history of medicinal antimony during the nineteenth century. Pulv. antimonialis sulph. was occasionally used for skin diseases and syphilis, pulv. antimonialis (antimony oxide Sb_2O_3) as an expectorant in cough mixtures, and tartar emetic as a diaphoretic and emetic.

In 1905 antimony as a therapeutic agent was almost extinct when H. W. Thomas at the School of Tropical Medicine, Liverpool, commenced investigations on trypanosome-infected animals with atoxyl (sod. arsanilate).[7] In 1906 Nicolle and Mesnil (*Annales de l'Institut Pasteur*, July, 1906), proposed the use of *antimony* salts in tropical medicine for trypanosomiasis, as atoxyl, the arsenical salt, had proved not satisfactory. In the same year Plimmer and Thomson at the Lister Institute tried potassium antimony tartrate on the suggestion of Cushing, who was supplying them with experimental drugs. They were the first to investigate the action of antimonyl tartrates on trypanosomiasis in animals, and found that of all the various drugs used, including atoxyl, sodium antimony tartrate had the most marked influence on trypanosomes in the living body. (They also tried sodium arsenyl tartrate and found that it had not anything like the same effect on the trypanosomes as the antimony salt.) They also made the important discovery that an atoxyl-proof race of trypanosomes could be transferred to another animal and was not antimony-proof. F. Mesnil and (E.) Brimont in France independently at this time were investigating the action of potassium, antimonyl-tartrate on trypanosomes. British and French investigators came to the conclusion that further investigations in trypanosomiasis were indicated. In 1910 certain organic compounds of antimony, e.g., sodium antimony thioglycollate, were advocated on the ground that the parasites (trypanosomes) did not become immune with antimony so readily as with arsenic.

The Intravenous Route.—Antimony tartrate was administered subcutaneously by the first investigators; this, of course, was followed by local sloughing and abscess. Oral administration did not prove any more satisfactory. But in 1906 a discovery of the highest value was made. It is recorded by G. C. Low in his useful paper on "The History of the Use of Intravenous Injections of Antimonium Tartratum.[8] Broden and Rodhain[9], working in the Congo, discovered the intravenous route for administering antimony tartrate by very careful work along a

6 NOTE.—*Antimonyall Cupps.* Pocula Emetica or Calices Vomitorii. Under this title Sir StClair Thomson described two antimony cups in a paper read before the Section of the History of Medicine, March 17, 1925. An excellent coloured illustration accompanies the paper (see *Proc. Roy. Soc. Med.*, 1925, xix (Sect. Hist.), 123.

7 Arsamin, Soamin.

8 *Trans. Soc. Trop. Med. & Hyg.*, Dec., 1915, 38.

9 "Maladie du Sommeil," *Arch. für Schiffs- und Tropen-hygiene*, 1906, x, 693; 1901, xi, 73; 1907, xi, 336; 1908, xii, Heft 14.

line of investigation which appeared impossible at that time. This discovery was not without other claimants. With or without arsenic, antimony has not established itself in the premier place as a remedy for human trypanosomiasis.

Leishmaniasis.—In April, 1912, Gaspar Vianna (at a meeting of the Dermatological Society of Brazil) showed cases of cutaneous leishmaniasis cured by intravenous potassium antimony tartrate, previously treated by arsenicals ("606") unsuccessfully. The fact that trypanosomiasis and leishmaniasis were both protozoal diseases prompted Vianna to try antimony on the latter. Three cases were cured, and Vianna noted that tartar emetic given intravenously in "cutaneous leishmaniasis" is a powerful remedy if not specific. In 1915, following the publication of Gaspar Vianna's cases of Brazilian dermal leishmaniasis, Di Cristina and Caronia, in Sicily, published cases of infantile leishmaniasis (kala-azar) cured by antimony tartrate. This was confirmed by Jemma and others. About this time Rogers [10] in India, and Castellani,[11] in Ceylon, commenced treating Indian kala-azar by intravenous antimony tartrate. Rogers and Muir published their results in October, 1915, and Rogers suggested the use of 5 per cent. metallic antimony ointment for kala-azar children. Rogers also tried with success colloidal antimony, a less toxic drug than potassium antimony tartrate. In 1917 Christopherson reported cases of "espundia" in the Sudan (naso-oral leishmaniasis) cured by the intravenous injection of potassium antimony tartrate, and three cases of Sudan kala-azar cured by intravenous potassium antimony tartrate. Many other investigators of antimony for leishmaniasis deserve mention, but this account is a bare outline of the subject. The value of organic antimony compounds in leishmaniasis will be dealt with, I hope, by later speakers.

In 1917, Christopherson, in Khartoum, investigated the action of potassium antimony tartrate on bilharzia disease, and in 1919 reported analyses of seventy cases. It appears that Macdonagh, having had experience of antimony in syphilis, had previously tried tartar emetic for bilharzia, but he recorded the fact in his work on syphilis, and did not follow his cases up. It would be impossible to give a complete list of those who have contributed valuable work in the treatment of schistosomiasis: Day, Faust, Cawston, Lasbrey, Coleman, Taylor, Fairley, Low, Manson-Bahr, Khalil, are a few names only.

In 1913, Aragão and Vianna, in Brazil, successfully treated granuloma inguinale (ulcerating granuloma) with intravenous potassium antimony tartrate. This was a work of some importance, for it established the interesting fact that antimony tartrate, which was specific in two protozoal diseases, was also specific in a bacterial infection; later, of course, it was found to be specific also for certain helminthic diseases. If it is specific for the *Diplococcus capsulatum mucosa* of ulcerating granuloma, it is probably specific for Egyptian rhinoscleroma, which is also caused by a *Diplococcus capsulatus mucosa* of the Friedländer type.

Other references consulted and acknowledged with thanks are:—

"A Short History of Medicine," Charles Singer, 1928 (also certain historical typed notes by Dr. Singer).
"Recent Advances in Tropical Medicine," Leonard Rogers, 1928.
"Pharmacology and Therapeutics," Cushing, 9th Ed.
"A Manual of Pharmacology," Sollmann, 3rd Ed.
"History of Organic Compounds," Benjamin Moore, 1916.
Private notes sent me by C. H. Armbruster, O.B.E., M.A.

Professor J. A. Gunn said: Up to about twenty years ago, antimony was employed medicinally either in the form of tartar emetic or of simpler inorganic compounds. The success of organic arsenical compounds led to the investigation of corresponding compounds of antimony, and Dr. Christopherson suggested that I might give a short account of those organic compounds of antimony which have been used therapeutically.

It is interesting, in the first place, to recall certain broad distinctions between arsenical and antimonial compounds which have become manifest during the progress

10 Rogers, *Brit. Med. Journ.*, July 31, 1915, 197.
11 Castellani, in Reports Trop. Dis. Advisory Committee, Ceylon, 2nd half-year, 1914.

of investigations. In the case of arsenic the first organic compounds to be used were pentavalent compounds, e.g., atoxyl; but these were soon replaced by trivalent compounds of the type of salvarsan, though there has been a return to pentavalent compounds, e.g., tryparsamide, for certain therapeutic purposes. In the case of antimony, the converse tendency has been shown. The first compounds to be used were trivalent, e.g., tartar emetic, and, up to the present, pentavalent compounds, corresponding in structure to atoxyl, have tended to displace them, especially in the treatment of leishmaniasis.

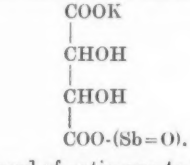
Antimony has shown a much wider range of parasiticidal activity than arsenic. The use of organic compounds of the latter is still, in the main, restricted to diseases of the trypanosome group for which they were originally introduced. Antimony compounds, on the other hand, while still occupying a place in the treatment of trypanosomiasis, have achieved brilliant curative results also in disease of very different origin, such as leishmaniasis, schistosomiasis and granuloma inguinale.

It has not been found—and was perhaps not to be anticipated—that any one compound of antimony should be pre-eminent in the treatment of all these diseases. Also it is well to point out that, so far as laboratory experiments are concerned, they have been almost confined—as they began—to trypanosome infections. It is not possible easily to duplicate bilharzia or leishmania infections in laboratory animals. As there is no exact parallelism between trypanocidal action and toxic action on unrelated parasites, it follows that pharmacological evidence of the type at present available can give only a limited indication even of the tentative therapeutic value of antimony compounds in the various diseases in man for which these compounds have been found useful. The inapplicability of laboratory experiments to the testing of the value of antimony compounds in some of these diseases has proved less of a handicap than might have been supposed, largely because in some of them, e.g., bilharzia, the remedial effects of a drug can be followed in man almost with mathematical precision. Indeed, the results attained in a few years and in spite of attendant difficulties, have constructed one of the most brilliant chapters in the history of therapeutics.

The following account is meant only to show the relationship of those compounds of antimony which have found most favour in therapeutic use. It is not exhaustive, and, for brevity, the compounds mentioned are arranged chemically and no attempt has been made to keep to the exact historical order of their introduction.

A.—Trivalent Compounds.

(1) *Tartar Emetic* or Potassium Antimonyl Tartrate has the formula



Tartar emetic was the first compound of antimony to be used by intravenous injection in man and it is still so used. Similar compounds have been prepared and tried in which (i) the metal potassium has been replaced by some other metal, or (ii) the antimonyl has been combined with a different acid.

(2) *Replacement of Potassium by Other Metals, etc.*—Fargher and Grey (1921) found that the salts of antimonyl tartaric acid form a series in which the toxicity (as determined by the weight of antimony in the M.L.D.) diminishes, while the solubility increases, in the order of potassium, ammonium, sodium, lithium. Thus the sodium salt is only about half as toxic as the potassium salt but is about twelve times as soluble. As it is also cheaper, sodium antimonyl tartrate has largely replaced tartar emetic for intravenous injections. The lithium salt would from these data appear to be even better than the sodium salt, but has not come into therapeutic use, possibly because of the difficulty of obtaining it in crystalline form.

Thomson and Cushny (1910) found that ethyl antimonyl tartrate (in which an alkyl radical is substituted for the potassium of tartar emetic) was powerfully trypanocidal in rats, but it does not seem to have been tried in man.

(3) *Replacement of Tartaric Acid by Other Acids.*—Thomson and Cushny (1910) also investigated antimonyl compounds with acids other than tartaric (e.g. lactate, citrate, malate and mucate) but did not recommend them as superior to tartar emetic.

Voegtlin and Smith (1920) pointed out that antimonyl lactate compares favourably with tartar emetic in respect of the ratio between M.L.D. and minimum effective dose, but the compound has apparently not come into therapeutic use.

(4) Rowntree and Abel, as the result of experiments on trypanosome infections in a variety of laboratory animals, recommended two compounds of antimony with thioglycollic acid, viz., triamide of thioglycollic acid,

$$Sb\ (S.\ CH_2.\ CONH_2)_3$$

and sodium antimonydithioglycollate.

$$Sb \begin{cases} \swarrow S.\ CH_2.\ COONa \\ \searrow S.\ CH_2.\ COO \end{cases}$$

Those compounds do not seem to have been often used medicinally, but Randall (1924) reported good results with them in the treatment of granuloma inguinale.

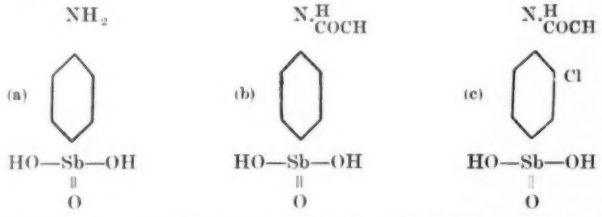
(5) Trivalent compounds in which antimony is united to a benzene ring have not, so far as they have been investigated, displayed qualities which recommend them for clinical trial. They have either been too toxic, or, if not, their trypanocidal activity has been low. Hence, no antimony compounds corresponding to salvarsan, for example, have come into use.

Of the trivalent antimony compounds not one can be said to have superseded sodium (or potassium) antimonyl tartrate. Owing to its cheapness and stability, and also because it is easily procurable, the latter is still the most widely used antimony compound and seems to be superior to any other for the treatment of bilharziasis.

B.—Pentavalent Compounds.

Especially within the last five years or so, a number of pentavalent compounds have been investigated. Clinical reports on some of these show that this type of compound may be superior to the tartar emetic group, especially for the treatment of leishmaniasis.

The pentavalent compounds are nearly all derivatives of paraminophenyl stibonic acid (*a*)



This substance (*a*) is the antimony equivalent of atoxyl: it was found to have a trypanocidal value roughly equal to that of tartar emetic, and was tried clinically, with moderately successful results, under the name "stibamine." The acetyl derivative of the above, viz. (*b*), which is the antimony equivalent of arsacetin, proved more successful, and was used under the names "stibenyl" and "stibacetin." It was, however, the sodium salt of metachlor-para-acetyl amino-phenyl stibonic acid (*c*) which, under the names "von Heyden 471" and "stibosan," first

definitely suggested the superiority of this type of compound in the treatment of kala-azar. Reports by Napier (1923) and others have shown that stibosan is superior to the simpler antimony compounds, especially in cases of this disease which are resistant to sodium antimonyl tartrate.

Other derivatives of para-aminophenyl-stibonic-acid (*a*) have also been tried. A compound with urea, "urea-stibamine," or "stiburea," was introduced by Brahmachari, and another with urea and glucose, "aminostiburea," has also been tested. There is some doubt about the actual structure of these compounds, but they have proved effective in kala-azar.

A glucoside compound of (*a*), "neostam," has been introduced by the Wellcome Research Laboratories. Greig and Kundu (1925) concluded that urea-stibamine, stibosan and neostam are of the same order of efficiency in the treatment of kala-azar.

Napier has more recently (1925-7) tested a compound of para-aminophenyl stibonic acid (*a*) with an amine ("von Heyden 693"), and suggests that it may prove the most useful drug at present available for the treatment of kala-azar.

It is too early yet to pronounce upon the relative values of the pentavalent compounds of this group. No doubt others will be introduced. The evidence so far adduced suggests that there will be a permanent place in therapeutics for at least one of these compounds in addition to sodium antimonyl tartrate.

C.—Dunning and Macht have recently (1927) been testing azo-dyes containing antimony in experimental trypanosomiasis, but these compounds do not seem as yet to have been tried clinically.

Dr. G. Carmichael Low said: In 1916 I wrote a paper on: "The History of the Use of Intravenous Injections of Tartar Emetic (Antimonium tartaratum) in Tropical Medicine."[12] In this it will be seen that Nicolle and Mesnil, Plimmer and Thomson used antimony subcutaneously for the treatment of trypanosomiasis in animals. So successful was this treatment that Sir Patrick Manson, who at this time had two cases of human trypanosomiasis, tried similar subcutaneous injections in them. The sloughing of the tissues was so severe, however, that this method of giving the drug had to be abandoned. Administration by the mouth was then tried, but the vomiting produced rendered this impracticable also. An impasse therefore was reached and remained until Broden and Rodhain tried tartar emetic intravenously in cases of sleeping sickness among natives on the Congo. It was found that the drug did not cause sloughing of the veins and could be tolerated in large doses, even up to as much as $2\frac{1}{2}$ gr. of antimony per dose.

This discovery of the intravenous route revolutionized the treatment of trypanosomiasis, and rendered the application of the drug to other diseases possible. In 1913 tartar emetic was used in leishmaniasis, for the first time, by Machado and Vianna in Brazil. Following this, Aragão and Vianna used it for ulcerating granuloma, again with successful results. Then cutaneous leishmaniasis was treated by the same drug in Brazil, this being followed by the treatment of kala-azar in the Mediterranean basin and India in a similar manner.

The successful results in these diseases prompted others to try antimony in other diseases, and to our President, Dr. Christopherson, fell the distinction of discovering that antimony was efficacious in bilharzial infections.

I do not think sufficient attention has been drawn to the enormous strides made in the treatment of tropical diseases by the discovery of the possibility of giving antimony intravenously. It is extraordinary that a drug which causes severe vomiting when taken by the mouth, and extensive sloughing when introduced into the tissues, should cause no toxic or troublesome symptoms when put directly into the veins. Again, it is extraordinary that the drug should act as a specific in diseases differing so much in their ætiology as trypanosomiasis, leishmaniasis,

ulcerating granuloma and bilharziasis. All these diseases are now brought under control, and the last three can definitely be cured in many instances.

The amounts of antimony given *in toto* are interesting. In a case of ulcerating granuloma, the first treated in England by antimony, Dr. Newham and myself gave a total of 53½ gr. in order to effect a complete cure (*Brit. Med. Journal*, September 16, 1916), while in another case, which clinically looked like an ulcerating granuloma, 114½ gr. in all were given without any improvement taking place. It is possible that this was not a real case of ulcerating granuloma, but it shows what large doses of antimony are tolerated by the system without toxic symptoms or poisoning.

In addition to trypanosomiasis, I have now also treated ulcerating granuloma, American dermal leishmaniasis, kala-azar and many cases of bilharzial disease, the majority of these being definitely cured.

Are the antimony derivatives now used better than the ordinary tartar emetic which we invariably used at first? It is supposed that they are more potent. I have dealt with the question in a paper entitled "A Series of Kala-Azar Cases Treated by Antimony Derivatives."[12] Of those derivatives I personally consider stibamine glucoside, "neostam," to be the best.

Dr. P. H. Manson-Bahr stressed the importance of Dr. Christopherson's work on the action of intravenous injections of tartar emetic in bilharziasis; he felt certain that not sufficient credit had been given for this notable contribution to the therapeutics of a disease which, until 1917, had been considered incurable. When one recalled the trepidation and caution with which intravenous injections of antimony had been given before that date, it marked a distinct advance that it had been shown how progressively increasing and cumulative doses of antimony exerted an increasingly lethal effect upon the adult schistosomes. The alarming reactions which used to follow intravenous injections of tartar emetic were due to the amount of vehicle (in those days normal saline) injected to dilute the drug. Since it had become customary to dissolve antimony tartrate in smaller amounts of distilled water (5-10 c.c.) the toxic reactions following injection had been greatly mitigated, or had entirely disappeared. He (Dr. Manson-Bahr) believed that the practice of dissolving antimony tartrate in 5 per cent. glucose solution was also an advance, for in some manner or another the toxic manifestations—joint pains and hyperæsthesia of muscles—were thereby averted. The pentavalent compounds of antimony were of no use whatever in treating bilharziasis, probably because the action of tartar emetic upon the adult worms were an index of toxicity. It was interesting to note that superheating of the antimony tartrate solution rendered it, according to the experimental work of Fairley, singularly inoperative on schistosomes, and that was a point which required further investigation. At any rate, it was not sound practice to overheat the more highly sensitive pentavalent compounds used in the treatment of kala-azar. That the introduction of these latter compounds was a distinct advance in the treatment of this disease there could be little doubt, but it was difficult to understand exactly how they acted, seeing that it was possible to recover the Leishman-Donovan bodies from the spleen during the whole course of treatment and to prove that they were still viable. It was difficult, too, to understand the rationale of urea stibamine (stiburea) which did not appear to be a true chemical compound, but which had a most definite curative action in kala-azar, as he (the speaker) had proved lately in a case which had resisted treatment with tartar emetic and stibamine glucoside. In ulcerating granuloma the pentavalent antimony compounds were as efficacious as tartar emetic, if not more so, but large doses were required before a cure could be registered, and often it was necessary to continue injections intravenously for a year or more till sound healing could be obtained; it was also essential that treatment should be continuous. In resistant cases of kala-azar and ulcerating

12 *Journ. of State Med.*, 1927, xxxv, 591-594.

granuloma it might be necessary to push the pentavalent compounds to the limit of tolerance, even to 0·6 grm., at which point toxic symptoms supervened, and, as MacArthur had pointed out, often resistant cases yielded best to these almost heroic doses.

Professor Sir Aldo Castellani said: I have been interested in antimony therapy for a number of years and, so far as I know, I was the first to introduce it in the treatment of kala-azar.

I should like to call attention to the phosphorus treatment of cutaneous leishmaniosis which I introduced some years ago. The phosphorus is given subcutaneously in the form of ol. phosph. B.P.; if the lesions are ulcerated the phosphorus oil is simply applied to the sores. I have found that phosphorus oil may be given by hypodermic injection (1-5 minims) without causing any severe pain.

Mr. A. H. Harkness said: I have treated a large number of cases of bilharzia with sodium antimony tartrate and consider that, provided the correct dosage is used, there are no dangers and the cures are permanent. In 1922 I had a fatal case due to an error of the dispenser. The patient, a male, aged 24, complained of hæmaturia. A diagnosis of bilharziasis was made by Mr. Clifford Morson on cystoscopic examination and was confirmed by the finding of ova in the urine.

Intravenous injections of sodium antimony tartrate were begun on April 20, 1922. The injections were given three times a week; the first injection contained gr. $\frac{1}{2}$ of the tartrate and the dosage was increased by gr. $\frac{1}{2}$ at each subsequent injection to a maximum of gr. $2\frac{1}{2}$.

The patient made no complaints after the first four injections. Before the fifth injection was given the urine did not contain blood and the condition of the patient had improved considerably. The fifth injection was given at 5 p.m.

The solution given to me by the dispenser was perfectly clear, but unfortunately contained gr. 25 to 10 c.c. When I had injected about 6 c.c. of the solution the needle was withdrawn as the patient coughed considerably and became cyanosed. He suffered from nausea and vomiting, and complained of pain of a diffuse burning nature in the substernal and epigastric regions. Two minutes after the injection, 180 c.c. of urine were passed which contained nearly a grain of sodium antimony tartrate. The mental condition was clear and the temperature subnormal. During the night there were several loose motions which did not contain blood. He was given copious amounts of fluids, rectal salines, and one ounce of brandy four-hourly. He also had 6 per cent. glucose injected subcutaneously.

On the following morning he said that he felt much fitter, but at midday he began to vomit more frequently and the vomit contained blood. An hour later blood appeared in the urine. In the evening the cyanosis became more marked. Substernal and epigastric pain was more severe and for the first time he complained of a very dry mouth. Diarrhœa became less marked.

During the night he became worse. Vomiting continued with severe hæmatemesis and the urine was loaded with blood. He died at 6.30 p.m., $37\frac{1}{2}$ hours after the last injection.

Urine (180 c.c. passed two minutes after injection) contained 0·92 gr. of sodium antimony tartrate. The liver contained 0·57 gr., and the kidneys and heart both contained traces. Dr. Roche Lynch, who kindly did the estimations in 1922, now tells me that the method gave results on the low side. The liver, heart and kidneys showed well-marked fatty degeneration.

Section of Epidemiology and State Medicine.

[*February* 22, 1929.]

Human Adaptation to the Parasitic Environment.

By SHELDON F. DUDLEY, M.D. (Surgeon Commander, R.N.).

ABSTRACT.—Man, in contact with the pathogens in his environment, responds by developing immunity with or without symptomatic illness. The incidence of infectious disease in a community depends on the parasitic factor or "infection pressure," and the host factor, "herd immunity," i.e., the resistance of the community as a whole to the infection. Environment is only a secondary factor which alters the relative values of the two primary factors. Morbidity varies directly as the "infection pressure," and inversely as the "herd immunity." The great difficulty heretofore has been to separate the two factors expressing morbidity. In diphtheria, to some extent, this is now possible by means of the Schick test. By using clues gained from the study of diphtheria, and examining the age-incidence, severity, and fatality, of other infections under various environmental conditions, the hypothesis is reached that herd-immunity increases with the herd's past experience of the bacterial causes of most, if not all, infectious diseases. This immunity may be acquired latently, without illness, and, even if not always enough to prevent symptomatic infection, may be such that severity and fatality are decreased. The process is an example of the general biological mechanism by which the members of a species acquire adaptative variations more suitable to the environment. Of recent years air-borne droplet infections have caused less fatality and trouble to the English herd than a century ago. The manifold increase of the density and of the motion in the English herd must have greatly raised the average infection-pressure, but since severity of clinical disease has diminished and incidence has not increased in proportion, the herd-immunity of the English must have outstripped the increase of infection-pressure, i.e., the herd has become more closely adapted to its bacterial environment. It must not, however, be forgotten that adaptive fluctuations in parasitic characters must also play some part in all the phenomena of infectious disease.

ADAPTATION is a word which means the same thing whether in small talk or in a biological discussion. A man may be well or badly adapted to his job, an animal to its environment, a host to his parasites, or *vice versa*.

When competition is slack animals can hold their environments in spite of considerable maladjustment; but if the struggle for existence is keen the man will be eliminated from his job, and the dependent organism will get "sacked" by its host, should they not be closely enough adapted to the niche they occupy in the world. The environment may change and a previously well-adapted organism find that it cannot exist under the new conditions. On the other hand another animal may vary readily in response to all the changes in its surroundings, and succeed where a more specialized organism would fail. The power of adaptability is called plasticity, and it is one of the most valuable biological qualities a species can possess. Men and bacteria are both most plastic organisms, and this largely accounts for their pre-eminence and ubiquity.

The free-living bacteria and the great majority of the specific parasites of other animals are innocuous to man, who destroys any who get in his way. Others are harmless commensals which man tolerates, since they cause him no inconvenience. The group of dependent organisms which concern medical men are the potential pathogens. These parasites are imperfectly adapted to their hosts, and have the power of causing those unpleasant reactions called disease. In some members of this group the mutual adjustment between man and microbe is all but complete. For example, the meningococcus usually causes a symptomless infection, or at most an unnoticed catarrh of the naso-pharynx, and meningitis is only a rare complication. Individuals who, though infected with potential pathogens, give no reaction marked enough to attract attention are the so-called carriers of disease. In the Navy I estimated that there were at least 3,000 carriers of *agglutinable* meningococci

to every case of epidemic meningitis. In other conditions, such as smallpox, clinical signs and symptoms are the usual accompaniment of infection. Between these two types there are many others, such as scarlet fever, where typical and atypical clinical cases and symptomless carrier infections are all common. Thus, according to circumstances, the same parasite may initiate a fatal, severe, mild, or carrier infection. Such divergent host reactions were commonly explained by variations in parasitic virulence. But many observations suggest that variable host resistance is the more usual explanation of the severity gradient of clinical symptoms which is to be seen in all epidemics. I strongly emphasize that fluctuations in the parasitic characters concerned with virulence and infectivity must, in the long run, be as important fundamental factors in epidemiological phenomena as host resistance. However, in most of the hypotheses formulated to explain the relationships between host and parasite, variable dosage of infective material can be used as a substitute for variable virulence. Therefore, to simplify the discussion I shall assume that we are dealing with that biological *chimera*, an immutable, rigid, parasitic species.

On this assumption there are two direct causes of the severity gradient from carrier to fatal infection—the dose of the infective agent and the resistance of the host to microbic invasion. In the same person, if the dose is too small to cause illness, it may be large enough to cause symptomless infection, or the carrier state, while still smaller doses may be destroyed, even before they begin to multiply in their host. With the same dose of infective material the receiver's degree of resistance will determine whether he destroys them or becomes infected as a carrier or case. A certain amount of immunity, or rather a power of rapid response to microbic stimuli, which is plasticity, may be a genetic quality, and inheritable in the strict sense, but human resistance to infection is more often than not acquired during life. The resistance to microbic attack will depend on how often the subject has been infected as a case or carrier, or on how many bacteria he has destroyed in the past, and the distribution of this bacterial experience in time. In infants and young children resistance depends also on the mother's history. This congenital, as distinct from true hereditary, immunity, is a remarkable adaptation to the bacterial environment. The baby is born with a ready-made immunity to the parasites it will find in its new environment, which is, of course, the same as that to which its mother is already adapted. This immunity is a passive one, but it tides the infant over its first few months until it can begin collecting an active immunity of its own. Congenital immunity is more common than was realized before the Schick test showed that a new-born infant practically always had the same immunity reaction as its mother. The newly-born mammal may also imbibe immunity with its mother's milk. The work of Theobald Smith and Little [1] is very striking in this respect. They found that eight out of twelve calves who received no colostrum from the cow died from *Bacillus coli* septicæmia within a few days of birth, whereas ten controls, removed after only fifteen minutes' suckling, survived. Owing to the difference in the anatomy of the placenta in cows and goats, maternal antibodies are chiefly transmitted in the milk, whereas, in rodents and man, maternal immunity is usually conveyed by way of the umbilical vessels. However, human milk also often contains diphtheria antitoxin, and Peter's [2] exhaustive analysis of epidemic diarrhœa suggests that human milk may contain immunizing substances against the agents which cause this infection.

To summarize: immunity to bacterial invasion is acquired by a visible attack of ill-health, or more frequently invisibly, by the following: latent sub-clinical infection, or the carrier state, auto-vaccination with living bacteria in the food or atmosphere, or possibly even by ingestion of dead bacteria, or their products, in the dust, as well as by passive transference of maternal antibodies by way of the placenta or in milk.

I will now consider the community, or the herd. The herd may be large or small, with many subdivisions or smaller herds. Nations may be divided into urban or rural herds, or we can contrast the shore-going herd with the sailor herd, or herds dwelling in hospitals can be compared with those who live in mental hospitals. In epidemiology, herd pathology, or medical ecology, the concept of resistance to the spread of infectious disease in the herd is not exactly the same thing as the sum of the resistance of its individual members. For example, if it were possible to measure resistance by titrating the antitoxin content of the blood of all the members of the herd, it might be found that, in a community of one hundred, there were fifty who had one unit of antitoxin per c.c., and fifty who had none. In a second group of one hundred, each member might have 0·1 unit of antitoxin. In the first herd the average resistance would be equivalent to 0·5 units per c.c. per head, fivefold that in the second herd. Yet all field observations go to prove that the first herd, with a large number of completely susceptible members, would be more liable to an epidemic than the second, where everybody had some resistance.

The term "herd-immunity" was, I believe, invented by Topley [3], who thus defines it: "The resistance of the herd at any given moment will be determined by the frequency distribution within it of individuals of varying orders of susceptibility." This definition refers neither to the environment nor to the density of, and motion in, the herd, and, as I use the term, it is not intended to do so. However, having heard Professor Topley on this subject since the delivery of his Milroy Lectures, I think he does include environmental factors in his concept of herd-immunity. In one sense the resistance of the herd to microbic attack varies with the position and movement of its members relative to each other, but I find it more convenient, and a seemingly more accurate description of fact, to consider such external factors as density of, or movement in, the herd not as directly affecting immunity, but as influencing the intensity, rate of distribution, and reception of infection. For example, when soldiers sleep in a hut so close together, and for so long at a time, that meningococci can cross the gap between one soldier and another in sufficient numbers to cause meningitis, the change in the environment has increased the duration and rate of distribution of infection beyond that existing outside the hut. Therefore the resistance to the spread of meningitis is less in the hut than outside it, but according to my concept, the herd-immunity of the group of soldiers was not instantaneously lowered when they entered their sleeping quarters. Herd-immunity, in this paper, only describes the fundamental host factor, irrespective of the environment. For want of a better name I shall call the fundamental parasitic factor "infection pressure," which is determined by the frequency distribution of the various rates at which infective material is being received by the members of the herd (variable "virulence" may also be included).

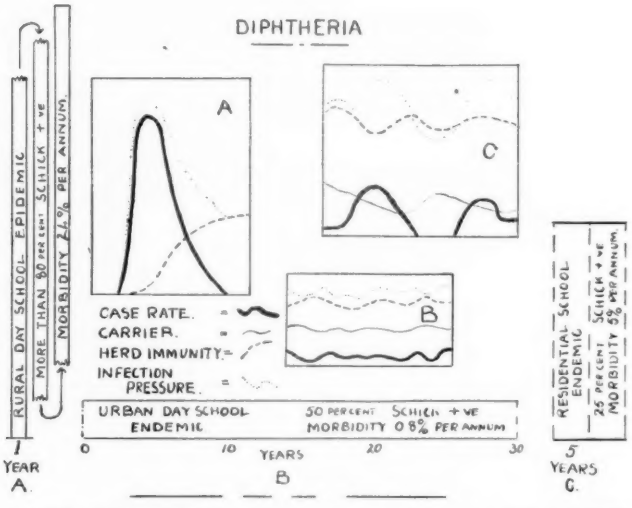
Infection pressure and herd-immunity react on each other. Increase of infection pressure is followed by a rise in the herd-immunity, which in turn retards, stops, and may ultimately decrease infection pressure to zero. A zero infection pressure means that the pathogenic virus has disappeared or relapsed into a harmless commensal. In this event the herd immunity falls to its minimum at a rate which, according to the nature of the infecting parasite, may require either a few weeks, or long enough to replace the whole population. In other cases the infection pressure does not fall to zero, but it and the herd-immunity attain an unstable equilibrium, the varying periods and amplitudes which are reflected in the incidence curves of endemic, sporadic, and seasonal disease.

The values, and differential value, of these fundamental factors, herd-immunity and infection-pressure, determine the type, quantity, rate of spread of infection, and the relative frequency and distribution of fatal, severe, mild, and symptomless cases in the herd. Environment is a secondary cause, it cannot produce disease in the absence of either host or parasite, but this does not mean it is not as important

a factor in determining the amount and type of herd infection. Environment includes all such secondary factors as atmospheric conditions, motion, density, and customs of the herd, which act directly on infection pressure; and nutritional changes, chill, and fatigue, which may temporarily, or permanently, modify the herd-immunity.

Fig. 1 represents these concepts diagrammatically. The rectangles and diagrams A, B, and C, refer to three school herds. A, a rural day school in New Zealand. B, the L.C.C. day schools, and C, Greenwich Hospital School. The vertical heights of these areas are proportional to the diphtheria morbidity and their bases proportional to the time taken for a number equivalent to 24 per cent. of the average population to contract diphtheria. Therefore, these areas are equal, and represent the same relative amounts of morbidity spread over different periods. The social class and age-groups of the three herds are comparable. The school environment of A and B are roughly similar. The environment of C differs in that the inhabitants

FIG 1.

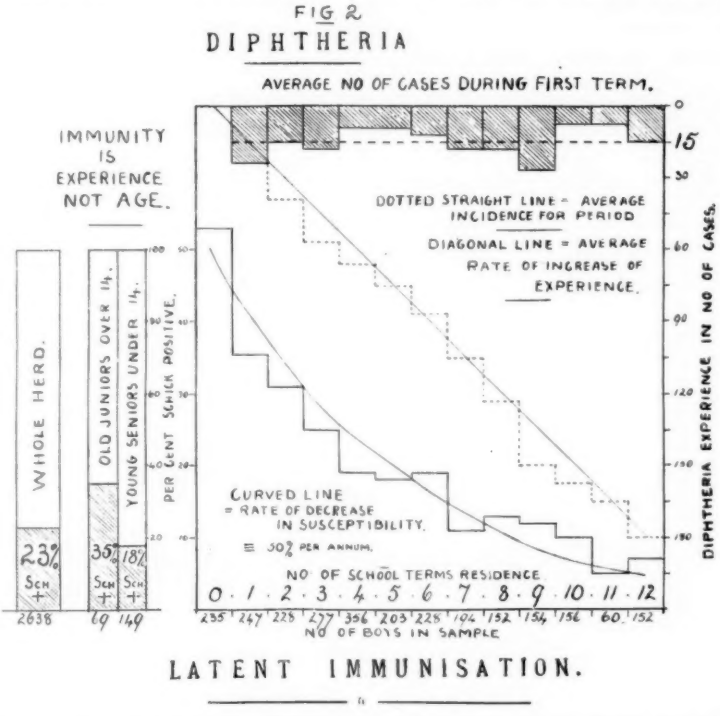


HERD IMMUNITY * INFECTION PRESSURE * ENVIRONMENT.

spend the night sleeping one hundred or more close together in one room (the mean age of this herd is also greater). A had been free from diphtheria for some years when the infection was introduced. Diagram A is supposed to explain what happened. The infection pressure at once rose steeply, unchecked by herd-immunity, which was at a minimum. After a preliminary lag the herd-immunity began to rise. The increase in infection pressure was retarded, and then declined to meet the rising herd-immunity, when infection vanished and the epidemic was over. I assume in these diagrams that the level at which herd-immunity is drawn is such that when infection pressure falls below this level symptomatic cases can no longer occur. Morbidity is thus made to represent the difference between infection pressure and herd-immunity. The thick continuous lines which are merely diagrammatic and are drawn to no scale envisage the attack-rate of clinical diphtheria. In fact, in these diagrams the infection-pressure curve is the ordinary incidence curve, using the maginary curve of herd-immunity as the base line. A, for simplicity's sake, may

be taken to represent an epidemic on virgin soil, which runs a rapid course, and finishes owing to the virus dying out. The epidemic cannot recur until the herd-immunity has subsided to a suitable level and infection is re-introduced. In B and C diphtheria has become endemic. In B it required fifteen years to accumulate as much diphtheria morbidity as the New Zealand epidemic produced in six months. The curves for infection pressure, herd-immunity, and morbidity in environment B tend to maintain the same relative levels and only oscillate within small limits determined by environmental factors. C shows the same thing, but the morbidity is less regular, herd-immunity is higher than in B, but owing to the environment (a residential school), the infection pressure is relatively higher still, as is indicated by the greater average distance between it and herd-immunity which is reflected in the higher morbidity. The herd-immunity level is drawn with regard to symptomatic

FIG 2


DIPHTHERIA
AVERAGE NO OF CASES DURING FIRST TERM.
IMMUNITY IS EXPERIENCE NOT AGE.
DOTTED STRAIGHT LINE = AVERAGE INCIDENCE FOR PERIOD
DIAGONAL LINE = AVERAGE RATE OF INCREASE OF EXPERIENCE.
CURVED LINE = RATE OF DECREASE IN SUSCEPTIBILITY. ≡ 30% PER ANNUM.
WHOLE HERD.
OLD JUNIORS OVER 14.
YOUNG SENIORS UNDER 14.
PER CENT SCHICK POSITIVE.
DIPHTHERIA EXPERIENCE IN NO OF CASES.
23% Sch +
35% Sch +
18% Sch +
2638
69 149
NO OF SCHOOL TERMS RESIDENCE
0 · 1 · 2 · 3 · 4 · 5 · 6 · 7 · 8 · 9 · 10 · 11 · 12
235 247 225 277 396 203 228 104 152 154 156 60 152
NO OF BOYS IN SAMPLE
LATENT IMMUNISATION.

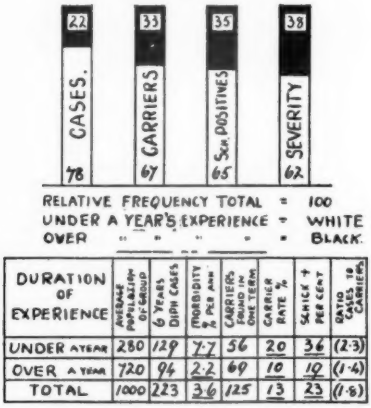
clinical infection, therefore carrier infection can continue even when the infective pressure has dropped below that value of herd-immunity at which clinical cases cease to appear. In diagram C this has been indicated by the gap in the case-incidence curve, without any corresponding break in the thin line which represents carrier incidence.

In diphtheria, intensive use of the Schick test, in combination with other clinical and laboratory observations, has given a clue to the solution of some general problems in the spread of infection. The Schick test has been so useful, because in it we have a simple reliable method of measuring the relative immunity of herds irrespective of infection pressure. Fig. 2 embodies some of the more interesting results of my work on this subject. The wide column on the left shows the average percentage of Schick positive reactors in the whole herd at Greenwich Hospital School during nine years.

In the diagram this herd has been broken up into thirteen groups of boys arranged according to the number of terms they had been in residence at school before being Schick tested. Zero represents new entries to the school, and twelve boys who had twelve terms (i.e. four years) at school. The bottom flight of steps represents the percentage of Schick-positive reactors in these samples. The upper flight of inverted steps gives the average number of cases of diphtheria notified during the residence of the corresponding sample below. The diagram shows clearly that, as experience of diphtheria increased, susceptibility declined. The shaded rectangles indicate the average number of cases occurring in the corresponding terms. The dotted line indicates the average number of cases per term for all terms, which was fifteen. The diagonal line represents the accumulation of this experience if it has been evenly distributed throughout the nine years to which these figures relate. Similarly the curved line indicates the decline in susceptibility, from 50 per cent. to 5 per cent. of positive reactors, if it had occurred evenly at a rate equivalent to 19 per cent. of all susceptibles per term. The expected closely follows the actual decline. In case it might still be thought that this increase in herd-immunity is a physiological function of age occurring irrespective of bacterial experience, two columns are given

FIG 3.

DIPHTHERIA.



DURATION OF EXPERIENCE	AVERAGE POPULATION OF GROUP	6 YEARS DIPH CASES	MORBIDITY % PER ANN	CARRIERS FOUND IN ONE TERM	CARRIER RATE %	SCHICK + PER CENT	CASES TO CARRIERS
UNDER A YEAR	280	129	7·7	56	20	36	(2·3)
OVER A YEAR	720	94	2·2	69	10	19	(1·4)
TOTAL	1000	223	3·6	125	13	23	(1·8)

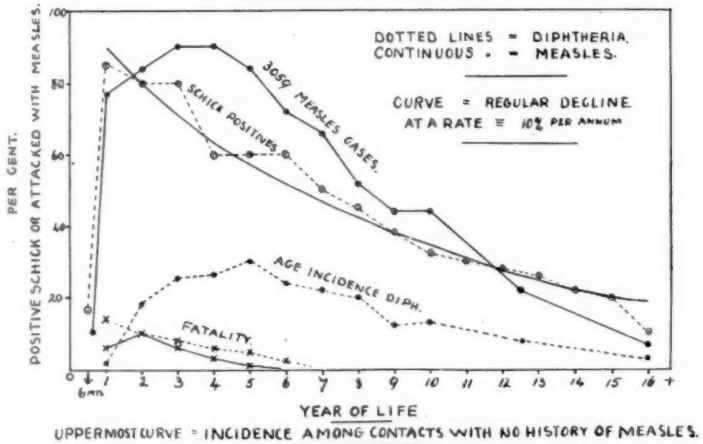
MORBIDITY * IMMUNITY * SEVERITY.

representing the proportion of Schick-positive reactors in two groups of boys tested at the same time. One set were all over fourteen, but had been less than a year in school, all the others were under fourteen, but had had over a year's experience of the school environment. The junior group, though older, contain relatively twice as many susceptible reactors as the younger, but senior, group. It is fair to conclude from these observations that immunity to diphtheria is an acquired adaptation to the diphtheria bacilli in the environment, which, far more often than not, is produced without any unpleasant reaction in those places where diphtheria is endemic. In Greenwich Hospital School about three boys became immune while one acquired clinical diphtheria. This ratio is probably lower than usual owing to the exceptionally high infection pressure of this special environment. According to Frost [4], the ordinary urban environment of Baltimore produces seven latent immunizations to each case of clinical diphtheria.

Fig. 3 gives a table in which the same herd has been split into two groups, the one with under, and the other with over, a year's experience of this high diphtheria infection pressure. The frequency distribution of cases, carriers, and Schick positive reactors, is given in the table, the last column of which shows the ratio of clinical diphtheria cases to carriers. The carrier figures were obtained from a single experimental swabbing of the whole school (1,000 boys) in order to avoid the errors due to the ever-changing carrier-rate. Because the carrier is only the limiting clinical case, the ratio of cases to carriers can be used as an index of severity of infection. The four columns above the table demonstrate the relative frequency of cases, carriers, and positive Schick reactions in the junior and senior sub-herds, when the number of each class in the whole herd is reduced to one hundred. The relative severity of infection in the two groups is suggested by a fourth column, which envisages the relative magnitudes of the case-carrier ratios. The senior group only contains about a quarter of the cases and one-third the relative amount of severity,

FIG 4.

DIPHTHERIA AND MEASLES



UPPERMOST CURVE = INCIDENCE AMONG CONTACTS WITH NO HISTORY OF MEASLES.

LATENT IMMUNISATION IN MEASLES.

carriers, and Schick-positive reactors, found among boys of under a year's experience. Hence in diphtheria, provided the environmental conditions remain the same, morbidity, carrier-rate, and the case carrier-ratio can all be used as an index of herd-immunity since all decline in harmony with Schick susceptibility.

Fig. 4 compares latent immunization in measles and diphtheria. I will first deal with the dotted lines which refer to diphtheria. The uppermost dotted line is an age-incidence curve of positive Schick tests in the Berlin population [5]. After the rapid loss of congenital immunity during the first year of life, the susceptibility, as measured by Schick testing, drops from 85 per cent. to 20 per cent. in fourteen years. This implies an average rate of immunization equivalent to about 10 per cent. per year of the susceptible population, as shown by the smooth continuous curve. The corresponding rate in the Greenwich residential school was over 50 per cent. per annum, due to the high infection pressure in the dormitories. Therefore using this rate of immunization as a measure of infection pressure, the pressure in the school was fivefold that of an ordinary town environment. The age-incidence group for

clinical diphtheria does not follow the decline in Schick susceptibility until the fifth year of life is reached. This increase in morbidity up to five can be explained by supposing the chances of infectious contacts to become greater as the developing infant increases in mobility, reaching a maximum when the child begins school life. It must ever be borne in mind that morbidity depends on both herd-immunity and infection pressure, whereas Schick test statistics depend on herd-immunity only.

The main object of fig. 4 (p. 31) is to demonstrate that latent immunization also occurs in measles. When in 1922 [6] I first wrote on latent immunization in diphtheria, measles was instanced as a disease in which immunity was only gained by actual attack. Since then I have carefully re-read the brilliant paper on measles which Butler [7] read at this Section in 1913. Butler noted that the number of contacts who escaped attack in measles could not be entirely explained by immunity due to previous attack, or by escape from all contact with the virus. He observed that the attack-rate among those in houses who were in contact with measles, but who had no past history of measles themselves, decreased regularly with age, i.e. susceptibility to measles declined with age, irrespective of a clinical attack of measles. The continuous lines in fig. 4 show a graph made from Butler's tables of the measles attack-rates in Willesden among house contacts who gave no past history of measles. This measles curve has been superimposed on the Schick-test curve in order to show how the decline in measles susceptibility closely follows the decline in susceptibility to diphtheria toxin. Note how the congenital immunity for measles and diphtheria both rapidly disappear during the first year of life. But the measles curve, which is a morbidity curve, and therefore depends also on infection pressure, does not definitely decline until the fourth year of life. This may either be a real immunity phenomenon, as loss of maternal immunity is slowly compensated for by increasing gains in active immunity, or it may have an explanation similar to that suggested to explain the age-incidence of clinical diphtheria, namely that an increase in infection pressure due to the change in environment as the child herd grows from one to five, has masked a real increase in herd-immunity. The curve of the Willesden children is confirmed by Butler's other observations on the escape of contacts at Marlborough College. 30 per cent. of the boys had no past history of measles against 10 per cent. of the similar age-group at Willesden; but at Marlborough only 17 per cent. of the boys escaped attack as against 82 per cent. of the poorer group. This suggests that, as in diphtheria, the poorer social group had acquired more immunity than the protected well-to-do one. This hypothesis is probably correct in part only since the contrast in measles morbidity between Marlborough and Willesden may have been caused by the environmental difference between a residential and day school. For example, in a reasonably long period (10 years), the boys of the naval school suffered ten times the diphtheria morbidity as boys in the same age-group (12-16) and social class in London day schools. Halliday's [8] recent analysis of the measles incidence in Glasgow tenements confirms Butler's deduction that latent immunity, without obvious clinical attack, does occur in measles. Thus in 1913, when Schick and Michiels published their original paper on the cutaneous immunity reaction for diphtheria, Butler was able to conclude from the vital statistics of measles that . . . . . "Not age, *per se*, but the opportunity it confers of acquiring an active immunity through exposure to attenuated or minor doses of infection brings about the changes in susceptibility exhibited in the table" (fig. 4).

Before leaving fig. 4, notice that severity as indicated by case-fatality also falls with susceptibility, but, without knowledge of all the details, case fatality may be a deceptive guide to immunity, because specific fatality is often exaggerated, by secondary infections: witness the malign partnership between the streptococcus and the diphtheria parasite at present causing anxiety on the Continent.

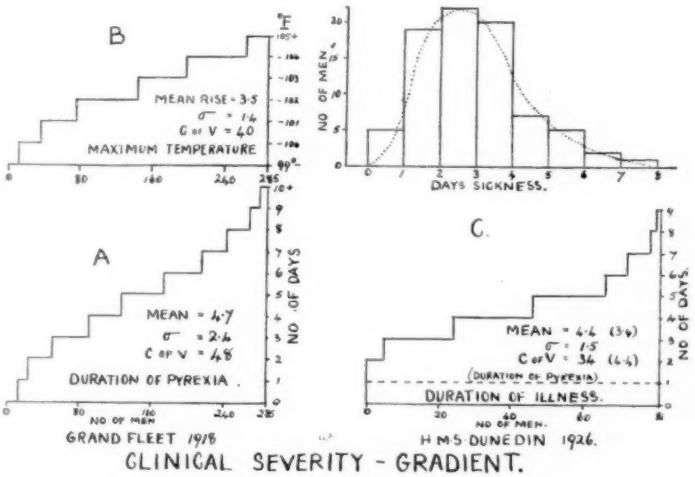
The recent English report [9] on vaccination, and that by Force [10] in America, state that many subjects with no past history of smallpox, or previous vaccination,

give an immediate reaction to vaccinia virus, which indicates immunity to smallpox. The popular explanation is that these immunes, owing the present mildness of smallpox or alastrim, are missed cases. Except on academic grounds, these men have acquired their immunity latently, since they were not ill enough to complain, or to attract the attention of others; hence the effect of their presence in the population on herd-immunity, and the spread of disease, is the same, whether they are called missed cases or carriers.

The difference in latent immunization in measles and diphtheria is, that whereas in the former the immunity acquired is generally sufficient to prevent subsequent clinical attack, in measles it only has a very small effect on susceptibility to ultimate attack, but a great effect on fatality and severity of infection, i.e., as susceptibility declines, the herd moves down the gradient in severity of clinical symptoms. I will try to explain what the severity gradient is, by the help of figures 5 and 6 (pp. 33, 35) made from data of influenza epidemics in ships. To measure the exact clinical severity

FIG 5.

EPIDEMIC INFLUENZA.



CLINICAL SEVERITY - GRADIENT.

of an illness is impossible, but rise of temperature, though not entirely satisfactory, is roughly related to the intensity of the uncomplicated reaction to the influenza virus. The diagrams A and B (fig. 5) refer to 285 patients that I diagnosed in 1918 as uncomplicated influenza. In B these men are arranged in order of duration of pyrexia. In diagram A they are grouped according to the maximum rise recorded on their temperature charts. Both sets of steps show a good range of variation. This series came from many ships in the Grand Fleet at a time when all sorts of secondary organisms were forming parasitic complexes with the influenza virus. This variability was as likely to have been due to variations in the infecting agents and environments as to immunity differences. Therefore I made diagram C from a series of 81 cases from one ship who comprised the total patients in one small epidemic, which produced no complications of any kind. My notes of this outbreak only gave the number of days each man was on the sick list, but since almost, without exception, I discharged each man to duty directly his temperature had been

normal for twenty-four hours, the mean duration of pyrexia was one day less than the duration of illness. Diagram C is arranged both as a set of steps and as a frequency polygon, and shows a range of variation quite comparable to A and B. This variability seemed more likely to be caused by some host factor rather than by variable parasitic dosage.

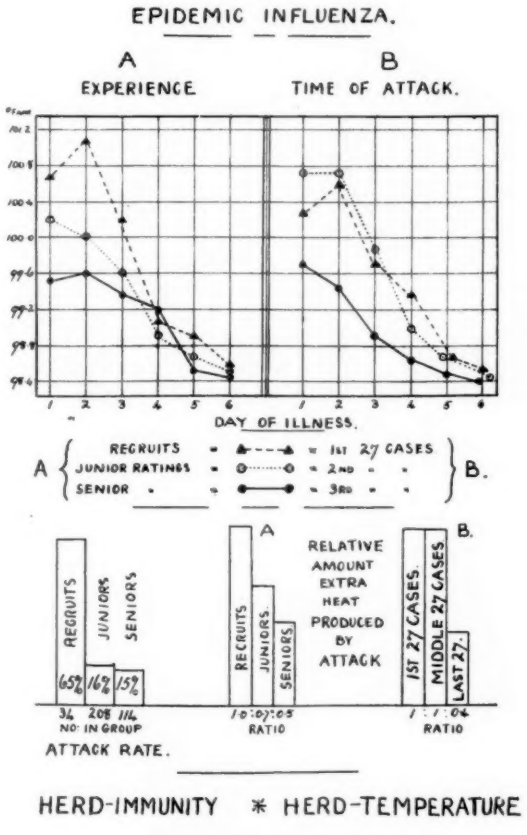
To compare the relative severity of infection in different sub-divisions of the herd, temperatures were averaged, thus making a herd temperature chart [11]. Fig. 6 gives some of these charts. In chart A the eighty-one cases are divided into three groups: recruits, junior trained men, and petty officers (senior trained men). The attack-rates of these three classes were 65 per cent., 16 per cent., and 15 per cent. respectively, making evident the lower herd immunity of the recruits; but there was no indication that the petty officers, in spite of having been longer at sea, had more resistance than the junior trained men. If, however, the herd temperature charts of these three groups are examined, the degree of pyrexia varies inversely as the experience of the herd, and the senior trained men are seen to have produced definitely less extra heat than the junior group. Thus, although senior were as susceptible to initial attack as junior trained men, yet, once attacked, the seniors escaped with a less severe illness, as is indicated by their lower average temperature. Brownlee [13] by comparing the morbidity among the vaccinated and unvaccinated demonstrated exactly the same herd-immunity phenomenon in smallpox. Twenty-five years after vaccination, immunity to attack is no greater in the vaccinated than in the unvaccinated, but the protection against death and severe illness persists throughout life. Thus the herd-immunity of two groups with differing past histories, subjected to the same infection pressure, may permit the same amount of initial infection in each, but the severity of the illness will be less in the group with the most bacterial experience. This phenomenon is parallel to that demonstrated by Glenny [12] for diphtheria toxin and other antigens. Though an animal may have no toxin in its blood, yet, if it has had previous experience of toxin, antitoxin will appear rapidly in response to the stimulus of a dose of toxin so small as to produce no effect in an animal which has not had a primary stimulus or experience of diphtheria toxin. Glenny has shown that "this principle of increased power to respond" is almost a general law as regards antigen-antibody reactions in experimental animals. Probably the same immunological mechanism is at the bottom of the phenomenon in herd pathology just described.

The temperature graphs in B (fig. 6), illustrate another fairly common observation. The eighty-one influenza patients were divided into the 1st, 2nd and 3rd twenty-seven men who reported sick during this epidemic. The first two batches have almost identical herd-temperature charts, but the last twenty-seven cases were very mild and produced only half as much extra heat per man as the first fifty-four. The mildness of the terminal cases in an epidemic can be explained thus: The outbreak starts off with a rush, and the infective pressure rises rapidly before herd-immunity can develop. In the middle period herd-immunity rises, but the infection pressure is now at its maximum, and the difference between pressure and resistance remains too great to allow clinical severity to subside, until the final stages of the epidemic, when infection pressure is rapidly waning, while herd-immunity is still rising. The terminal cases of an epidemic, although they have not resistance enough to avoid attack, yet have acquired enough experience of the infecting agent, during the earlier stages of the outbreak, to limit the average severity of their illness, i.e., severity is another index of closeness of adaptation between the herd and its potential pathogens. Since no line can be drawn between the mildest case and a contact carrier, diphtheria also illustrates very well the diminution of severity as the herd-immunity rises towards the end of an epidemic. At a time when the incidence of clinical diphtheria in a herd had almost dropped to zero, experimental throat swabbing showed that the carrier-rate for virulent bacilli had actually gone up.

Therefore the epidemic was still in progress, but no longer visible: "mild," or symptomless, had replaced "severe," or symptomatic, infection. In such a case the infection pressure may have remained as high as ever, but the severity of infection had been swamped by the rising herd-immunity.

The case-carrier ratio as an index of herd-immunity can only be used where the environments are similar. For instance, the average carrier-rate over seven years was roughly the same (about 5 per cent.) in both Greenwich Hospital School and

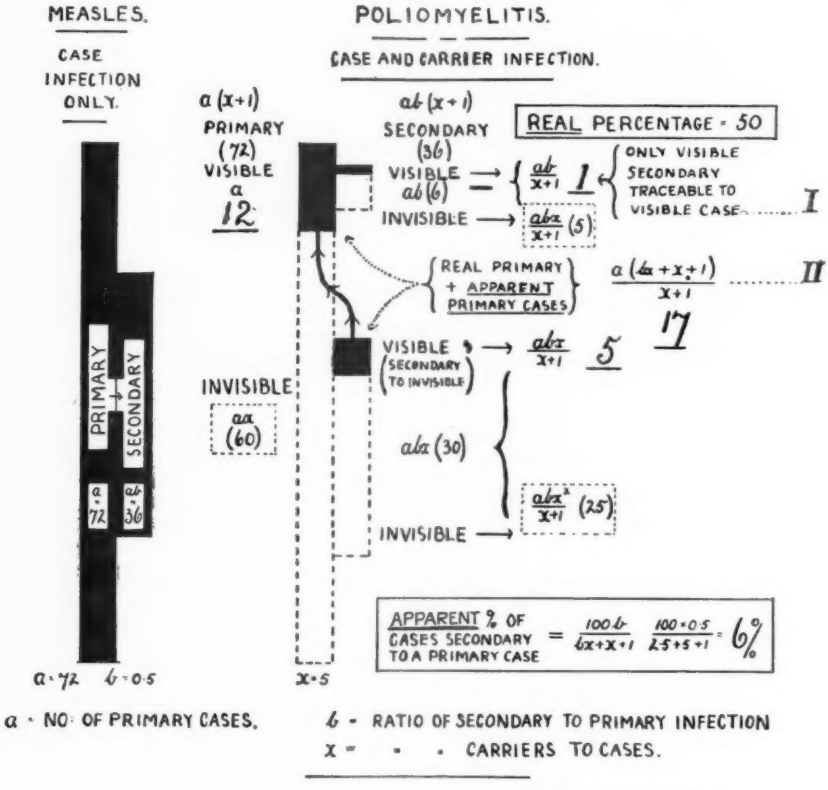
FIG. 6.



the L.C.C. day schools, but the case-rate was five times as heavy in the residential establishment. Therefore the case-carrier ratio was also five times as great in the Naval school as in the London schools. In the absence of other data, this observation might suggest that the immunity of the Naval herd was less than that of the London schoolboys. However, Schick-testing showed that the herd-immunity in the naval residential establishment was always much greater than among the London school children. Although the herd-immunity was high, infection pressure

was relatively higher and outweighed any tendency of the herd-immunity to reduce the severity of infection or the relatively high case-carrier ratio seen in residential as compared with day schools. The effect of herd-immunity in altering the ratio between cases and carriers is best exemplified by the epidemiology of poliomyelitis. Before investigating this let us examine briefly the effect of carrier infection on certain morbidity statistics. Aycock and Peaton [14] suggested that infantile paralysis and measles may have different methods of dissemination as there

FIG 7.



EFFECT OF CARRIERS ON MORBIDITY STATISTICS.

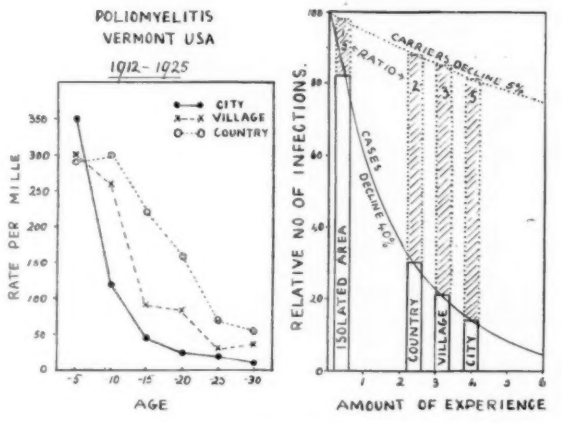
is a great contrast between the two diseases in the frequency with which secondary infections follow a primary one in the same family. These authors gave statistics which showed that about 50 per cent. of primary measles cases were followed by a secondary infection, whereas for infantile paralysis this percentage was only 4·3.

The diagram shown in fig. 7 is an attempt to explain the apparent low rate of secondary infections in poliomyelitis, or any other infection which produces carriers as well as cases. For the purpose of explanation, it is assumed that all infections

with the measles virus are visible cases, and that the distribution of the virus of poliomyelitis is *exactly* similar to that of measles, except that five out of every six infections are invisible carriers. In measles, 108 infections appear as 72 primary followed by 36 secondary cases. In poliomyelitis 108 infections would only show as 17 primary cases and one secondary case, because only 12 out of 72 primary infections are visible. These 12 cases are followed by six secondary infections of which only one is a visible case. The other 60 invisible infections produce 30 secondary infections, of which only five are visible cases, since the primary infections producing them were all invisible carriers. These five cases, which are really secondary, will be classed with the 12 real primary cases in the notification returns. Thus the apparent percentage is 6 per cent., while the real percentage of secondary infection is 50 per cent. just as in measles. Fig. 7 also gives a general equation for any proportion of primary and secondary cases, and any ratio of carriers

FIG. 8.

DECLINE OF SUSCEPTIBILITY TO CASE AND CARRIER INFECTION



INCREASE OF CARRIER-CASE RATIO WITH HERD IMMUNITY

to cases. If the figures of Aycock and Peaton are substituted in this formula, the value of the carrier- to case-ratio would be seven for New York. Another set of measles and infantile paralysis statistics makes carriers 42 times as numerous as cases. As little or nothing is known of the relative infectivity and herd-immunity of measles and poliomyelitis, the algebra in fig. 7 has no practical significance, and was only introduced to emphasize a principle which explains why there is sometimes great difficulty in tracing infection from one symptomatic case to another; and also to show that there is no necessity to assume that the means of dissemination of the virus of poliomyelitis is essentially different from that of measles, just because so few families report a second case of infantile paralysis.

Returning to the thesis that herd-immunity alters the ratio of cases to carriers: fig. 8 is modified from some statistics in another recent paper by Aycock [15].

During a reasonable period (12 years) it is seen that the total incidence of clinical infantile paralysis is greater in the country than in town. It is scarcely conceivable that the virus, if it spreads by droplet infection, can be disseminated more easily n the country than the town. The obvious explanation is that carriers are in a larger ratio to cases in town than in country, and therefore the total infections with the virus of poliomyelitis are just as high or higher in the town. The age-incidence in the town drops more quickly than in the country, suggesting a higher average, or less intermittent infection pressure in town than country, which implies that there is, if a sufficient period is considered, a greater average density of infectious foci in town than country. But as fig. 8 shows, infantile paralysis is more common in the country, therefore the surplus of infections in the town must be symptomless carriers. In the United States poliomyelitis infection becomes more irregular and intermittent as it passes from the town, to village, to country, but intermigration is so extensive as never to let the virus disappear for very long at a time, hence herd-immunity can never fall as low as it may do when the virus disappears for long periods, as happens in certain remote villages of Sweden, the Pacific islands, and even in exceptional spots in England. To prove a rule, examine the apparent exceptions. Reece [16] reports that in Stoke Rivers, an isolated Devonshire village, 14 houses out of 18 were infected in 40 days; 11 houses returned multiple cases. The total morbidity of symptomatic poliomyelitis was 30 per cent. of the 119 inhabitants. The attack-rate for infantile paralysis in Stoke Rivers was 120 times that of Vermont district, U.S.A. for 12 years. In Stoke Rivers multiple cases in one house were 20 times as frequent as in New York. Again, in Stoke Rivers the infection was probably most often direct from case to case, and carriers were not common; because, even if it is assumed that everybody who escaped symptomatic infection was a carrier, the ratio of carriers to cases could not have been more than 2 : 1 in Stoke Rivers, whereas in New York, on the most conservative estimate, this ratio must have been over 7, and, in my opinion, was probably nearer 100. The possibility that epidemics of infantile paralysis where multiple cases occur in one family, and the general attack-rate is high, are due to massive doses of the virus in milk must not be overlooked. For example Aycock [17] has suggested that milk was the vehicle of infection in the Broadstairs outbreak [18], where about 7 per cent. of the inmates of certain private schools contracted symptomatic poliomyelitis. But, even if both Broadstairs and Stoke Rivers were infected by milk, the difference between incidences of 7 per cent. and 30 per cent., especially when we consider how easily disease spreads in residential schools, indicates that the herd-immunity of the private schools was much higher than that of the isolated village, therefore the hyopothesis that the case-carrier ratio is higher in rural than urban districts still holds good. I do not think either outbreak had anything to do with milk. Reports of poliomyelitis outbreaks, such as those at Dennington and Grays [19], suggest that in rural districts the tracking of infection from case to case is often easy; and the "carrier hypothesis" in unnecessary. In contrast, the M.O.H. of a great town like Leicester [19], reports 82 cases, with little evidence of any connexion between them, and states that the 19 school children who were attacked with infantile paralysis in Leicester were attending 16 schools.

The diagram in fig. 8 tries to envisage the relation between carrier and case infection and herd immunity. The heights of the unshaded portions of the columns represent the relative morbidity of symptomatic infections. The heights of the three unshaded columns on the right are proportional to the total morbidity in the country, villages and towns of Vermont, U.S.A. The numbers along the base line represent the relative intensity of the stimuli from the virus of polio-myelitis each of these herds is imagined to have had. The susceptibility of the herd-to-case infection declines with the experience, as shown by the lower curve. The susceptibility to carrier infection is presumed to decline much more

slowly, as suggested by the dotted upper curve. Therefore the relative number of carrier infections corresponding to any intensity of experience will be represented by the vertical distance between these two curves, which represent the rate at which immunity is acquired to case and carrier infection respectively. In the selected examples the relative incidence of carrier infections is represented by the dotted and shaded parts of the columns. The column on the left-hand side of the diagram may be taken to represent a place like Stoke Rivers, which had had no experience of the virus before being attacked. In this column the difference between the curves as drawn would indicate that the herd-immunity was such that only one carrier would occur to every five cases; whereas in the column marked "city," the number of carriers to cases would be in the inverse ratio of 5:1. This diagram may also be used to envisage an ideal age-incidence curve. The first column would then represent young unsalted children, who tended to become cases more often than carriers, and the column labelled "city" would correspond to seasoned adults, in which carrier, rather than case, infection was the rule. It is also of interest to compare this hypothetical diagram with the three real age-incidence graphs, in fig. 8, which show nicely the different rates at which susceptibility declines in three herds of decreasing experience. Thus, the hypothesis of a variable case-carrier ratio can explain a good deal in the epidemiology of infantile paralysis. The case carrier-ratio is another index of the degree of adaptation of the herd to the parasitic environment.

Before leaving the subject of poliomyelitis, I should like to say that variations in the functional characters of the virus probably play an important part in its epidemiology.

I will now try to show that the herd can acquire a considerable immunity to even such a trivial and ubiquitous infection as the "common cold." In such a vague recurrent syndrome, the clinical and bacteriological correlations of which are not yet understood, and in which the issue is so complicated by auto-infection and short-period fluctuations in herd-immunity, secondary to temperature and other environmental factors, it is useless to expect evidence of increased resistance by finding a complete absence of attack, subsequent to the first cold. But if a census were made of all the colds suffered by each member of the herd, during a fixed period, and if it were discovered that the frequency of attack diminished as experience increased, provided that allowance could be made for environmental factors, it would be good evidence that the herd did acquire immunity to the common cold. Such evidence is contained in a report on the minor respiratory diseases, issued by the U.S.A. Public Health Service [20]. In this investigation the heads of 775 families, comprising 2,498 individuals (1,203 ♂, 1,295 ♀) reported all the "colds" in their families during one year. This population produced 3,794 "colds in the head and nose." Fig. 9 (p. 40) is made from data supplied in the report. The continuous line in the figure represents the frequency of male, the dotted line that of female "colds" at different age-periods. The frequency of colds in males falls to a minimum between the ages of 15 to 24; because during home, school and college life resistance is increased with age and bacterial experience; but some time, between the ages of 25 and 34, the male cold again becomes common, as though the herd-immunity gained in early life had been lost. This *apparent* loss of resistance was disconcerting, until the character of the population under examination was realized. The heads of the families, who each reported for his own household, were mostly medical men in the public services, and therefore the majority of the males, between 25 and 34 years old, must have been young husbands, or even elder sons, starting the practice of their profession. Many of these males, especially those in the fighting services, would be continually coming into contact with the minor droplet infections, and thus run a greatly increased risk of catching colds. The sudden rise in the curve was therefore due, not to loss of immunity, but to the increased

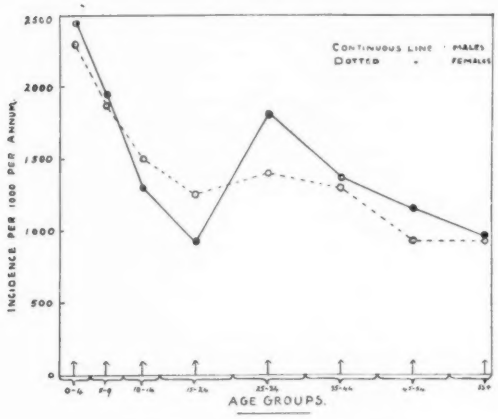
infection pressure inseparable from medical and service life. As adaptation to the new environment was slowly developed, the frequency of colds again diminished.

If these inferences are correct, the young professional men who caught cold must often have carried the infection to their homes, and a similar check in the decline of colds should be seen in the female incidence curve. However, because the relative number of males and females of the same age in each family would be approximately similar, the check in the female curve should be spread over a longer period, i.e., the female curve should show the same oscillations as the male, but considerably smoothed out—and it does.

Let us consider a simple case. Two husbands, in the 25 to 34 age-group, catch cold and infect their wives. One wife is the same age as her husband, the other one is 20 years of age and therefore in the younger group of 15 to 24. The result of these infections will be to increase two of the female age-groups by one. At the same time

Fig 9.

"COLDS" U.S.A.



HERD IMMUNITY IN MINOR DROPLET INFECTIONS

the one male group will be increased by two, while the other remains unaltered. The total of similar infections introduced into the home will cause the relative number of 15-24 male colds to be less, and the 25-34 infections to be greater, than the corresponding numbers for female colds, as is clearly demonstrated in fig. 1 (p. 28) by the difference between the male and female incidence curves.

In small isolated communities, such as ships or islands, colds often disappear when the herd breaks contact with civilization for any length of time, only to reappear when contact is made again. Either the infection dies out in the intervals, or the closely intermingled members of the herd soon adapt themselves to each others' commensal flora. I found an interesting example of this type of phenomenon in Boswell's "Life of Johnson" [17]. Boswell relates how Johnson criticized Macaulay for offering as true, the absurd story that whenever a ship arrived at St. Kilda, a small island in the outer Hebrides, all the inhabitants at once caught cold. Subsequently, Johnson apologized for the criticism, because he had meanwhile

learned from a Dr. Campbell that the story was a well attested fact, and that the inhabitants of St. Kilda caught cold because they were unaccustomed to the "effluvia from the human bodies of strangers." Boswell, while he admits the fact, can scarcely swallow the explanation. However, a Mr. Christian wrote to Boswell, expressing his surprise that a man of Dr. Johnson's acumen should have missed the real cause of the St. Kilda colds. There was no mystery at all, because, owing to the situation of the harbour, no vessel can approach the island except when the wind is in the nor'-east; and anyone knows that north-east winds and colds are cause and effect. In view of van Loghem's [22] recent paper on the effect of temperature changes on the incidence of colds, Mr. Christian may have been correct, but, nevertheless, I favour Dr. Campbell's explanation of the St. Kilda colds and would merely substitute "infected droplets" for the word "effluvia." A modern confirmation of Boswell's story is of interest. In June, 1913, H.M.S. *Active* was ordered to steam at once to the assistance of the inhabitants of St. Kilda, who had been stricken with a mysterious disease. Surgeon Captain Hall [23], then medical officer in H.M.S. *Active*, notes that for nine months of the year St. Kilda has no communication with the outer world, and even in the summer the swell is frequently so great as to make landing impossible. He then states that on June 12 a tourist steamer anchored at dawn in St. Kilda harbour, sailing again at noon. This steamer was boarded by many of the islanders; within twelve hours of the ship's arrival a few individuals were already complaining of symptoms. (A shortening of the incubation period seems to be another indication of low herd-immunity.) Within five days the whole population of St. Kilda, except six elderly people, were suffering from a complaint with the following symptoms: sudden onset, great prostration, temperatures up to 104° F., acute post-orbital pain, aching in all limbs, irritable cough, and no definite physical signs; the duration of the illness was generally less than a week. In fact they showed one of the most typical of the many clinical pictures of uncomplicated epidemic influenza.

In 1913 the virus of influenza introduced by some stranger caused a 92 per cent. attack-rate in the inhabitants of a completely unsalted island. This rate is higher than any I have heard of, even among densely packed herds which occupy men-of-war. Therefore, in 1913, influenza could strike down a whole herd whose immunity was at a minimum, yet it required five years of war, with its commingling of nations, before the virus acquired sufficient potency, or a high enough infection pressure, to swamp the herd-immunity of the whole world. In St. Kilda everyone gets a cold at once; the herd-immunity rises at once to a relatively high level, and the infective agent is either eliminated or becomes avirulent. Herd-immunity to colds, being of a most transitory nature, then drops rapidly to a minimum so that the next time the virus is introduced the islanders all catch cold together once more. Since these paragraphs were written Heinbecker and Irvine Jones [24] published an investigation which shows that the behaviour of minor respiratory infections among polar Eskimos (probably the most isolated race in the world) is an exact replica of Boswell's story of St. Kilda. Immunity to colds requires constant stimuli to maintain, hence the St. Kilda cycle is only a short-period example of a long-period phenomenon; this can be seen in an infection like measles which confers life-long immunity. Take for example the two classical epidemics in Fiji. The Fijians had never heard of measles before 1875, when the virus found a population with no herd-immunity and caused a morbidity estimated at 100 per cent. of 100,000 population with a 25 per cent. fatality. After the epidemic the virus disappeared and the herd-immunity was absolute. As new members were born and old ones died, the herd became sharply divided into two distinct classes: the absolutely immune and the completely susceptible. For some years all infants would be born with a transitory congenital immunity to measles, but after thirty years even congenital immunity would be getting scarce and more than half the Fijian population would have had no

measles experience. Time would be ripe for another disaster. In 1907, 40 per cent. of the population acquired measles, and the fatality was estimated to be also about 40 per cent.

Although these morbidity figures are crude estimates, it is fair to conclude that though the morbidity was halved, the fatality in 1907 could have been no less, even if it was not nearly double, that of 1875. This is exactly what was to have been expected if the herd was divided sharply into immunes and susceptibles. The Fijian experience indicates clearly that herd-resistance is not the same thing as the summed resistance of the individual members. Such a disaster as happened in 1907 could not occur in London, where nearly everyone has a little experience of measles before he is ever attacked by it. Notwithstanding, it is conceivable that the sum of the resistance of an average hundred Londoners might be less than that of one hundred average Fijians just before the 1907 epidemic. If, for the sake of explanation, complete herd-immunity is envisaged as black, and full susceptibility as white, and the population is represented as a strip from birth to death. Before 1875, the Fijian strip was pure white from end to end; immediately after the first epidemic it became wholly black. Then (congenital immunity being omitted for simplification) one end of the strip would rapidly become a pure white sharply divided from the black; and as time went on, the line, dividing white from black, would travel along the population strip towards old age, until the second epidemic arrived and painted the whole strip once more black. In contrast to Fiji, in a city where measles is ever present, the population strip would never be pure white or black. Typically it would be dark grey at birth, due to transmitted maternal immunity. Then, after rapidly fading to a very light grey, the shade would slowly deepen to nearly black as adult life was approached. In other types of environment, such as country places with variable degrees of traffic with the outer world, in which the visitations of an infectious virus are irregular and intermittent, the population strip might be painted in almost any arrangement of black, grey and white bands, in any order, width, or depth of shading.

Schick test graphs, of herds arranged according to age, often give indications of these imaginary bands in the varying slopes of the curve. Special environments of a stable character tend to balance their herd-immunity and infection pressure at a specific level. The introduction of recruits of lower herd-immunity than the original inhabitants causes a fall in the immunity of the newly constituted herd, followed at once by a rise of infection pressure. The incidence of symptomatic infection is thus augmented, not only in the more susceptible recruits but also in the original inhabitants, who succumb to the increased pressure of infection. This phenomenon has been shown experimentally, in a herd of mice, by Greenwood, Topley, and their collaborators [25] and perhaps explains the very high incidence of infectious disease in schools, recruiting depots, and any other place where the inexperienced are mixed with those who have become closer adapted to their parasitic environment.

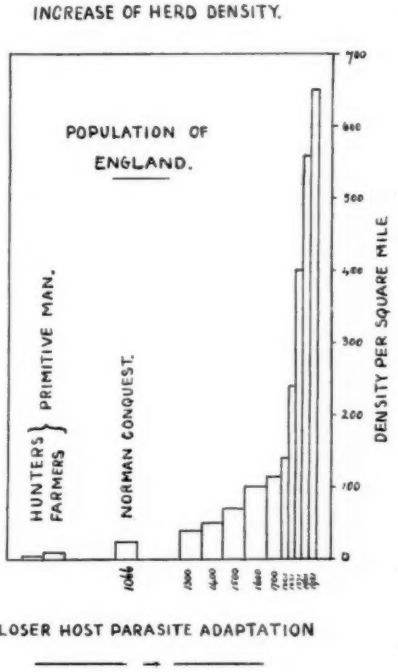
Many good examples of the adaptation of the herd to its parasites may be found among protozoal, and even helminthic, infestations, but I have confined my illustrations to droplet infections only; because none can claim that, up to the present, intelligent human interference can have done much to disturb the natural relations between man and the air-borne bacteria, as is the case with those dependent organisms which travel from host to host in food, water, or other vehicles.

Adaptation to the parasitic environment may also be effected by a true natural genetic immunity, as opposed to acquired natural immunity. In this respect Hirszfeld's [26] concept that natural anti-bodies may be inherited as ready-made "serum structures" or "biochemical organs" is of speculative interest.

Moreover, I must mention non-specific acquired herd-immunity and susceptibility: How far does experience of one variety of parasite alter the herd-resistance to others?

Just as it was found that the augmentation of immunity following artificial bacterial vaccination was an imitation of a natural process, so it follows that the numerous test-tube experiments, which show all sorts of cross-and-group immunity reactions, also imitate processes which occur in Nature. Hamer's well-known demonstrations of the sequence of epidemics in a definite cycle is of great speculative interest in this respect; they suggest that the experience of one complex of organisms paves the way for another. If so, Hamer's [27] epidemic sequences are perhaps another example of ecological succession, such as is seen after a forest fire, when an orderly sequence of plant communities follow each other, as the forest slowly returns to its pre-fire condition. On a smaller scale the ecological succession of bacterial species can be

FIG 10.



seen in a test-tube of an infusion of fæces, or other material, as Topley [28] and other biologists have demonstrated. The bacterial flora in the test-tube changes in a definite order as the environment varies, and may be, bacterial communities, when living in their normal habitat, the herd, follow similar laws. I have had no time to discuss important secondary factors, such as weather. However, Sir Leonard Rogers [29] has dealt exhaustively, and scientifically, with the effect of atmospheric conditions on the spread of many important diseases, such as cholera, plague, leprosy, and small-pox. Perhaps climate and weather have bulked so largely in epidemiology, to the exclusion of more fundamental factors, because the action of weather is so marked

in diseases transmitted by an insect vector. Gill's [30] work in the Punjab clearly shows the pre-eminent importance of weather in the herd-pathology of plague and malaria. These diseases were almost synonyms for fever to the ancients, and malaria was rampant in classical Greece and Rome. Hence Hippocrates and his followers were naturally impressed by the weather, since malaria follows the seasons, can be blown away by a wind, or become a malignant epidemic after a flood.

My concluding diagram (fig. 10, p. 43) is modified from one by C. Saunders [31], and shows the density of the population of England throughout the ages. Primitive hunting man was a beast of prey, and, like other wild animals, must have been much more closely adapted to his environment than modern man. The struggle to avoid elimination was so severe that it is unlikely that sickness and malignant parasites could survive for long in prehistoric times. As man passed into the agricultural and historic period, the margin of safety separating the human species from elimination widened, and there was more room for the malignant parasite. Inter-communication was nothing like it is to-day, and more or less isolated tribes probably attained a fair degree of temporary equilibrum with their own parasitic environments; a balance which was continually upset whenever intermingling of the herds took place, such as in war. The conjunction of war and pestilence in history is significant. As the diagram shows, the population of agricultural England increases very slowly to the end of the eighteenth century. Then, suddenly, the rate of increase becomes so greatly accelerated that in one century the density is increased sixfold. This increase, due to the industrial era, is really greater than the diagram indicates, as the population is now concentrated into the relatively minute areas of the big towns. Intermigration and contacts between different herds have increased relatively even more than density. This continuous mixing of the population implies a continuous interchange of parasites, by which a very effective balance is maintained between infection pressure and herd-immunity. During the last century the expectation of life has increased, the fatality and severity of practically all common infectious diseases have declined, and morbidity has shown no increase commensurate with that of the compression, multiplication and activities of the population.

As Rolleston [32] and Chapin [33] have pointed out, marked changes have occurred in the clinical characters of disease. Are we to attribute all these recent variations in man's host-parasite relationships entirely to improved public health measures, or to admit that a part of the improvement is due to closer adaptation between man and microbes? Over twenty years ago Ray Lankester [34] noted "that unless further evidence should prove him wrong, the adjustment of organisms to their surroundings was so severely complete that disease is probably unknown as a constant and normal phenomenon; and recently Dean Inge [35] stated, with no qualification, that wild animals are "completely adjusted to their surroundings." Wild animals are by no means as closely adapted to their environment as was once believed, and even epidemics are a normal and constant periodic phenomenon in the life histories of many species.

The following sequence of events, as given by Elton [36], is most instructive. Every eleven years solar activity is at a maximum and causes the average temperature of the earth to go up 0·6 degree. The extra heat, in turn, produces an increase in vegetation, as is proved by the periodic fluctuations in the width of the annular rings of the giant pines of California. More plant life means more food for many animals, including the varying hare of North America, which, as a result, increases its fertility from 5 to 6, to 8 to 10, young per litter. The resulting increase in numbers now places the hare beyond the control of its natural enemies or checks—the fox and lynx. These animals, since they are slower breeders than their prey, cannot multiply fast enough to eat up all the surplus hares. Once out of control the hares multiply more and more rapidly until the increase is terminated by an epidemic of disease, which wipes out all but a few of the more resistant animals, and another eleven-year cycle commences.

Thus a cosmic cause, the sun, acts, through the weather and food-supply, on the environment of an animal and allows it to multiply far beyond the control of its normal biological checks. An increase in infection pressure or some other alteration in the host-parasite relationship, which is thus produced, in turn stimulates a usually harmless commensal to escape the control of its host, and become a malignant pathogen. The result is an extensive epidemic which restores the balance of numbers, of both host and parasite, to where they were eleven years previously.

It is interesting to note that Wayson [37] in describing the causative microbes of similar epidemics in field mice, is unable to determine definitely the systematic position of these bacteria because "the virulence is subject to such vagaries." England is perhaps fairly safe from a big disaster due to one of the common pathogens in their present form, but there is ever the possibility of some new mutant, or variety of the bacteria, arising in response to the ever-increasing density and motion of modern herds, and wiping out the excess population, as happens periodically with the varying hare. This would not be altogether undesirable, as the density of the English herd must be already well beyond the optimum. Dependent organisms must vary in response to the stimulus of the environment, just as their hosts do, but the parasitic characters on which virulence and infectivity depend are vague, elusive, and difficult of measurement, even in the controlled and unnatural surroundings of the laboratory, so that at present it is not possible to learn much about the adaptative fluctuations that must take place in the natural environments of parasites—their specific hosts. I conclude, therefore, by emphasizing once more that to describe adequately the phenomena of herd pathology, parasitic adaptation to the human environment is no less important than its antithesis, human adaptation to the parasitic environment.

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DISCUSSION.

Professor W. W. TOPLEY said it was a great encouragement to those who were attempting to study the genesis and progress of epidemics under relatively simple experimental conditions, to hear Commander Dudley's account of careful and prolonged observations, carried out on human herds. With regard to the particular question of herd-immunity, it seemed desirable to include in this category two different types of increased resistance, both dependent on the structure of the herd as such. They might reasonably regard any herd as immune to

a particular infection, if it was so constituted that the introduction of a focus of that infection was not followed by any spread among the members of the herd. Herd-immunity, as so defined, might be divided into two main types : (*a*) That in which the immunity was dependent on the distribution of resistance, innate or acquired, among the individuals composing the herd, and (*b*) that in which the immunity was determined by environmental factors—for instance, by the absence of some insect vector of infection. It was, of course, the former type with which Commander Dudley had been mainly concerned, and which had occupied the attention of Professor Greenwood and himself in their experimental work. One of the greatest merits of such studies as those of Commander Dudley seemed to him to be that they afforded an example of the study of infective disease as a biological, rather than as a purely clinical phenomenon. By studies along such lines as these, it was probable that they should one day be able to discuss the phenomena which had been recorded by Sydenham and his followers, in terms of the reactions between parasite and host, which had been studied in the laboratory and in the field.

Sir WILLIAM HAMER said there was good reason for thinking that Sydenham had in mind a *specific*, not a non-specific, interaction between germ and host, and the paper on "The Influenzal Constitution," to which Surgeon Commander Dudley referred, had as its text a sentence of Sydenham (M.O. I., i, 6), which, it was claimed, "might be regarded as a first attempt at formulating the fundamental concept of immunology. Sydenham and Willis both realized, 250 years ago, "That there were infecting particles and a disposition of the blood and humours to react when acted upon by them. There was an interplay of conflicting forces and constitutions were thus engendered." In this interplay, or interaction, parts would (nowadays) be allotted doubtless not only to the host and the specific germ, but also, in Surgeon Commander Dudley's phrase, to any "usually harmless commensals" which might chance to be stimulated into "malignant pathogens." Thus "the sequence of epidemics in a definite cycle," so carefully described by Sydenham, represented much more than mere ecological succession"; his successive writings displayed to view two almost complete cycles of changes (1658-1675 and 1675-1688), and, when the phases of influenza thus exhibited were fully envisaged, it became difficult to refrain from concluding that specific changes, primarily due to interaction between a germ and its host, were in question. If it were objected, why not regard the pandemic influenzas as forest fires and all the rest as "ecological sequence," the reply was that it could not be done, for the simple reason that the view adumbrated by Sydenham, Willis and their followers had, during the last half century, been amply confirmed by Creighton and his disciples, by official reports coming from Whitehall, and by a number of witnesses from abroad. The fire, in fact, was ever smouldering, though it was only at intervals of from ten to twenty years that it burst into flame. The key to the mystery was that, as Sydenham had it, while the fever "sometimes attacked the bowels, sometimes the head," and might cause great prevalence of coughs and catarrhs, it was "the same fever throughout, . . . all the diseases which had originated from it had been the same."

Professor M. GREENWOOD said that Surgeon Commander Dudley's work was of first-rate importance. Nobody else having similar opportunities had had the patience or the sagacity to make precise observations of the changing epidemiological constitution of a human herd. Whatever they might think of Surgeon Commander Dudley's interpretations—and certainly he had amply earned the right to frame hypotheses—they must be of one mind as to the value of his exact observations. One must try to imagine what happened within a herd defined in the simplest terms and then seek to test one's hypothesis against the observed facts. He supposed the simplest case was that of a herd receiving no new members during the period of observation but subjected to a steady bombardment of "infection." One might suppose that "shots" were fired at random, so that some individuals were never hit during the period of observation, some were hit once, some twice, and so on. One must then suppose that the fact of being hit an assigned number of times determined in some a clinical illness, in others an enhancement of resistance. How the constitution of the herd, in respect of the proportions of members of it becoming (*a*) "cases," (*b*) "carriers," (*c*) "immunes," changed with time had to be determined. They must restrict themselves to a "law" the parameters of which were not too numerous to be determined from the data. Even in this simplest case of a closed herd there were great difficulties of description to be conquered; when the herd accepted immigrants the difficulties increased. But they must

and should conquer these difficulties in time as data accumulated. He hoped that Surgeon Commander Dudley's example would stimulate others with similar opportunities to take up this fascinating branch of research.

Dr. R. A. O'BRIEN appealed to Surgeon Commander Dudley to extend his work to influenza and the common cold. He considered the present position with regard to these two diseases as the greatest reproach to modern medicine, for the available methods of research had not been used. In the case of yellow fever Reed and Lazear showed that volunteers could be obtained for infection with deadly diseases, and the Laidlaw-Dunkin work emphasized how important it was to be sure that the experimental animals should be unquestionably susceptible to the disease under investigation. Proper inquiries into influenza and the common cold would need the support of large funds and, most of all, the help of suitable volunteers who would undergo the necessary isolation for considerable periods and the subsequent infection experiments with filtrates, etc. But they might end up by finding that there was a long series of filterable viruses causing common colds. And here knowledge was lacking. Did the cold, with sore throat, cough and tracheitis breed true, and did it produce immunity? Did the cold with sneezing and nasal catarrh produce it? He (the speaker) begged Commander Dudley to make records of his boys. He hoped the medical officers or matrons of isolated preparatory schools in the country would also make fully detailed daily records of every boy. From these we might begin to get some accurate knowledge about common colds.

Dr. A. JOE said that he was not sure that the inherited immunity in measles was such a definite phenomenon as that seen in diphtheria, since children at very early ages succumbed to the first chance of exposure. He also said that, since the point had been raised by Professor Topley, as far as clinical observation could show there was no evidence of an infective carrier state in measles.

Professor S. LYLE CUMMINS said that it was interesting to find that Surgeon Commander Dudley had not confined his statistical studies to morbidity and Schick tests, but had also taken into account the "severity" rates, as estimated in terms of temperature and "stays in hospital." He (Professor Cummins) thought that modifications in severity in the course of epidemics of given diseases had not been sufficiently studied in their bearing upon the immunological side of epidemiology. He suggested that Commander Dudley would find many facts to support his hypothesis of "herd immunity" in the study of modifications of clinical types, and also of pathological characters in contrasted sections of human populations in chronic diseases, such, for instance, as tuberculosis.

Surgeon Commander S. F. DUDLEY (in reply) said he was glad Professor Topley was in agreement with him in emphasizing the fact that all medical problems were problems in biology. In order to be an epidemiologist, in other words a medical ecologist, one had to be a "Jack-of-all-trades," and as a result master of none. All the "masters,' especially those whose trade was pure biology, must turn their attention to human herd pathology, and lend a hand to "Jack" if they wished to accelerate progress in medical science. Sir William Hamer's objection to describing "the sequence of epidemics in a definite cycle" as an ecological phenomenon was, perhaps, due to a misunderstanding. Ecology was only a "shorthand" expression for the study of the mutual reactions between different kinds of living matter and environments. The ecological succession of epidemics was only a clumsy attempt to translate Sir William's next phrase into general biological terms, and neither explained nor contradicted anything Sydenham ever said. Professor Greenwood's warning concerning the complexity of the subject was timely, but it was hoped that the few simple hypotheses put forward would not be taken as evidence that he (the speaker) imagined that the behaviour of epidemic disease had been adequately described by them. These hypotheses, better termed "suggestions," were only made to be refuted or modified by fresh facts. Dr. O'Brien might like to know that a large amount of time had been expended in studying the distribution and incidence of minor respiratory disease at Greenwich, but with little result. But, as Dr. O'Brien pointed out, this work, especially the technical bacteriology concerned, required more labour and time than one man, even without other duties, could find for it. The answers to the questions raised by Dr. O'Brien were of such paramount importance to the health of the human species that it was to be hoped the time was not far distant when

someone, with the control of the necessary funds, would realize this was a fact. The support of Professor Cummins on the significance of the severity gradient in epidemic disease was most encouraging. Severity was as important as morbidity, considering that 100 men sick for three days and three men sick for 100 days was a fair description of some outbreaks of so-called "influenza." As Dr. Joe, remarked there was no evidence that active carrier infection existed in measles. Nevertheless there were observations which showed that immunity could be acquired irrespective of clinical attack. How it was acquired was unknown, perhaps by the absorption and immediate destruction of subinfective doses of the virus.

Section of Neurology.

CASES SHOWN AT THE CLINICAL MEETING HELD AT THE NATIONAL HOSPITAL, QUEEN SQUARE, W.C.1, ON JANUARY 10, 1929.

Chronic Hydrargyria.— MACDONALD CRITCHLEY, M.D.—F. G. V., male, aged 56, has been a thermometer maker for forty years. About three years ago tremor first appeared in the hands and still persists. He complains of intermittent pain, cramps and dysæsthesiæ in the arms.

Some generalized loss of weight; slight, fine, rapid tremor of the hands accentuated by voluntary movement. Arm-jerks sluggish.

Teeth have almost all dropped out, and those that remain are intensely blackened. Peripheral arterio-sclerosis; blood-pressure 150/110.

Some intellectual deterioration.

Trigeminal Neuralgia Associated with a Spastic Paraplegia. Disseminated Sclerosis (?).—MACDONALD CRITCHLEY, M.D.—T. H. S., male, aged 56, has for the past four years complained of difficulty in walking and of a cold feeling in the right leg. No sphincter trouble. For fifteen months he has complained of severe paroxysms of pain in the right lower jaw.

Examination reveals a slight increase of tone in the legs; knee- and ankle-jerks brisk; bilateral Babinski response. Lower abdominals absent.

No cutaneous analgesia; vibratory sense diminished at malleoli; postural sensibility poor in the toes of the right foot.

Brain-stem Tumour.—MACDONALD CRITCHLEY, M.D.—L. B., male, aged 19, was admitted to hospital for headache, vomiting, and giddiness, which had first occurred fourteen days previously; the next day he felt a sensation of numbness over the right face. He thinks he has always been rather hard of hearing, but for a few days before admission he became quite deaf in the right ear and noticed tinnitus for the first time.

Eight years previously he had a similar attack of headache and vomiting, which cleared up entirely after five months.

Physical Examination.—Right Claude-Bernard-Horner syndrome. Paralysis right seventh nerve (peripheral type). Right eighth nerve deafness; motor weakness right fifth, with loss of sensation over the right trigeminal area, especially over the lower division. Palate raised slightly to the left on phonation. Slight weakness right twelfth nerve.

Bilateral cerebellar manifestations much more marked on the right side. Nystagmus on lateral and upward deviation. Head held tilted to the left.

Slight left-sided hemiparesis with signs of pyramidal involvement on that side.

Loss of all forms of sensation over right face, especially in lower third. Diminution of sensation (especially for pin-prick) over left arm and leg, and left trunk and neck up to the angle of the jaw. Vibratory sense normal; postural sensibility defective in left toes.

Optic discs show slight blurring.

Mother and Daughter showing a *Forme fruste* of von Recklinghausen's Disease.—FERGUS R. FERGUSON, M.D.—Mother, M. A., aged 56. Complained of attacks of giddiness of six months' duration. Examination of the skin shows freckling, varieties of pigmentary spots, blue spots, *café-au-lait* patches and molluscous tumours. Six years ago a fibroma was removed from the right upper arm. Wassermann reaction in blood: negative. Skiagram of skull: negative. Eighth cranial nerves functioning normally.

Daughter, M. A., aged 23. Shows similar cutaneous lesions but no subcutaneous neurofibromata. She has, in addition, some degree of kypho-scoliosis.

Familial Tremor and Spinal Amyotrophy with Cardiovascular Degeneration and Cerebral Thrombosis.—I. M. ALLEN, M.D.—A. E. T., male, aged 56, admitted December 10, 1928.

Complaints.—(1) Loss of power in the left arm for two months. (2) Loss of power in the right hand for fifteen years. (3) Shaking of the head for twenty years.

History of Present Illness.—Two months before, acute pain appeared in the left shoulder, shooting from there at times into the left hand. This persisted for ten days, and then, during one night, the left arm became powerless. One week later extreme wasting was evident. Severe attacks of pain had appeared in the left supra-orbital and occipital regions for five years up to one year before. The right hand has been of little use for fine work for fifteen years. Shaking of the head had been present for twenty years, worse during the past ten years. There was some nocturnal frequency of micturition.

Previous History.—Typhoid fever followed by a relapse between 20 and 25. Dysentery and cholera at 26. Venereal disease at 27. The right hand was injured in machinery at 26 and was not quite so useful afterwards. Valvular disease of the heart at 46.

Family History.—Married twenty-five years; no children. One aunt, now over 80, has been "shaky" for many years. A brother, now aged 40, is developing "shakiness" rapidly.

Condition on Examination.—Cranial nerves normal. Sensation normal. Upper limbs: in the right arm, wasting of interossei, of thenar and hypothenar eminences, and of the muscles in the front of fore-arm. The muscle tone is decreased in these muscles; adduction of fingers and flexion and extension of wrist are poor. Supinator-jerk absent; biceps- and triceps-jerks present. In left arm, wasting of all the muscles of limb. Muscle tone throughout much diminished Fingers are partly flexed, and, when not supported, the limb hangs helpless at the side. Movements of the fingers, hand, wrist, supination of the fore-arm, flexion of the elbow and abduction at the shoulder are almost impossible; all other movements much decreased. Supinator-, biceps- and triceps-jerks absent.

Lower limbs normal. Abdominal reflexes present. Gait normal. Spontaneous movements: slight shaking and nodding movements of the head and neck, especially on effort; sometimes the movements involve the right arm. Coördination is normal in right arm and legs.

Digestive system: some carious teeth in the lower jaw. Heart: apex beat in sixth intercostal space in anterior axillary line; impulse weak; at apex, first sound of poor quality, with slight presystolic roughening; in aortic area and to left of sternum, soft systolic and diastolic murmurs. Pulse collapsing. Blood-pressure: systolic 165, diastolic 75. Vasomotor system: left arm and fore-arm blue, cold and puffy. Other systems normal. Urine normal.

Electrical reactions: in right arm, with strong current, faradic response is present and galvanic brisk. In left arm, faradic response is present with a strong current excepting in intrinsic muscles of hand. The responses to galvanism are sluggish in all muscles.

Cerebro-spinal fluid: one cell per c.mm.; protein and globulin normal; Wassermann reaction negative; Lange test negative. Blood: Wassermann reaction negative. Skiagram of neck: negative.

Note.—Motor power in the left arm has improved, especially abduction of the shoulder, extension of fore-arm, flexion, abduction and adduction of the fingers and flexion and extension of the wrist. The voluntary movements of the left side of the face are now less active than the right, the left pupil is smaller than the right and the left plantar reflex is extensor. In addition, there are two fusiform swellings of the left ulnar nerve immediately above the internal condyle of the humerus.

Delayed Cerebellar Atrophy.—C. WORSTER-DROUGHT, M.D.—A. B., aged 62, male, complains of weakness of legs and unsteadiness of gait.

History.—The symptoms began about 1911—eighteen years ago—and have been steadily, though very slowly, progressive. No previous illnesses excepting gonorrhœa at 19.

Physical Examination.—Pupils equal and react normally. Fundi normal. Coarse bilateral nystagmus; speech somewhat thick and indistinct. Sensation normal.

Upper limbs: motor power good, but rapidly alternating movements poorly performed. No definite hypotonia. Incoördination with finger-nose test, right more than left, and with eyes open as well as closed. Arm-jerks normal. No rebound phenomenon. Abdominal reflexes present and equal.

Lower limbs: motor power moderate; slight hypotonia of calf muscles. Knee-jerks brisk and equal; ankle-jerks sluggish. No ankle clonus. Plantars flexor.

Gait: walks on a wide base and reels, especially on turning. Balance easily upset and not improved with eyes open. Romberg positive and incoördination on heel-knee test even with eyes open.

Other systems normal. Blood-pressure 160 systolic, 100 diastolic.

Wassermann reaction in blood and cerebro-spinal fluid negative. Cerebro-spinal fluid normal.

The case has been under observation for nearly ten years. At first nystagmus was only to the right; that to the left appeared in 1922. Incoördination of the arms developed in 1923.

Commentary.—The gradual development of incoördination of cerebellar type, relatively later in life, at first affecting the lower limbs, and in the absence of any recognizable cause, points to a diagnosis of delayed cerebellar atrophy. The arms may be affected after some years and in the present case incoördination of the arms was not observed until eleven years after the onset.

Dr. WORSTER-DROUGHT also showed a Case of Rifle Bullet in Brain for Thirteen Years, previously shown at the Clinical Section (vide *Proc. Roy. Soc. Med.* [Clinical Section], 1929, xxii, 397).

Olivo-Ponto-Cerebellar Atrophy.—C. WORSTER-DROUGHT, M.D., and I. M. ALLEN, M.D.—A. H., female, aged 67. *Complaint.*—Unsteadiness in walking. *Duration.*—Eighteen months to two years.

History of Present Illness.—Onset very gradual, with slight unsteadiness on attempting to walk. The condition progressed steadily. The left leg sometimes became stiff and several falls resulted. Could not fasten buttons or do knitting so well. Vertigo on attempting to walk. Bladder: slow micturition and difficulty in beginning if delayed. Constipation as a rule.

Previous History.—The left breast was removed at age of 45. Later a tumour was removed from the left buttock. The right breast was removed at age of 58.

Family History.—Married twenty-nine years; no children.

Condition on Examination.—Pupils small and slightly irregular, reacting normally. Visual fields normal. Optic discs normal; veins full; arteries show no obviously abnormal changes. Deficient lateral deviation of eyes to right. Nystagmus present; small jerks to left and long jerks to right. Speech: words a little spaced. Hearing normal. Other cranial nerves normal.

Sensation: occasional pains in outer side of both arms. Sensation in arms normal. In legs there is apparent diminution of pain and temperature, but if examined when the patient is not fatigued sensation is normal.

Upper limbs: motor power poor; tone fair; supinator-jerks absent; biceps- and triceps-jerks present. Lower limbs: right, motor power fairly good, tone slightly increased, position normal; left, flexion impaired, tone increased in extensors, position full extension; knee-jerks present, right greater than left; ankle-jerks

absent; plantar reflexes both extensor. Abdominal reflexes absent. No spontaneous abnormal movements. Gait: ataxic with lack of control of legs; staggering from side to side, with tendency to go towards the left. Coördination: slight intention tremor, left greater than right; heel-knee test impaired; Rombergism absent. Rebound phenomenon—left more than right. Repeated alternating movements clumsy. Sphincters: micturition slow.

Heart: apex beat in fifth intercostal space in nipple line; faint impulse; lateral dullness increased in second and third spaces. At apex, first sound of poor quality, faint systolic murmur, occasional presystolic roughening. X-ray: the aortic knuckle is somewhat prominent, suggesting some sclerosis. Peripheral blood-vessels tortuous, but not obviously thickened. Pulse normal. Blood-pressure: systolic 180, diastolic 100.

Mental condition: euphoric and facile; coöperation not good, and becomes appreciably worse as examination proceeds.

Cerebro-spinal fluid normal; Wassermann and Lange reactions, negative. Blood: nothing of note except moderate anicytosis; Wassermann reaction, negative.

This case resembles the preceding one as regards signs of cerebellar disorder, and, in addition, there is evidence that the pyramidal tracts are involved.

Choreiform Movements in a Case of Cerebral Syphilis.—C. P. SYMONDS, M.D.—L. C., female, aged 48. Admitted for involuntary movements of face and right arm. Married twenty-nine years; four children; no miscarriages. Her husband attended Guy's Hospital under Dr. Symonds in April, 1927, then aged 48, complaining of pains in the back and attacks of vomiting. He showed bilateral ptosis and analgesia to pin-prick over the left leg with a very much diminished knee-jerk on that side. He denied syphilis, but the Wassermann reaction in the blood was strongly positive. He was diagnosed as an early case of tabes. The patient herself has been well until a year ago, when she had a hæmoptysis. The involuntary movements now complained of began in September, 1928, and have progressed gradually.

On examination there are continual grimacing movements of the face of the kind commonly seen in Huntington's chorea, involving the forehead, both sides of the face, the lips and tongue. Choreiform movements are also present in the right upper limb, being much increased on excitement or effort, as when she attempts the finger-nose-finger test with the left arm. The movements involve the right shoulder, fingers, wrist and elbow—in this order of severity. Tone in the two upper limbs appears normal, but when she is intent on the finger-nose-finger test with the left upper limb, the right upper limb tends to pass into a mild tonic spasm in which the shoulder is adducted, the elbow flexed, the wrist pronated and flexed and the fingers flexed. Power in the upper limb is normal, but coördinated movements are somewhat interfered with by the involuntary movements. Intelligence and temperament appear normal. Speech is slurred and interfered with by the involuntary movements. Optic discs and fields are normal. The pupils are equal and central, but irregular in outline. The left does not react at all to light, the right very slowly. They both contract normally in convergence. The remaining cranial nerves are normal. There is no motor abnormality in the trunk or lower limbs. No defect of sensibility could be made out, but the patient is a bad witness. The triceps, biceps and supinator jerks are greater on the right side than on the left. The upper abdominal reflexes are present and equal—the lower absent. Knee-jerks present and equal. Ankle-jerks absent. Plantar responses difficult to obtain—apparently flexor. Heart: There is an impaired note at the left apex with fine crepitations. Blood-pressure, 110/65. No abnormalities elsewhere. Wassermann strongly positive in blood and spinal fluid, protein 0·09 per cent., lymphocytes 15 per c.mm., colloidal gold curve paretic.

Epileptiform Attacks Apparently of Ponto-Bulbar Origin. — C. P. SYMONDS, M.D.—M. G., female, aged 37, unmarried. Admitted for attacks of twitching and pains in the head. Family history unimportant. Previous health normal.

Present Illness.—Fifteen years ago, at the age of 22, she had the first of the attacks to be described. This consisted of a sudden twitching of the left side of the face and tongue. Subsequently she has had attacks from time to time, latterly becoming more frequent, so that they now occur about once a week. Two years ago for the first time the attacks began to be preceded by a feeling of numbness at the tips of the first and second fingers of the *right* hand. Eighteen months ago the patient first experienced a headache, which she described as a localized sore pain in the left temple, which has remained fairly constant ever since, irrespective of attacks. The attacks may occur at any time of day or night without other warning than the numbness of the finger tips. They last from five minutes to half an hour. The premonitory numbness only lasts a few seconds. The patient then notices a spasm of her tongue, which seems to move to and fro in her mouth. Then a twitching of the left eyelids, left side of mouth, and, finally, of the whole left side of the face and raising of *both* eyebrows. There is no disturbance of consciousness; no pain.

Seventeen months ago she had an attack of pain in the left lower jaw, which was relieved by the extraction of a molar root. The attacks were unaffected.

For the past fifteen years since the onset of the attacks her hair has been very sparse.

On admission to hospital, December 2, 1928, she appeared a well-nourished healthy looking woman with no abnormalities outside her nervous system.

She is intelligent, an excellent witness and emotionally stable. No abnormality of speech. Visual acuity, fields and fundi normal. Hearing and smell show no defect. Ocular movements good and full. No nystagmus. Pupils central and circular with normal reactions. Both corneal reflexes sluggish, but fifth nerve functions otherwise normal. At rest and in emotional movements there appears to be some slight tonic spasm of the left face, but on smiling to command there appears a definite weakness on this left side. The remaining cranial nerves are normal.

Sensory examination shows no defect to cotton-wool or pin-prick. Postural sense and vibration sense normal. There is no weakness, wasting or incoördination. All tendon jerks present and equal. Abdominals all four present and equal. Plantar responses flexor. No sphincter disturbance. Gait normal.

Wassermann reaction negative in blood.

Cerebro-spinal fluid: First specimen—Wassermann reaction positive, no white cells, protein 0·03 per cent. Second specimen—Wassermann reaction negative.

Stereoscopic skiagrams of the skull show an area of calcification on the right side with dilatation of the venous diploic channels in the neighbourhood.

On December 9, 1928, an attack was witnessed by Dr. S. Abrahams. After complaining of a feeling of numbness in the index and middle fingers of the right hand she began to have spasms of the left side of the face and tongue. These consisted of irregular spasmodic twitching of the whole of the left side of the face, fibrillary twitchings on the left side of the tongue, and raising of both sides of the forehead from the eyebrows upwards. The eyes remained normal and were not fixed. No spasms in palate. The attack passed off in from five to ten minutes, the intervals between the spasms becoming longer till they ceased.

The patient knew of no cause for the attack. There was no disturbance of consciousness. After the attack the left corneal reflex was more sluggish than the right.

December 19: The patient complained of double vision, and a weakness of the left sixth nerve was observed.

December 20: Sixth nerve palsy more marked.

December 31: Sixth nerve palsy slowly decreasing.

January 5: She still has double vision on looking to the left.

Narcolepsy with Cataplectic Attacks.—J. P. MARTIN, M.D.—W. F., female, aged 18. The patient had no symptoms till the beginning of September, 1928. Then, when told that her sister had been taken to hospital to undergo an operation, she began to cry and "fell in a heap" on the pavement. Since then similar attacks have occurred frequently and are usually brought on by a fright or other emotional shock or excitement.

It has been possible on two occasions to examine the patient during an attack. The onset is absolutely sudden; the patient falls down and, according to her statement on recovery, is unable to make any movement, even to open her eyes, although she knows what is going on around her. She has been able to describe the greater part of the examination made during the attack. Her limbs feel completely atonic. Tendon-jerks absent. Pupils dilated and inactive to light. Plantar responses flexor. No change of colour. No twitching or convulsive movement. In about a minute she can sit up and speak. As she recovers, the tone in the limbs can be felt gradually increasing, tendon-jerks return and pupils become active.

The patient states that she knows all the time what is happening around her, but apparently she had no recollection of the torch-light being flashed in her eyes about 5 seconds after the onset of an attack, although she recalled the removal of her glasses which occurred a few seconds later. In the interval between these two incidents and for about half a minute afterwards no outward change was noticed in the patient's condition.

Cerebro-spinal Syphilis with Lesions Simulating Eighth Nerve Tumour.—W. ESMOND REES, M.D. (introduced by Dr. MACDONALD CRITCHLEY).—C. P., female, aged 53, admitted complaining of deafness of two years' duration, with headache, giddiness and numbness of left side of nose of seven months' duration. No head noises or aural discharge. Seven months ago complained of morning nausea and vomiting, and she tended to lurch to the left when walking; there was some numbness of left face. Five months ago "dropping of right eyelid" and weakness of left arm and leg were noted.

Examination.—Discs and vessels normal. Old nebula on left cornea. Deficient upward movement and poor convergence of right eye. Nystagmus, coarse and slow to left, and rapid and fine to right on lateral deviation. Some vertical nystagmus. Left palpebral fissure wider than right. Argyll-Robertson pupils. Temporal and masseter muscles contract poorly on left. Left corneal reflex sluggish. Blunting to pin-prick over all three divisions of left fifth nerve. Weakness of left face—upper and lower halves. Air conduction absent on left—cannot hear watch. Weber referred to right. Other cranial nerves normal.

Power good in all limbs, but some weakness in both left limbs as compared with right. Left upper limb hypotonic. Gross inaccuracy in "finger-nose-finger" test on left. Dysdiadochokinesia on left.

Sensory System.—Loss to pin-prick and cotton wool over left fifth cranial nerve area. Vibration diminished over left tibial malleolus. Deep sensibility reduced in both calves. Sense of position and passive movement normal in all four limbs.

Deep reflexes all brisk. Abdominal reflexes absent. Plantar responses extensor right and left. Gait very unsteady. Rombergism present.

Cerebro-spinal fluid: Yellowish; cells 7 per cm. Total protein 0·8 per cent. Wassermann reaction doubtful.

Blood: Wassermann reaction positive.

Clinical Section

[*December* 14, 1928.]

Multiple Subcutaneous Sarcomata.—R. COVE SMITH, M.B.—H. C., male, aged 72. About six months ago patient first noticed two lumps about the size of a pea in the right groin. Two months later nodules appeared on the front of the chest. These nodules gradually increased in size and fresh ones appeared on the trunk and arms. Some are now definitely infiltrating the skin and becoming tender. So far none have appeared on the legs or head.

The patient is having lead selenide treatment.

Discussion.—Dr. BURRELL asked whether there had been any reaction as a result of the treatment.

Dr. COVE SMITH (in reply) said he thought some of the smaller nodules appeared to have decreased in size; the larger ones had shown no such tendency, but rather had commenced to ulcerate through the skin. While giving lead selenium, subsidiary treatment with liver was essential to counteract blood destruction, and with magnesium sulphate to prevent the accumulation of lead in the intestine; calcium iodide mixture had also been given to prevent the deposition of lead in bone.[1] So far, 5, 7, 10 c.c. of lead solution had been administered and no toxic reaction had been seen, except that after the last injection the red cells had somewhat diminished. For that reason there would now be an interval of a fortnight between the doses.

Specimens from the Case of Stenosis (Co-arctation) of the Aortic Isthmus shown on October 12, 1928.—F. PARKES WEBER, M.D., and F. KNOP, M.D.—After October 12, 1928, when the patient (L. C.) was shown at the Royal Society of Medicine, he at first continued to gain in strength, but then he became rapidly weaker, and in spite of cardiac therapy, etc., died on November 20.

The *necropsy* showed complete closure of the isthmus region of the aorta (see fig. 1) for a length of 5 mm., exactly at the site of the insertion of the ligamentous remnant of the ductus arteriosus; this was 13 mm. distal to the origin of the left subclavian artery, the short segment between the root of the latter artery and the complete block being extremely stenosed. The collateral arterial circulation between the upper and lower segments of the aorta had evidently been carried on in great part through the enlarged internal mammary arteries (especially the left one) and some enlarged intercostal branches of the aorta distal to the stenosed isthmus.

The heart was greatly hypertrophied,[2] especially on the left side. There were only two aortic cusps (see fig. 2), which might for convenience be called an anterior one and a posterior one, though the true embryological equivalence of these cusps was shown by the position of the origins of the two coronary arteries in regard to them. The two cusps were of nearly equal size, but the anterior one was divided into two equal parts by a kind of frenum (or "crossbar" of John Hunter[3]). The two coronary arteries arose above the anterior cusp, one above each of the two divisions. The whole aortic ring and the two cusps were completely calcified, the calcareous cusps having a verrucose surface, especially on the distal (aortic) aspect, as if the calcification had followed old verrucose endocarditis. Between the two calcified aortic cusps there was only a thin slit-like passage, that is to say, there was extreme aortic valve stenosis. There was also a good deal of calcification of the mitral ring (that is to say, at the insertion of the mitral cusps). For the diagrammatic drawings we are indebted to the kindness of Mr. A. Compton, F.R.C.S. The heart is in the Museum of the Royal College of Surgeons, London.

1 A. T. Todd, "Chemotherapeutic Researches on Cancer," 1928.

2 The weight of the heart, without the portions of aorta and pulmonary artery attached to it, was estimated to be about 750 grm. (26½ oz.).

3 Regarding John Hunter's observation of a kind of "fraenum or crossbar" dividing one of the aortic or pulmonary cusps when there are only two, see F. Parkes Weber, "Congenital Defects on the Left Side of the Heart," *St. Bartholomew's Hospital Reports*, London, 1899, xxxv, 149.

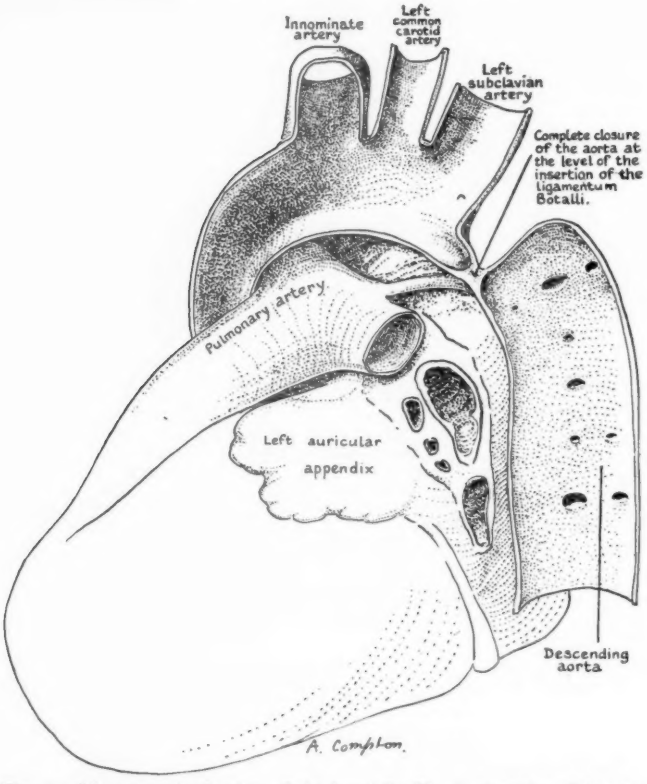
Innominate artery
Left common carotid artery
Left subclavian artery
Complete closure of the aorta at the level of the insertion of the ligamentum Botalli.
Pulmonary artery
Left auricular appendix
Descending aorta
A. Compton.

FIG. 1.—Diagrammatic drawing of the heart (by Mr. A. Compton, F.R.C.S.).

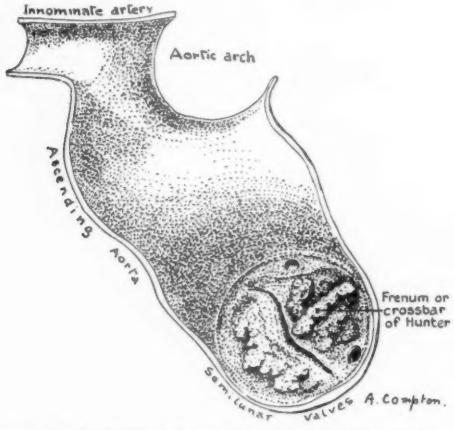

FIG. 2.—Diagrammatic drawing of the aortic orifice (by Mr. A. Compton, F.R.C.S.).

The pericardium was completely adherent from old pericarditis, and the pleuræ over both lungs were also in great part adherent. There was recent pneumonia (macroscopic and microscopic examination) of the lower lobe of the left lung. The spleen was somewhat enlarged and adherent by old perisplenitis. Part of the upper surface of the liver was adherent by old perihepatitis to the diaphragm. Nothing else of importance was found at the post-mortem examination. The brain was not examined.

REMARKS.

Since we showed our case in October, Dr. Maude Abbott has kindly directed our attention to a rather elaborate study of two cases—one with necropsy and one without, but both diagnosed during life—by H. Rösler, "Untersuchungen an zwei Fällen von Isthmusstenose der Aorta," *Wiener Archiv für innere Medizin*, 1928, xv, pp. 521-538. A smaller article is that by Dr. L. M. Blackford, in the last October number of the *Journal of the Medical Association of Georgia*.

In our own case the brachial blood-pressure was low, but in most cases it is high, as one would expect. In this connexion we might have mentioned that a special feature in the case of Drs. MacIlwaine and Turkington, shown in 1927 at the Belfast meeting of the Association of Physicians, was that the patient (a young man) had a very much higher blood-pressure in the right arm than the left. This phenomenon, observed also in Deneke's case (*Virchow's Archiv*, Berlin, 1925, ccliv, p. 336), strongly suggests that the *paulo-post-natal* process causing the stenosis of the aortic isthmus in the case in question involved the root of the left subclavian artery to some extent; and Dr. D. E. Bedford tells us of a specimen from a case of Dr. R. O. Moon, in the Museum of the Heart Hospital, in which there is a fibrotic stenosis at the origin of the left subclavian artery, doubtless part of the obliterating *paulo-post-natal* process that gave rise to the occlusion of the aortic isthmus in the case in question.

Unique in our case was the association of occlusion of the aortic isthmus with aortic valve stenosis, the latter being doubtless due to later acquired disease and calcification. Before the occurrence of this aortic valve calcification the patient probably had a high brachial blood-pressure, as most patients with aortic isthmus stenosis (or occlusion) have. The extreme stenosis of the aortic valve was almost certainly the main cause of the low brachial blood-pressure.

Another very interesting paper that has just appeared is the account by Dr. R. G. Anderson (*St. Bartholomew's Hospital Journal*, London, 1928, xxxvi, pp. 18-19) of the case of a man, aged 44, who died in October, 1928, on the day after admission to hospital. In spite of the evidence of extensive collateral circulation afforded by tortuous, pulsating subcutaneous arteries on the back of the thorax, the necropsy showed that the stenosis of the aortic isthmus allowed the passage of a cylinder which was ½ in. (12·7 mm.) in diameter. This relatively large lumen at the stenosed part of the aorta (together with cardiac failure) may have accounted for the absence of high brachial blood-pressure (systolic, 112 mm. Hg; diastolic, 60 mm. Hg).

Death in Dr. Anderson's case was caused by malignant endocarditis, as in many other recorded cases, and it is exceedingly interesting that, as in Drs. Poynton and Sheldon's case (communicated to the Association of Physicians, May, 1928) and in some others, there was malignant aortitis present, affecting not the part of the aorta proximal to the stenosis, nor the actual stenosed portion, but part of the aortic wall (which was abnormally fibrosed) distal to the stricture. The stricture in Anderson's case was ¼ in. (6·35 mm.) distal to the spot where the remnant of the ductus arteriosus (Botalli) was attached to the aorta. In the discussion on Drs. Poynton and Sheldon's case one of us (F. P. W.) remarked that the occasional occurrence of malignant aortitis beyond the stricture could be partly explained on the ground that the movement of the blood during life must be less active there (the position was somewhat like a blind pocket) than in the aorta proximal to the stricture.

The study of such cases of stenosis of the aortic isthmus illustrates the late results of an apparently useless operation, occasionally performed by Nature on new-born children. The constriction or complete occlusion of the aorta below the origin of the left subclavian artery is indeed an operation carried out by Nature, slowly (within a few weeks or months), without shock and probably without pain; it is performed without anæsthesia and without loss of blood, and is unaccompanied by sepsis, though, as a late after-result, it predisposes to septic complications.

Unilateral Erythrocyanosis Crurum Puellarum.—O. HILTON, M.D., and F. PARKES WEBER, M.D.—The case is one of a chronic erythematous condition of the lower part of the leg which one of us (F. P. W.) discussed in 1925,[1] but, according to the present prevailing Continental nomenclature, it is termed erythrocyanosis (frigida) crurum puellarum.[2] In the present case the condition is practically limited to the right leg, which is not obviously shorter, but slightly smaller in circumference than the left leg (about $\frac{3}{4}$ in. difference at the calf). The patient (M. M.) is a rather plump girl, aged 17, apparently in other respects healthy, though subject to chilblains on the hands (formerly on the feet also). The erythematous condition of the leg commenced three years ago, but as yet has always completely disappeared during the hot summer weather. Ultra-violet light baths seem to have been beneficial.

In the present case it is not clear why the right leg only is affected, but in another unilateral case (in a young woman, aged 20 years), shown by one of us in March, 1928, the right leg seemed to have been a *locus minoris resistentiæ* owing to infantile paralysis in early life.[3] Perhaps the present case is a rare example of inherent (constitutional) asymmetrical or unilateral reaction towards cold.[4]

Discussion.—Dr. L. T. BURRELL said that these cases were not very uncommon. Two or three years ago he had shown two, and Dr. Parkes Weber had suggested the name "erythrocyanosis" for the condition. The patients whom he (the speaker) had shown did not become well in the hot weather, although they improved. He believed that these cases differed from those which were strictly seasonal.

Dr. A. CAWADIAS said that although such predominantly unilateral forms of this disease were rare, the disease itself, especially in its lighter forms, was frequent. The erythrocyanosis affected all four limbs in their distal parts. At one time he wondered if there was any obstruction in the arteries of the limbs which would contribute to this vaso-dilatation of the cutaneous vessels, but in all cases which he examined he did not find any disturbance in the arterial circulation. He considered, therefore, that this condition was a general disease, the result of a general metabolic disturbance, and it would be interesting in these cases to try the parathyroid calcium treatment which has given some good results, and was based on the disturbance in calcium metabolism found in allied conditions.

Progressive Obesity in a Boy, with Attacks of Somnolence and Head-nodding.—F. PARKES WEBER, M.D.—The patient (A. W.), aged 11 years, English, is apparently well-developed, excepting for his obesity (fig. 1). His height is 136 cm. (4 ft 5 in.), and his weight is 47·7 kilogrammes (7 st. 6 lb.). His head is rather large, the maximum occipito-frontal circumference being 57 mm., that is to say, equal to that of an average adult man's head. Mentally he is rather dull and inclined to fall asleep, though at night he often gets up to micturate. He frequently

1 F. Parkes Weber, *Brit. Journ. Dermat. and Syph.*, London, 1925, xxxvii, 259; and *Proc. Roy. Soc. Med.*, 1926-1927, xx (Sect. Derm.), 61; also *Proc. Roy. Soc. Med.*, 1928, xxi (Clin. Sect.), 84.

2 *Cf.* A. V. Karwowski, *Dermat. Wochenschr.*, Leipzig, 1927, lxxxv, 1161, and A. Alexander, *ibid.*, 1927, lxxxiv, 601.

3 F. Parkes Weber, "Unilateral Erythrocyanosis Crurum Puellarum," *Proc. Roy. Soc. Med.* 1928, xxi (Sect. Derm.), 62.

4 "On Asymmetry and Unilateralism in Diseases of 'General' or Constitutional Origin," *cf.* F. Parkes Weber, *Med. Press*, London, 1927, clxxv, 244, and 1928, clxxvi, 327; and *Proc. Roy. Soc. Med.*, 1928, xxi (Sect. Study of Dis. in Child.), 44 (Discussion).

has rhythmical nodding movements of the head (about 60 to 80 per minute), resembling the so-called "head-nodding" ("spasmus nutans") of younger children. The movements cease during sleep and when he is interested and engaged with anything. His face often has a congested appearance, especially when he has been sleeping after meals. There is no abnormal facial hair, nor any axillary or pubic hair. There are no cutaneous "striæ." The skin of his body is slightly rough (hair follicles), but not abnormally dry. There is no evidence of enlargement of the thyroid or thymus glands. The testes are perhaps rather small for his age. The pituitary fossa, as estimated by the radiogram (Dr. E. J. H. Roth), measures 13 mm. by 8 mm.—not too large for the patient's head; the posterior clinoid processes are rather long; there is no evidence of erosion. Dr. C. Markus, reporting on the eyes, says that there is no nystagmus; there is definite pallor of the temporal portion of both optic discs; no

FIG 1

hemianopsia; no colour-scotoma; the visual acuity in each eye is about $\frac{6}{18}$. The Wassermann reaction and Pirquet's cuti-reaction are both negative. The urine (about 480 c.c. in twenty-four hours) is free from sugar and contains at most a trace of albumin, but in the centrifuge-sediment there are many hyaline and slightly granular tube-casts. The brachial systolic blood-pressure (hard to estimate, owing to the obesity) is about 80 mm. Hg. Blood-count (December 1928): hæmoglobin, 100 per cent.; erythrocytes, 5,200,000; white cells 7,650 (50 per cent. of which are lymphocytes). No abdominal tumour can be felt. Blood-sugar (fasting): 0·094 per cent. No alimentary glycosuria followed the ingestion of 50 grm. of glucose.

The boy, who is said to have been of average size at birth, seems to have been normal until two years ago. He then developed the nodding movements, which at first involved the whole body. They have considerably diminished, and for the last three months have been limited to the head-nodding. But the obesity and the tendency to fall asleep, which began to develop soon after the nodding movements, have been progressive, and he was even allowed to sleep in his class at school, before June, 1927, from which time, on medical grounds, he was not allowed to attend. His tonsils were excised about the summer of 1927, but this led to no improvement in his general condition.

A question that arises is whether the case might have been an insidious one of epidemic encephalitis, and whether the condition at present might be regarded as one of post-encephalitic obesity. The tube-casts in the urine point to there having been an infection of some kind. Anyhow, the obesity may be regarded as of "cerebral type," due to a lesion of some kind in the neighbourhood of the infundibulum.

Dr. E. STOLKIND said he agreed that this was a case of post-encephalitis lethargica. At first there might have been a slight degree of epidemic encephalitis.

Dr. H. STANNUS showed: (*a*) Two Cases of Tabes, (*b*) A Case of Myxœdema in a Child.

[*January* 11, 1929.]

Eventration of the Diaphragm.—LESLIE WITTS, M.D.—E. A., female, aged 52. Complains of pain in the left shoulder, of a few weeks' duration. Patient a healthy looking woman; no previous history of ill-health.

The cardiac impulse is in the third right intercostal space, and the right border of cardiac dullness is two finger-breadths to the right of the sternum. The costal

FIG. 1.—Eventration of diaphragm; patient erect.

margins move equally, but air-entry is diminished over the left lung anteriorly. Posteriorly, there is some restriction of movement at the left base and fremitus is diminished. The left base is dull to percussion, from the middle of the scapula downwards. In the left axilla the percussion note is usually impaired, sometimes

tympanitic. Breath sounds are very distant at the left base and in the left axilla, and gurgling sounds are occasionally heard.

X-ray Examination.—Heart and trachea displaced to the right. Right diaphragm normal, left dome greatly raised, being at the level of the second interspace with the patient in the erect position. Contour of left diaphragm that of a smooth, sharply defined, bow-shaped shadow, with a bright area below and lung-shadows above. The tissue of the left lung does not appear collapsed. The movements of the diaphragm synchronize, but the right moves much better than the left. When barium is given, the stomach is seen to lie close up under the left dome of the diaphragm, which is clearly seen separate from it. The small gut lies low in the pelvis and the colon is quite clear of the diaphragm.

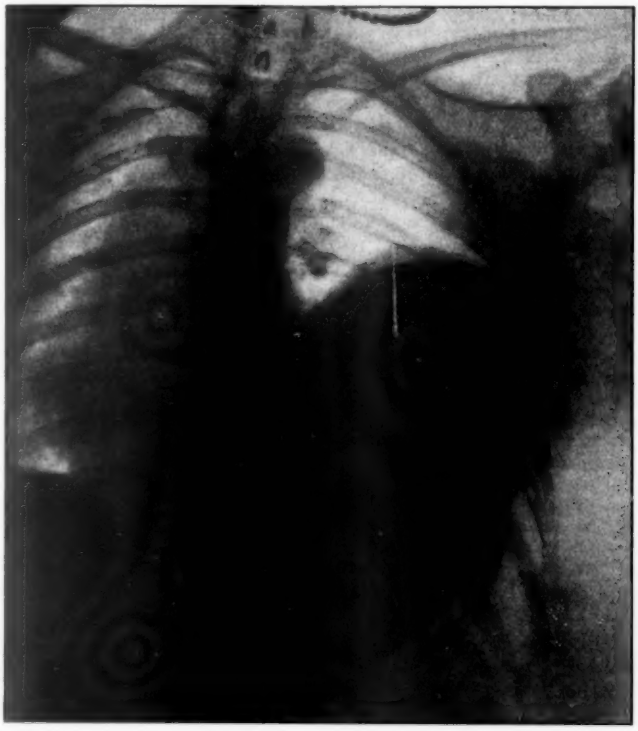

FIG. 2.—Eventration of diaphragm, after barium meal, patient recumbent.

The possibility of effusion or of a neoplasm, especially the latter, occurred to me. The skiagram, however, established the diagnosis of the rather rare condition known as eventration of the diaphragm. Bayne-Jones,[1] in 1916, was only able to collect forty-five cases of the condition; but since then a wider use of the X-rays has resulted in a number of other cases being described.

It is believed to be a congenital condition, and is often associated with other congenital deformities. It has been found in very young children. As can be seen by the X-rays, the lung is not collapsed. In some of the cases which have been

[1] Bayne-Jones, S., "Eventration of the Diaphragm," *Arch. Inter. Med.*, 1916, xvii, 221-237.

examined post mortem the lung has been found to be smaller than normal; often the lower lobe is stunted. In only three of the forty-five cases was the condition on the right. The development of the left side of the diaphragm is of a more complicated nature than that of the right, and therefore congenital defect is more likely to occur on the left side. Of Bayne-Jones's forty-five cases thirty-six were men, eight were women, and in one the sex was not mentioned.

The condition usually causes no symptoms, many of the cases being accidentally discovered by X-ray examination, or at a surgical operation, or post mortem. The physical signs in the present case are typical.

After excluding such conditions as effusion, localized pneumothorax, subphrenic abscess, or excavation of the lung, a differential diagnosis must be made from paralysis and hernia of the diaphragm. Unilateral paralysis is usually due to trauma, and so we expect a history of injury, but there is no such history here. With paralysis of the phrenic nerve the movements of the lower ribs are increased, and the diaphragm is sucked into the thorax during inspiration. In this patient the movement of the diaphragm is normal, though restricted. Hernia may be congenital or acquired. Both forms are commoner on the left side than on the right, the congenital for developmental reasons and the acquired on account of the greater frequency of the right-handed stab-wound. In both varieties of hernia the lung is collapsed, the contour of the diaphragm is irregular, and the abdominal viscera are inside the thorax.

There seems no reason to doubt this being a congenital eventration of the diaphragm. I do not think it was responsible for the patient's pain, which was probably rheumatic.

Dr. J. WALTER CARR (President) said he could not help reflecting how easy was the diagnosis of such a condition to-day in comparison with the time before the discovery of X-rays. The latter had spoilt, from an academic point of view, a most interesting discussion on the probabilities of what this case might have been without their aid. Certainly a variety of opinions would have been expressed. He recalled a case he saw many years ago in hospital. A man had been admitted after a buffer smash. He (the speaker) was asked to examine his chest, as unusual signs were present. He was very ill, and a complete examination was impossible; it was before X-ray days. At the left base there was a large area of hyperresonance and great displacement of the heart to the right. With considerable confidence—which he thought was justified by the history of the accident—he diagnosed traumatic pneumothorax. The man died a day or two later, and it was found that the diaphragm was ruptured and the stomach in the thoracic cavity. The stomach, filled with air, gave the physical signs which he regarded as diagnostic of a pneumothorax. Now by X-ray examination a correct diagnosis would be made at once.

Carcinoma in the Male Breast.—CECIL P. G. WAKELEY, F.R.C.S.—J. G., aged 55, first noticed a hard lump in his right breast a month ago, but thought it was of no importance as it was not painful.

A few days ago, while washing, he felt another small lump in his right axilla. This was also painless and caused him no inconvenience. However he saw his doctor because of a cold and quite casually told him about the lumps. His doctor examined him and sent him to hospital for treatment.

Examination shows a tumour in the right breast the size of a small hen's egg. It is hard, nodular and attached to the skin but not to the deeper structures. The axillary glands are enlarged but not fixed.

One of the characteristic features seen in carcinoma of the male breast is non-retraction of the nipple. In the cases formerly recorded it was noted that though there was definite carcinoma in the breast, retraction of the nipple was very unusual. In every such case the glands in the axilla have been very hard and large. In this patient the axillary glands are larger than the primary growth. He has suffered from this carcinoma for a long time.

The only treatment is a radical excision of the breast and axillary glands, and deep X-ray therapy subsequently.

The ratio of carcinoma in the breast of males to that of females is very small indeed.

Discussion.—Dr. HUGH STANNUS said that one of the patients with tabes whom he showed at the last meeting of the Section (p. 42) had a carcinoma of the male breast as well. In his case, also, there was no retraction of the nipple, but there was only one enlarged gland in the axilla, about the size of a hazel nut. The lump in the breast had been noticed a little over a year.

Dr. H. AWROUNIN said he had recently seen a man who had a carcinoma of his breast removed four years ago, and at that time no glands could be felt in the axilla. After removal of the carcinoma the man seemed well until four months ago, when he began to spit blood. Physical signs of secondary lung deposits were absent, and X-ray examination was negative. A month ago the man returned to hospital, and though there were no physical signs in the chest, the X-rays showed deposits in both lungs. There were recurrences also in the scar of the former operation wound. There were still no enlarged glands in the axilla.

Juvenile Tabes Dorsalis with Negative Wassermann Reaction in Blood and Cerebro-spinal Fluid.—C. WORSTER-DROUGHT, M.D., and I. M. ALLEN, M.D.—E. B., female, aged 26. *Present Complaint.*—(1) Pain in the back. (2) Severe headaches. (3) Attacks of vomiting. *Duration.*—Two years.

History of Present Illness.—The illness began suddenly two years ago with an attack of vomiting which lasted one week. During the attack there was pain in the lower part of the abdomen. There have been three such attacks during the last two years. Headache nearly every day for five years—across the eyes, a pressing feeling on top of the head, and "like wires in the back of the head." Pain in the back for four or five days at a time, usually associated with the vomiting. Fears operation and dreads being tuberculous. Depressed at times; often irritable. As long as she can remember has had difficulty in saying words. Since childhood difficulty in holding water when excited. Menstruation scanty, occurring every two months for the past four years; before that normal.

Previous History.—Sore throats. She has been to a number of tuberculosis dispensaries to make sure that she is not tuberculous.

Family History.—Father died aged 48 from tuberculosis of the spine. Mother died when aged 52 from heart failure and dropsy.

Condition on Examination.—Both pupils irregular and eccentric; right slightly smaller than the left; both fail to react to light and consensual stimulation, but react to accommodation. The optic axes are not parallel, but the ocular movements are normal. Voluntary movements of the face are weak. The other cranial nerves are normal. Sensation normal. Upper limbs: triceps jerks not elicited; otherwise normal. Lower limbs: motor power and muscle tone good; knee- and ankle-jerks absent; plantar reflexes flexor. Bilateral pes cavus. Abdominal reflexes present. Gait normal. No spontaneous movements. Coördination: finger-nose test slightly clumsy with the eyes closed. Romberg's sign absent. Teeth good; left upper central incisor somewhat peg-shaped; edges of upper central incisors uneven. Blood-pressure: 115 systolic, 70 diastolic. General pallor. Skin dry and scaly, especially on the hands and forearms. Other systems normal.

Cerebro-spinal fluid normal; Wassermann reaction and Lange gold curve, negative. Blood: Wassermann reaction, negative.

Skiagram of skull and spine normal.

Her mental-age, according to the Stanford modification of the Binet-Simon scale, is 12 years.

Commentary.—The combination of bilateral Argyll-Robertson pupils, which are unequal, with the absence of knee- and ankle-jerks indicates a clinical diagnosis of tabes dorsalis. The absence of any degree of ataxia and of deep analgesia is in favour of the juvenile form of tabes, in which such symptoms are unusual. An additional factor present is the bilateral pes cavus. The point that would seem against a diagnosis of tabes dorsalis is that both the blood and the cerebro-spinal

fluid yield negative Wassermann reactions, and the latter shows no increase in cells, or change in the Lange curve. The negative Wassermann reaction, however, again favours the juvenile form of the disease rather than the acquired. On the other hand, absence of palpable thickening of the peripheral nerves and the negative family history exclude chronic interstitial hypertrophic neuritis (Dejerine-Sottas' disease). A *forme fruste* of Friedreich's disease appears to be excluded by the negative family history and the absence of ataxia.

Discussion.—Dr. J. WALTER CARR (President) said that although some important symptoms were absent, juvenile tabes seemed to be on the whole the most probable explanation of the condition. He asked whether it was common, in that disease, to find the Wassermann reaction negative, both for cerebro-spinal fluid and blood.

Dr. BERNARD MYERS asked whether Lange's colloidal gold test had been done in this case. He had shown a case before the Section four years ago, of a girl aged 11, who differed a good deal from the present case. Up to the age of 11 she had slight amentia, with positive Wassermann of blood, and later on of the cerebro-spinal fluid. Hutchinson's teeth were present. Some nine months afterwards there occurred a distinct change and she became obviously very ill. She had Argyll-Robertson pupils, slurred speech, tremors of tongue and lips, difficulty in walking and failing mentality. The knee-jerks increased. Lange's colloidal gold test gave the paretic curve.

Dr. G. M. SLOT expressed his disagreemen with the diagnosis which had been arrived at in this case; he did not think the case fitted in with the pictures of juvenile tabes. On superficial examination he could not find any sensory signs.

Recently he had been seeing two or three cases of a kind similar to this, i.e., those in which there had been a considerable duration, the condition having begun in childhood or early adolescence; and there were mental changes of the same kind as in the present patient, sometimes starting in an illness of vague nature, and progressing in this way. On examining the patient shown to-day, he was struck by her wooden expression and the rigid attitude in which she stood.

He would venture the tentative diagnosis that it was a mild case of encephalitis lethargica, with the sequelæ which were so well recognized. There were here no abnormal neurological changes, but in both hands there was tremor, and there were the changes in the skin which had been described, with Parkinsonian expression. With regard to onset, frequently one could not obtain an account of this, or if one could, the symptoms were similar to those present at the onset of any other acute condition.

As regards juvenile tabes, many cases of the condition had been recorded in which knee-jerks in some cases were present; they were even exaggerated; the explanation being that the syphilitic lesion was not necessarily confined to the posterior columns; it was in the nature of a syphilitic encephalo-myelitis.

Dr. RUBY O. STERN agreed with Dr. Slot that there was an element of doubt in this case, and she would lay stress on the absence of sensory changes. She thought that in the absence of such changes many neurologists would hesitate to diagnose juvenile tabes, even though the disease showed differences from that in the adult. Had any Member suggested the diagnosis of a mid-brain tumour owing to the presence of the Argyll-Robertson pupil? Such a tumour was usually associated with unilateral Argyll-Robertson pupil, but she had seen a case of cerebral tumour in which that pupil phenomenon was bilateral.

Dr. ALLEN (in reply) said experience had shown that in a certain proportion of cases of juvenile tabes the Wassermann might be negative. In many instances there was a reaction so faint that pathologists did not speak of it as positive. It had also been noticed in other congenital syphilitic conditions that a faintly positive reaction might be present, or one which was almost too weak to be regarded as positive. In the present case, the cerebro-spinal fluid had been examined in detail. The cells were normal, so also was the protein.

As to the juvenile type of case, the posterior column signs were usually somewhat indefinite, and as a rule ataxia and Rombergism were absent. It was not infrequent, in the juvenile type, to find optic atrophy as a prominent feature rather than the spinal cord symptoms so common in the adult type.

With regard to the suggestions of Dr. Slot, this patient had, in addition to her organic condition, a very definite anxiety state, and he thought that that was quite sufficient to account for the tremor she showed. The tone in the muscles of the limbs was rather below than above

normal, one point at least against the suggestion that this was encephalitis lethargica with a slight immobility of the face.

The thickening and roughening of the skin had been present in this girl since birth, and, so far as could be ascertained, she had suffered from her disease condition for two years, though probably it had been there longer. The latter fact, he thought, was somewhat against the idea of a mid-brain tumour. Also, there were no changes in the ocular fundi.

Case of Coxalgia, for Diagnosis.—CECIL P. G. WAKELEY, F.R.C.S.—Patient, a girl, aged 7. Five months ago she had slight pain in her right leg, and three months ago she limped slightly; and at school it was noticed that she could not cross her right leg over the left.

Skiagram of pelvis and femora in case of coxalgia: for diagnosis.

I first saw her this week, and was surprised to find that she had definite limitation of flexion, adduction and abduction. There was $\frac{1}{4}$ in. of shortening of the right leg, and $\frac{3}{4}$ in. of wasting of the muscles of the right thigh. I thought it was probably a case of pseudo-coxalgia, but the skiagrams do not show what is typical in that condition, nor the appearance of tuberculous disease of the hip.

There is congenital asymmetry of the pelvis, but I do not think it has anything to do with the hip condition. There is a large mass of calcareous glands in the ileo-cæcal region. On the right side there is a slight tendency to flattening of the upper epiphysis, with some eating out of the epiphyseal margin.

Is the case one of pseudo-coxalgia or of tuberculosis? I propose to treat it by calliper extension for six or twelve months, taking a skiagram at regular intervals to see if a complete diagnosis can be arrived at.

Discussion.—Dr. G. M. SLOT (speaking as a physician) said he saw calcified glands in the pelvis, and he suggested they were tuberculous. As they were very numerous and definite they probably showed a tuberculous diathesis, though the patient did not look tuberculous. The case was more likely to be one of tuberculosis than of pseudo-coxalgia. To-day there seemed to be no pain produced by examining the joint, and that was unusual in both conditions.

Dr. HUGH STANNUS said he had elicited pain and tenderness, not only on movement, but on pressure over the joint. Was pain a point in the differential diagnosis?

Dr. H. AWROUNIN said that the appearance of this hip-joint suggested a travelling acetabulum moving upwards.

Mr. WAKELEY (in reply) said he thought the girl experienced definite pain, especially on flexion of the joint. She had pain on walking, especially after she had gone about a mile. To-day she rested, on advice, before coming to the meeting, hence the pain was not so marked as usual. Pain in pseudo-coxalgia was a very variable feature. There might be advanced pseudo-coxalgia with no pain, or an apparently slight degree of the condition and considerable pain. He had seen a patient with severe pain which had only been relieved after he had applied a plaster of Paris splint. Two months later a skiagram showed that fragmentation was more marked than before the splint was applied. In one of a series of cases which he had shown last May, the upper epiphysis was very flat, and there was fragmentation into five or six pieces. Yet that child walked without a limp, and, further, there was neither shortening nor pain.

With regard to treatment, in pseudo-coxalgia some surgeons put the patients into abduction and walking callipers. He (the speaker) left them alone if there was no pain. Whether one put them up or allowed them to walk about seemed to make no difference to the X-ray picture.

He hoped to be able to report more definitely in six months' time.

Section of Balneology and Climatology.

[*January* 10, 1929.]

The Suspension Stability Test in Rheumatoid Diseases.

By Joseph Race.

(*Biochemist, Devonshire Hospital, Buxton.*)

The appearance of a "buff layer" in blood has been noted since the days of Hippocrates, but the phenomenon attracted little scientific attention until Fahraeus investigated it some ten years ago and published his results [1]. Fahraeus observed that the rate of fall of the corpuscles after the addition of an anticoagulant was accelerated in pregnancy and various pathological conditions. Other workers have applied this test to numerous diseases, and a fairly complete bibliography is to be found in a recent review by Fahraeus [2]. It should be noted that the first extensive investigations of rheumatoid conditions in relation to suspension stability were made by Kahlmeter [3] and Westergren [4].

Technique.—Fahraeus used long tubes of about 9 mm. diameter, and a mixture of 2 c.c. of sodium citrate and 8 c.c. of blood. Later workers used smaller amounts of blood and tubes of smaller bore, and varied the amount of citrate and blood; hence the results obtained by the various observers are not comparable.

In this investigation I have followed the method of Cooper [5] who used 15 c.c. graduated centrifuge tubes and potassium oxalate as an anticoagulant. Potassium oxalate is to be preferred to sodium citrate on account of the simpler technique and because it does not materially dilute the sample, which can be used later for chemical analysis.

The technique employed is as follows: about 1 c.c. of sterile saturated potassium oxalate is drawn from a rubber-capped vaccine bottle into a 15 c.c. syringe, and after moving the piston up and down several times, the solution is ejected. The amount of solution adhering to the wall of the barrel is sufficient to prevent clotting. About 11 c.c. of blood are drawn from a vein with as little stasis as possible, and 10 c.c. are run into a graduated centrifuge tube for observation at 17° C. to 20° C. over four 30-minute periods. The 30- and 60-minutes readings are regarded as the significant ones, but it is advisable to take at least one further reading in order to plot out the results. The readings are finally expressed as percentages of the total volume; if the volume is not exactly 10 c.c., the calculation is simplified by use of a slide rule.

In order to obtain strictly comparable results the following precautions must be observed:—

(1) The calibration of the tubes must be checked over the range 4 c.c. to 10 c.c.

(2) The samples must be kept at a constant temperature, 17° C. to 20° C. being the most convenient. As the temperature increases, the suspension stability is lowered [6].

(3) The volume of the sample must be approximately 10 c.c. With smaller volumes of blood in tubes of the same diameter the first readings of the corpuscular volume tend to become smaller in accordance with Stokes' law.

General Considerations.—If the sedimentation test for suspension stability is to be of value in clinical medicine, the following postulates must be satisfied:—

(1) All healthy persons should give approximately the same results.

(2) The results should be consistent, i.e., the same person should always give the same result provided that the state of health is unaltered.

(3) The suspension stability should alter when a morbid state arises and, if possible, the extent of the alteration should vary with the severity and extent of the morbid process.

(1) All who have worked with the test agree that normal healthy blood has a high suspension stability, but it is impossible to analyse and compare the various

figures quoted on account of the variations in the technique employed. With the technique described above, over 90 per cent. of samples from healthy males gave a stability of 92 per cent. or over after one hour, and all (105 cases) showed 90 per cent. stability or over; a series of 74 females gave 82 per cent. with 92 per cent. stability or over; 95 per cent. with 90 per cent. or over; and only 5 per cent. under 90 per cent. stability. Several Continental observers have reported greater differences between male and female bloods, but a portion of these may be due to the nature of the examinations as to normality. Some years ago a series of nurses and massage students was examined thoroughly in the Devonshire Hospital and nearly 25 per cent. had to be eliminated owing to abnormality. Male and female bloods differ in erythrocyte content and in chemical composition [7], and these may lead to slight variations in the stability. A factor of possibly greater moment is menstruation, which has been shown to reduce the suspension stability. Age does not appear to affect the suspension stability. W. Malone [8], in a large series of cases at Buxton could find no correlation between age and stability, and the same is true of the Devonshire Hospital results as a whole. In the 70-80 decade there are numerous stabilities of 90 per cent. and over in males, whilst one female gave a 90 per cent. stability at the age of 77. (2) The evidence regarding the consistency of results from the same person is more meagre; this is to be expected in view of the comparatively short time that the test has been in use. My own blood shows no appreciable variation in rather more than two years and similar results have been obtained with other bloods over shorter periods. Further data on this point will require time to accumulate. (3) There is abundant evidence as to the diminished stability of the corpuscular suspension in such acute conditions as rheumatic fever and pneumonia, and the loss of stability is usually so marked that the cell volume approaches the minimum within 30 minutes. Other conditions in which an abnormal stability has been found are tuberculosis, various tropical diseases, malignant neoplasms, pernicious anæmia, appendicitis, arthritic diseases, certain psychoses, and pregnancy, especially as the full term approaches.

Similarly, Violato [9] found in a series of 103 cases of appendicitis that in all, and especially in those accompanied by suppuration or periappendicular abscess, the suspension stability was reduced, but increased immediately after the operation, and returned to normal following the healing of the incision.

I have also found that in the convalescent stage following acute nephritis, the stability has increased concomitantly with the concentration factor of the kidneys and the gradual disappearance of albumin and casts from the urine.

In tuberculosis, the correlation of the suspension stability with exacerbation and ameliorations is not so definite as could be desired, but some at least of these discrepancies are to be attributed to the difficulty of determining the extent of the active process. In fact the discrepancies may be more apparent than real and may disappear once the basic principles of the suspension stability test are elucidated and more is known regarding tuberculosis.

It is important to note that even such minor maladies as common colds reduce the suspension stability of the blood by small though definite amounts (3 to 4 per cent.), and that the stability returns to normal with the disappearance of the usual symptoms.

Suspension Stability in Rheumatic Diseases.—In dealing with these diseases it is proposed to follow the classification adopted by Dr. J. Alison Glover [10].

A. (1) *Rheumatic Fever.*—No records are available in this hospital; according to results obtained by other workers the stability is very low during the acute stage but improves with the abatement of the symptoms and usually becomes normal after a few weeks.

(2) *Subacute Rheumatism* (49 cases). Diagram I.—The results in this subgroup are extremely variable. A few samples exhibit marked loss of

suspension stability whilst others show little deviation from the normal: the great majority give intermediate results. In view of the extremely variable conditions of these cases a wide variation in the results is to be expected.

Again, as might be expected, these cases show the highest percentage of improved suspension stability after treatment and the most marked increase in the stability. The results agree with the general clinical experience that the subacute type is the most amenable to treatment and that the improvement is usually more rapid than in the insidious chronic types.

B. (3) *Muscular.* Fibrositis, pleurodynia, and torticollis (101 cases). Diagram I.—All the cases under consideration were diagnosed as fibrositis and gave variable results which, on the whole, are more normal than those obtained with the subacute group.

SUSPENSION FACILITIES.

1 Hour Results Frequencies.

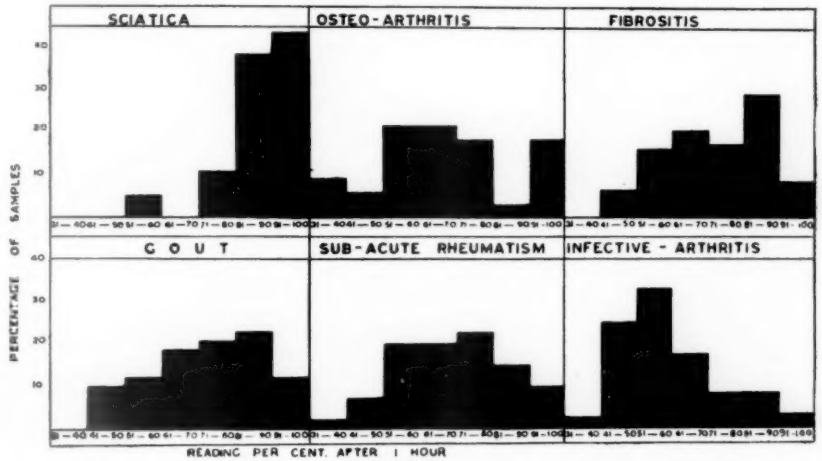

DIAGRAM I.

On separating the male and female cases, it was noted that the males gave definitely higher stabilities; this can be ascribed to the greater severity of the symptoms usually attained in females before seeking alleviation by spa treatment. Economic conditions usually impel men to seek relief at an earlier stage. A certain proportion of these cases may be due to traumatic or static causes only, while in others an infective factor plays a more or less important part.

The male group also show a higher percentage of improved suspension stability results, although the increase in the stability is not nearly so rapid or marked as in the subacute type and comparatively few attained a normal value.

Such results suggest either that recovery is exceedingly slow or that the causal agent had not been eliminated, even though the subjective symptoms had been ameliorated and the capacity for work more or less restored.

Inability to follow up these cases after discharge from the hospital prevents definite conclusions being made, and it may be several years before the readmissions are numerous enough to permit of statistical analysis.

(4 & 5) *Lumbago and Sciatica* (20 cases). Diagram I.—It will be seen from the diagram that the suspension stability in these cases is either normal

or only slightly subnormal. It has been observed that cases of uncomplicated trunk sciatica, usually occupational, give a normal stability, but if a root sciatica is accompanied by muscular wasting, the stability is diminished.

Comparatively few observations are available as to the neuroses as a whole, but they suggest that the stability is unaltered. Cases of chorea, paralysis agitans, brachial neuritis, migraine, hemiplegia, and post-encephalitic Parkinsonism gave normal results, although secondary neuropathies due to specific infection gave low stabilities.

C. (6) *Rheumatoid Arthritis* (Infective Arthritis) (190 cases).—As will be seen from Diagram I, this subgroup has the lowest stability. In many cases the corpuscular volume diminishes so rapidly within the first 30 minutes that an observation after 15 minutes is desirable and readings after 1 hour are unnecessary.

As is well known, this type is particularly prevalent in women and, in the majority of cases, the disease is well advanced before systematic treatment is sought by the class of patient admitted to the Devonshire Hospital. If a definite focus of infection can be found and eradicated, a marked improvement in the general symptoms and in the suspension stability results, but an absence of improved stability in numerous cases suggests that the discovery and removal of the infective focus or foci is a matter of considerable difficulty.

This group shows the smallest percentage of improved stability after treatment and also the greatest number of cases in which the clinical report on discharge does not agree with the stability indication. Until more is known as to the nature of the changes that reduce the suspension stability these anomalies can only be recorded for reconsideration at some future date. At present it remains to be stated that either the stability does not improve *pari passu* with the reduction in the morbid process, or, more probably, that the subjective symptoms on which the clinical report is based are misleading and that the patient has obtained relief from pain and greater joint mobility although the infective process remains unchecked. A case suggestive of the latter hypothesis is as follows :—

C. R., female, aged 64, was first admitted August 8, 1924, as a typical case of infective arthritis. She improved under treatment, but relapsed and was readmitted April 4, 1925. She again improved in the hospital and again relapsed; a similar sequence of events occurred in subsequent years. During the last three visits the suspension stability has been determined; the admission curves are shown in Diagram II. On each occasion the stability showed no improvement on discharge, and the figures as a whole indicate a gradual deterioration in the general condition; this conclusion is confirmed by a comparison of the case sheet notes of 1924 and 1928.

In the absence of other evidence the clinical report is necessarily determined by the pain sensations of the patient, and by the mobility of the joints, or by both; and in these respects this patient undoubtedly improved on each occasion. For example, on the last admission she was unable to walk up stairs, but could do so on discharge. The fact that the patient returns for treatment testifies that the subjective symptoms are relieved, but the stability results indicate why she is *compelled* to return: the causal infective process is unchecked and progressive.

If each treatment is considered separately, the clinical and suspension stability results do not agree, but when the course of the case over the four years is considered there is a striking agreement which demonstrates the value of the stability test.

A word of warning should be given that it is useless to expect an improvement in suspension stability during treatment with vaccines, bacterial endotoxins, or non-specific proteins, as all of these tend to reduce the stability temporarily. The test should be delayed until about a week after the last injection.

(7) *Osteo-arthritis* (thirty-eight cases).—The results are shown in Diagram I, (p. 17) and suggest that this subgroup is composed of two different types, one in which the stability is normal, or nearly so, and possibly traumatic or static in

origin, and the other in which it is materially lowered and possibly infective in origin.

Kahlmeter (loc. cit.) reports that osteo-arthritis always shows a normal value, but as his cases would be classified as malum coxæ senile under the English system, they would be comparable with the first type and the results are in agreement.

It is also of interest that the cases in the first group are mostly monarticular and free from Heberden's nodes, whereas the second group tends to be polyarticular, with development of Heberden's nodes.

It is also possible that a normal stability indicates a quiescent or arrested condition, although the osteo-arthritic changes remain.

(8) *Gout* (105 cases), Diagram I.—The results depend upon the activity of the disease, the stability being low during and immediately subsequent to an acute attack and returning to a normal or slightly subnormal value during the quiescent periods. As was noted by Kahlmeter, the suspension stability improves with the

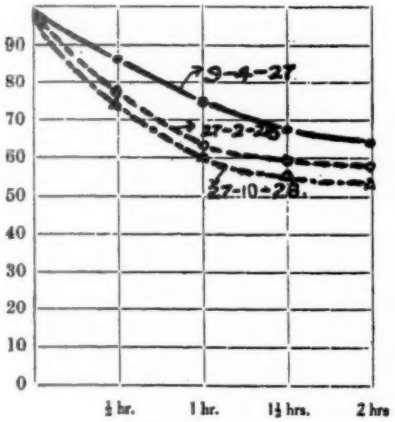

DIAGRAM II.

CLIMACTERIC-ARTHRITIS

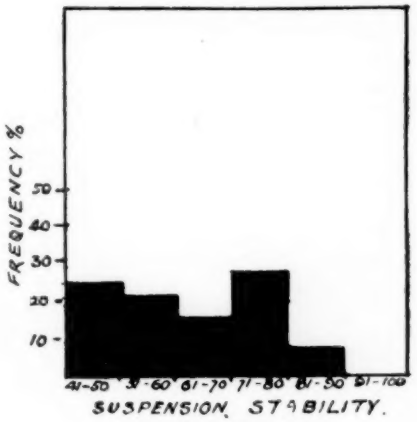

DIAGRAM III.

reduction of the uric acid content of the blood, but there is no definite evidence that these alterations are directly related as effect and cause.

If a typical case of gout shows a low stability several weeks after recovery from an acute attack, some other cause for the lack of stability should be sought, and in doubtful cases a low stability suggests an infective arthritis rather than gout, or a combination of the two.

(9) *Chronic Joint Changes, Unclassifiable.*—Under this heading a number of cases (twenty-three), diagnosed as climacteric arthritis, have been grouped.

It will be seen from Diagram III that there is a marked reduction of stability that can scarcely be accounted for by purely metabolic errors. It is at present impossible to state whether the results could be attributed to endocrine disturbances, as there is practically no information on this point.

Diagnosis and Prognosis.—The results so far recorded indicate that little or no change occurs in the suspension stability in metabolic errors and in primary neuroses. Any pyrexial condition tends to reduce it, and in mild cases the temperature and loss of stability are definitely correlated. In acute conditions such as pneumonia the test is practically of no value owing to the magnitude of the loss of stability; it

is too delicate for this purpose. In gout, a low stability during the quiescent stage suggests a concurrent low-grade infection and search should be made for possible foci. The same applies to sciatica unless the nerve involvement has led to tissue-wasting. In the so-called infective group, in which there is no increase of temperature, the humoral changes produce marked instability of the corpuscular suspension, but similar changes, though in a minor degree, are usually associated with fibrositis and certain types of osteo-arthritis. The test is not specific and is of comparatively little assistance in the differential diagnosis of these types of rheumatic diseases.

In following the course of the disease and the effect of treatment, I hold that the test is of the greatest value and should be given an extended trial. Though some apparently anomalous results may be encountered, they will only stimulate the interest of serious workers and lead the sooner to their elucidation. One great advantage is that the test is so simple that it can be performed by any practitioner in his consulting room; further, a definite numerical value is obtained strictly comparable with results obtained on any other occasion—provided the simple precautions described at the beginning of this paper have been observed.

The Nature of the Reaction.—Blood-corpuscles possess a higher specific gravity than that of plasma (1·085 : 1·031), and, in the absence of other forces, would tend to settle in accordance with the general physical law of Stokes as expressed by the equation

$$v = \frac{2r^2 (S_1 - S_2) g}{9 \eta}$$

in which r is the radius of the suspended spheres,

S_1 is the specific gravity of the suspended particles,

S_2 ,, ,, ,, ,, medium,

η ,, viscosity coefficient of the medium,

and g ,, gravity constant.

This equation applies only to spherical bodies suspended in a free medium, and as these conditions are not satisfied in the ordinary blood-suspension stability test, the law is only partially applicable. Red blood-cells are not spherical but biconcave and discoid in shape, and Ponder [11] has found that the rate of sedimentation in a free medium is more accurately expressed by the equation

$$v = \frac{2 ac (S_1 - S_2) g}{7 \cdot 65 \eta}$$

in which a and c are the axial dimensions of a flat, circular, discoid cell. The medium, owing to the large number of red cells, is not free, and the rate of settlement even in the absence of rouleau formation is much less than the 4·5 mm. per hour calculated from the above equation. The free space for movement is restricted from the commencement of the test and becomes less until, as pointed out by Malone (loc. cit.), the red cells and associated plasma form a mass of contiguous corpuscles when the apparent corpuscular volume is about 62 per cent. This is based on the usually accepted data as to the dimensions of the average erythrocyte, from which he calculated that a parallelopiped of the same dimensions as the cell (i.e., a rectangular box just large enough to contain one corpuscle) is $1 \cdot 32 \times 10^{-10}$ c.c. The volume of the corpuscle is $0 \cdot 82 \times 10^{-10}$ or 62 per cent. of that of the parallelopiped it would occupy. This gives a mathematical basis to the retardation of the sedimentation at about 65 per cent. stability, which is reflected in the flattening of the curve about this value.

The factors involved in Stokes' law and Ponder's modification of it have been extensively studied by Fahraeus (loc. cit.) and other observers, and have been found to be entirely inadequate to explain the differences between the stabilities of normal and pathological bloods. The specific gravity of the corpuscles and plasma, the viscosity and the size of the corpuscles vary in pathological states, but are insufficient to account for tenfold increases in the sedimenting velocity.

Rouleau Formation.—It is now usually accepted that the accelerated rate of sedimentation in morbid conditions is due to the formation of rouleax which results in a great increase in the effective dimension of r in Stokes' equation and ac in Ponder's modification. This so-called agglutinating tendency of the red cells in pathological bloods has been known for over a century and has intrigued clinicians and physiologists up to the present date. One of the earliest descriptions is that of John Hunter who, in a lecture in 1786 stated, "In all inflammatory depositions in the solids, whether universal or local, the blood has an increased disposition to separate into its component parts, the red globules become less uniformly diffused, and their attraction to one another becomes stronger, so that the blood when out of its vessels soon becomes cloudy or muddy and dusky in its colour, and when spread over any surface it appears mottled, the red blood attracting itself and forming spots of red." In spite of the work that has been done since this description was given, no explanation has yet been suggested which satisfactorily accounts for the phenomenon of rouleau formation.

The great majority of observers agree that whether coagulation is prevented by the addition of oxalate, sodium citrate, or by defibrination, rouleau formation proceeds and, under normal conditions, largely determines the results obtained in the stability test. Fahraeus, working with saline-washed corpuscles and serum, stated that he "had a series of tests with admixture of corpuscles standing at room temperature for a week, and repeated mixing day after day has resulted in the characteristic curve." I have obtained similar results with defibrinated human bloods, but numerous experiments with oxalated bloods show that this does not apply when this anticoagulant is employed. With normal bloods and those of comparatively high stability, the first curve can be repeated at any time up to 8 to 10 hours on re-suspension of the corpuscles by gentle mixing, but with bloods of low stability the results are affected in the direction of increased stability even after one hour.

Microscopical examination of the oxalated bloods of low stability showed that rouleau formation was very rapid and there was visible evidence of rouleau disintegration in a few hours. The biconcave discs apparently alter slowly in shape, some becoming biconvex, and others spherical, with a diameter about 66 per cent. of that of the discoid cells; definite crenation appears, and after 24 to 36 hours all the cells are spherical and nearly all are crenated. On further standing the cells are still spherical but the number of crenated ones is much reduced. In citrated plasma the dissolution of the rouleau proceeds more slowly and is reflected in a slower increase in the suspension stability after standing. In defibrinated samples the rouleau remain intact for several days and the stability is constant.

Examples of the effect of rouleau dissolution on the stability are given in the following table:—

SUSPENSION STABILITY OF OXALATED BLOODS.

Test made after	Stability per cent. after								
	1 hour A	1 hour B	15 minutes C	30 minutes C
0 hour	93	59	60	48
1 hour	—	—	60	49
6 hours	93	60	89	57
12 "	94	—	96	94
24 "	96	93	98	97
48 "	98	98	—	—

The change from the discoid to the spherical type on collecting blood in saline or citrate has been known since 1882 [12], and was studied later by Hamburger [13], but the spontaneous disrupture of the rouleaux on standing in contact with serum was first recorded by Sellards [14] and was confirmed by Ponder [15], who showed that plasma was equally effective.

According to Brinkman and van Dam [16] the cells retain their discoid form in saline and only become spherical in this fluid if they come into contact with glass, but this explanation cannot account for the differences in the appearance of the

samples of blood mentioned above which were taken at various intervals and examined at once. The changes in the shape of the cells cannot be ascribed to internal adjustments caused by the loss of carbon dioxide from the plasma, as they occur when gaseous exchange between the plasma and air is prevented by covering the surface of the sample with paraffin oil.

The forces concerned with the formation of rouleaux have been reviewed recently by Ponder (loc. cit.), who considers that changes in the potential difference between cells and plasma and in the cohesive and electrical forces do not offer an adequate explanation of the facts observed. The change from the discoid to the spherical form suggests changes in the surface tension and the hydrophilic properties of the cell contents, but the manner in which they are initiated remains to be discovered.

Is the Determining Factor contained in the Plasma or Cells?—Fahraeus, after interchanging the plasmata and corpuscles of various samples, concluded that the main factors determining the suspension stability are present in the plasma, and this view was generally accepted until Newham published results [17] from which he concluded that the cells contained the determining factors. Experiments on the

INCOMPATIBLE BLOODS.

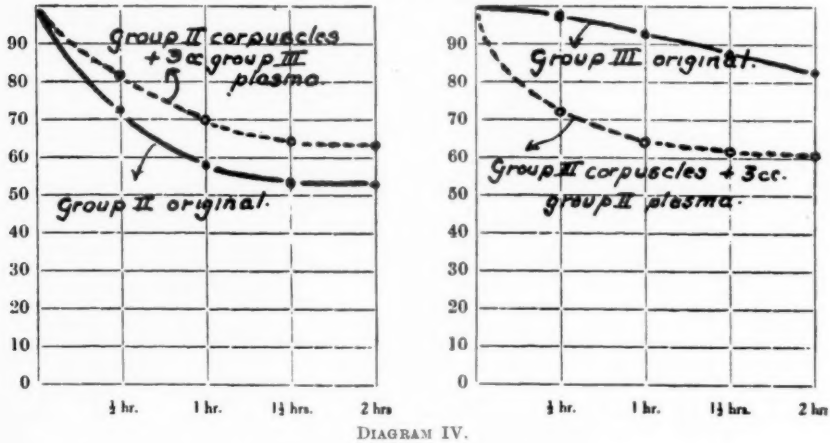

DIAGRAM IV.

effect of corpuscular and plasmata exchange had been made before the publication of Newham's work, but in view of his findings it was deemed advisable to repeat and extend them. All the results confirmed the findings of Fahraeus, and showed that the main factor resides in the plasma.

In these experiments precautions were taken to avoid mixing incompatible corpuscles and plasma, although it had been found that the transfer of about 50 per cent. of an incompatible plasma did not alter the significance of the results. Diagram IV (p. 22) shows the result of a typical experiment, and Diagram V those from one with compatible bloods. The exchanged plasmata were obtained by natural settlement as it was found that when they were separated in the centrifuge the red cells, in some cases, did not behave normally after re-suspension. If natural settlement is prolonged the break-up of the rouleaux might vitiate the results; hence it is inadvisable to attempt the transfer of more plasma than can be obtained in two hours. Cells that have been centrifuged and washed with saline are useless for such experiments, for Seizue [18] has shown that such treatment changes the electrical charge of the corpuscles from negative to positive. When an equal volume of isotonic saline is added to red cells a very stable suspension results.

In all reports of experimental work on cell and plasma interchange in relation to suspension stability, although sedimentation is always ascribed to agglutination, it is tacitly assumed that agglutination of the nature found in ordinary serological work (i.e., the action of an agglutinin on an agglutinogen) does not occur; the specific

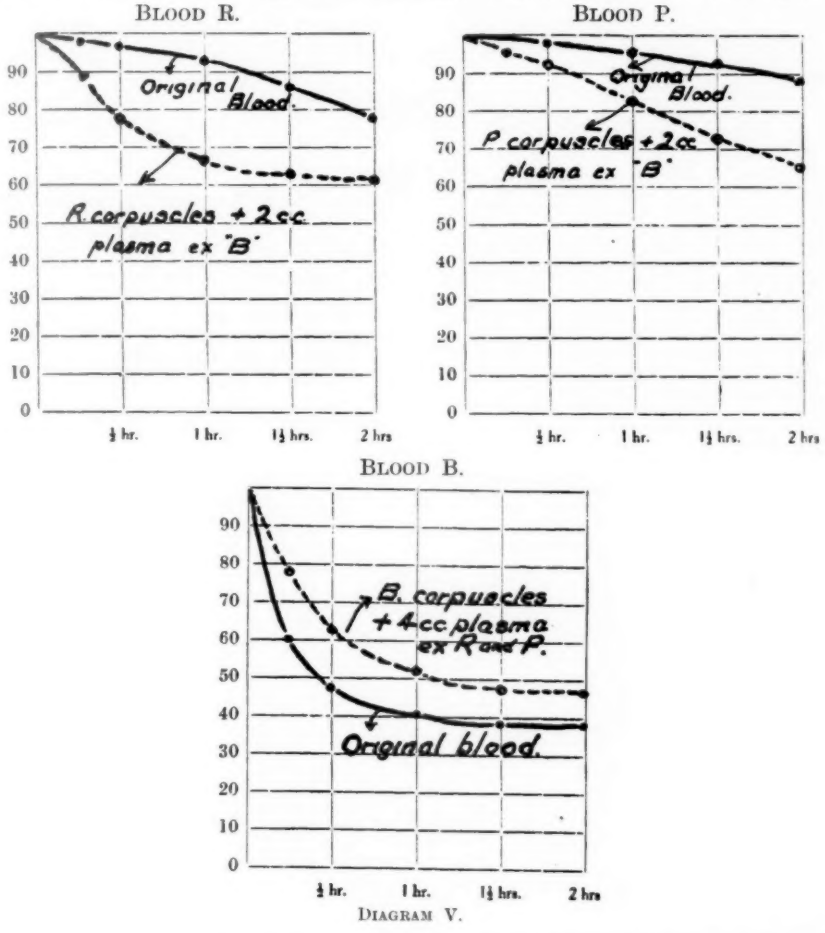

Diagram V.

agglutinins are absorbed and the system cannot be redistributed by gently inverting tubes of cells and plasma.

Schloss [19] has shown that there is no relation between suspension stability and true hæmagglutination, and Landsteiner and Witt [20] have pointed out that "pseudo-agglutination" found in bloods of very low stability differs from "auto-agglutination," as: (1) the former is reduced by dilution, (2) the active substance of the former cannot be absorbed by the red cells, and (3) the former action tends to increase with temperature whilst the latter does not.

If blood is regarded as a colloidal system, the cells can be considered emulsoid particles negatively charged in relation to the serum-medium containing electrolytes and proteins, the potential difference being about 9 millivolts (Ponder, loc. cit.).

Electrolytes generally reduce the stability of colloids, and, according to Fahraeus (loc. cit.) the proteins, albumin, globulin and fibrinogen also have an instabilifying effect, albumin having very little and fibrinogen a very marked action, with globulin occupying an intermediate position. The action of fibrinogen is demonstrated by comparing oxalated (or citrated) and defibrinated bloods, the latter having a much higher suspension stability.

In view of the well-recognized properties of albumin and globulin as protective colloids for negatively charged particles, the conditions obtaining in the experiments of Fahraeus demand careful consideration. The corpuscles employed were obtained by centrifuging and washing with saline; to these a mixture of the protein under consideration in dilute ammonium sulphate solution was added, with sufficient ammonium sulphate to make the whole solution isotonic. Centrifuging and washing with saline alters the shape of the corpuscles and may also result in the adsorption of sufficient electrolyte to impart a positive charge to the corpuscles; these factors alone may be sufficient to vitiate the results obtained with such material except perhaps those in which the proteins are compared one with another.

If this view is accepted we may consider that the plasma proteins, with the exception of fibrinogen, act as protective colloids, and that reduction leads to decreased stability of the corpuscular suspension. If fibrinogen is regarded as the precursor of both globulin and fibrin, increase in the globulin fraction would be accompanied by the instabilifying fibrin and decrease in the albumin-globulin ratio by a lowered suspension stability.

Fahraeus also attributes the decreased stability in pathological conditions to disturbance of the plasma proteins, and quotes analyses in support of this view. He suggests that the change should be designated as "globulin increase," but emphasizes that this term embraces a qualitative as well as a quantitative, change of the plasma. There is much evidence that these changes in the plasma proteins are accompanied by decrease in the "gold sol" values of the plasma,—pointing to reduction in the protective power of the plasma.

In addition to diseases mentioned by workers in which changes in the plasma proteins are accompanied by loss of suspension stability, similar changes were found to occur during active immunization [21], either with living or dead bacteria, or with bacerial endotoxins. Vaccine therapy, as mentioned, also produces temporary loss of suspension stability, probably the result of the changes in the plasma proteins.

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Dr. C. W. BUCKLEY: I have found the test of great value, especially in three respects; it appears to indicate clearly in infective rheumatic conditions how much success has been attained in dealing with septic foci and overcoming the infective factor; it is of great value in various forms of fibrositis which may have arisen either from physical causes or from an infection, the curve being quite different in the two conditions, and one is thus enabled to differentiate them and to adapt the treatment accordingly; I have also found it useful in those forms of gout which sometimes closely simulate infective arthritis, the curve given by the test often serving to differentiate them clearly. The test should soon be recognized as a valuable addition to clinical methods.

Section of Ophthalmology.

[*January* 11, 1929.]

CASES.

Tumour of Ciliary Body.—R. FOSTER MOORE, O.B.E., F.R.C.S.—Patient, L.P., male, aged 10. Noticed three weeks ago that vision in the right eye was defective. Four weeks ago was struck in right eye with india-rubber ball, after which there was some pain and redness.

Present Condition.—Left eye normal, vision $\frac{6}{5}$. *Right eye.*—Some enlargement of conjunctival vessels, especially on nasal side. Cornea, bright, K.P. present, aqueous hazy. Anterior chamber rather shallow temporally. Pupil reacts to light, reaction not sustained; right larger than left. Lens dislocated backwards on nasal side; posterior opacity on nasal side. Some uveal pigment can be seen as if pushed forward by something in ciliary region. Fundus not seen. Vision: finger counting at $\frac{1}{2}$ metre.

Transillumination, no definite opacity.

X-ray Report.—"In A.P. view the right orbit appears somewhat less dense than the left. No neoplasm is shown." 7th January, 1929.

I shall be glad to hear any suggestions, either as to treatment or diagnosis.

It would seem certain, from the presence of keratitis punctata, that it must be an inflammatory condition. X-rays prove the absence of any metallic intra-ocular foreign body. The cyst-like body is translucent.

Discussion.—Mr. M. S. MAYOU said that some years ago he had a case similar to this, in a child. It was a cyst of the pigment epithelium of the iris. Mr. Treacher Collins had recorded one or two such cases. He did not remember whether there was a history of trauma in his own case, but there had been iritis.

Mr. HUMPHREY NEAME suggested that in this case the keratitis punctata might be due to lens matter. It did not strike him that the lens had been absorbed at all—he heard a Member suggest that absorption of the lens had taken place, or else a displacement of it. If there was a ruptured capsule and a splitting up of the lens, it was conceivable that some of the lens matter might have been carried forward into the aqueous and have been deposited as keratitis punctata on the posterior surface of the cornea.

Some years ago he had reported pathologically a case of cyst of considerable size in a man who was shown at the Section, the general opinion expressed being that it was a growth of the choroid. It was a much bigger swelling, reaching up from below, in the same way as did sarcoma of the choroid. It gave a shadow, even well forwards. All looked upon it as a malignant growth of the choroid. Upon the eyeball afterwards being sectioned, however, the mass proved to be a cyst of the retina. On transillumination during life it had been dark owing to the presence of blood in the cyst. That was a distinction from the present case, in which the cyst was translucent, whereas in the case of which he was speaking the cyst was opaque, for the reason given. He would remind Members of the possibility of a cyst looking dark if it contained blood.

Central Choroiditis.—ROSA FORD, M.B.—Male, aged 57, was first seen in 1920, with a small central patch of choroiditis in the left eye, and vision $\frac{5}{60}$. The Wassermann was then said to be slightly positive and he was sent to his doctor for treatment, and not seen again till 1923, when he came complaining of the other eye, in which was a small patch of choroiditis close to the fovea, with vision $\frac{5}{9}$. In the first eye was a large patch of choroidal atrophy.

There is now a large area of choroidal atrophy in both eyes.

The case is mainly interesting because of the difficulty in determining the ætiology.

Syphilis and dental sepsis can probably be excluded.

Careful medical examinations have failed to elucidate any other cause, and the patient's general health has remained excellent throughout.

There was no reason to suspect a nasal focus. He had no nasal symptoms nor history of such; his nose was declared free from any evidence of sepsis by three nasal surgeons, and, radiologically, the sinuses were clear. Yet the first check to the steady progress of the choroiditis in the right eye took place after the resection of a deflected septum undertaken as a last resource in December, 1924.

No further extension in the choroiditis of the right eye took place during the year which followed and vision rose to $\frac{6}{9}$, after remaining $\frac{5}{12}$ for six months. As hæmorrhages continued to appear in the other eye, the antra were then irrigated as a diagnostic measure, and, though the washings returned clear, vision, a month later, was $\frac{6}{6}$ in the right eye, and no fresh lesions occurred in either eye for nearly two years.

In June, 1927, the patient had a cold in the head and soon found himself unable to read. A month later his vision was $\frac{6}{36}$, but no fresh lesions were evident in the fundus. Later, however, an acute exacerbation of choroiditis occurred in his right eye, numerous hæmorrhages appearing during the next four or five months.

During this time he underwent tonsillectomy with removal of portions of turbinates; the antra, the ethmoids and sphenoids were opened, and a small polypus removed from the left ethmoid. He also received a course of treatment for intestinal sepsis and another course of treatment by autogenous vaccines prepared from the secretion of the prostate, which contained streptococci. Finally, the choroiditis again became quiescent and there have been no fresh lesions for about a year in either eye. Vision has, however, not risen above the $\frac{2}{60}$ to which it had been reduced.

In view of the success of the first nasal operations, of the fact that relapse followed close upon a cold in the head, and perhaps also of the fact that a polypus was found in the ethmoid, there seems good reason to suspect a nasal focus. If that is correct, it is remarkable that so perfectly normal a nasal mucous membrane should have concealed active mischief in the sinuses, of sufficient virulence largely to destroy the sight of both eyes.

The marked recovery of sight after such prolonged diminution and the fact that during the relapse, with vision as low as $\frac{6}{36}$, no fresh lesions were observed in the fundus, point to some factor acting with the choroiditis in producing the loss of vision. Possibly this factor is a retrobulbar neuritis.

Argyll-Robertson Pupil due to Head Injury.—E. WOLFF, F.R.C.S.—Patient, a male, aged 41, received a head injury in November, 1927, resulting in a fracture of the base, of the right superior maxilla, and of the right mandible. I saw him in April, 1928, and his pupils were then inactive to light but reacted on convergence. I had just read a paper mentioning a number of cases in which there were said to have been Argyll-Robertson pupils due to head injury; therefore I watched this case very carefully. He was taken into a dark room every fortnight and the pupils tested. He retained the bilateral Argyll-Robertson pupils until July, 1928, and then the *right* eye recovered. It reacted to light directly and consensually, but the *left* eye remained inactive to light both ways. Wassermann reaction negative. There is some slight atrophy of the right disc. The vision in each eye is $\frac{6}{5}$ and the *right* eye is placed lower and farther back than the left.

I made this diagnosis because of the large pupils, which are unusual in tabes, because there was recovery on one side, and because there is weakness of up-and-down movements on the left side.

These cases are thought to be due to a *contrecoup* injury of the mid-brain in the region of the superior corpora quadrigemina, and Mr. Wilfred Trotter tells me that it is the free margin of the tentorium cerebelli which causes the damage.

MR. ARTHUR COOKE, F.R.C.S., showed a Case of Albuminuric Retinitis.

MISS MARGARET DOBSON, M.D., showed a Case of Cyst of Ethmoidal Labyrinth causing Proptosis of Left Eye.

An Attempt to Classify (pathologically) the Various Fundus Pictures in Diseases of the Choroid.

By MALCOLM L. HEPBURN, F.R.C.S.

[ABSTRACT.]

MR. HEPBURN read a paper on the above subject, and by means of suitable fundus drawings endeavoured to prove that all pictures could be relegated to one or other of the following groups :—

(1) Inflammatory : (*a*) Acute stage ; (*b*) scarred stage.

(2) Vascular : (*a*) Acute stage ; (*b*) scarred stage ; (*c*) slow progressive changes due to disease or spasmodic contraction of the vessel walls.

(3) Hyaline, calcareous, and similar degenerations of the membrane of Bruch.

(4) New growths.

(5) Congenital ; including colobomata.

(6) Injuries ; leading to concussion changes and ruptures.

Both inflammatory and vascular conditions have been seen in the acute stage, but the various pictures in the scarred stage merely demonstrate the degree of pathological changes, the position in the fundus, or the depth of the choroid where such changes occurred.

He (the author) considered that the claim to regard such terms as choroiditis juxtapapillaris, retinitis circinata, and macular degenerations, as representing clinical entities, was unjustifiable and unscientific.

For choroiditis juxtapapillaris, usually associated with a sector-shaped defect in the visual field in the scarred stage to be described as a special entity, assumed that this type of field did not occur with other isolated patches of scar tissue, an assumption which is not true.

Retinitis circinata, as a partial or complete ring of exudates, again merely represented the outlying exudate surrounding a large inflammatory metastatic deposit, or an acute vascular obstruction, and, although more commonly seen round the macula, was by no means unknown elsewhere in the fundus. A circinate arrangement of exudates was often seen round the macula in retinitis, because the large retinal vessels encircle this region, and when the vessels are diseased the largest number of exudates tended to collect in their neighbourhood. In retinitis circinata, due to choroidal cause, a scar at the macula was always seen, but in retinal disease there was no such scar. This pictorial arrangement of exudate did not justify its being described as a special entity.

Again, it was suggested that macular degeneration was really only pigmentary degeneration of the retina occurring at the macula instead of at the usual mid-peripheral region, where we are accustomed to see it in its typical form. But the change in the picture, as shown by the granular form of pigment migration, was merely due to the difference in the anatomical arrangement of the various structures at the macula. The familial characteristics of pigmentary degeneration of the retina at the macula was only in keeping with what we already know about the same type of degeneration in the mid-peripheral parts of the fundus.

Under the head of slow progressive changes, due to vascular disease, the pathology of primary pigmentary degeneration of the retina was fully discussed in order to prove that the fine pigmentary migration associated with this class of case led to the typical arrangement seen in the mid-peripheral parts of the fundus, or to the granular type found at the macula ; and that both were due to the same cause, viz., destruction of, or interference with, the circulation in the chorio-capillaris.

Wagenmann's experiments on animals had proved, many years ago, that pigmentary degeneration of the retina could be produced by tying the posterior ciliary vessels,

and the same results could be shown to occur from constriction of the intermediate-sized vessels running in the stroma of the choroid by any undue formation of fibrous tissue. Such fibrous tissue did occur under certain conditions, and the effect on the retina was the same. In this case, however, the pigment migration was seen in the picture on a greyish background, and not on an otherwise normal looking fundus as in the primary degeneration form; therefore this type should be called secondary pigmentary degeneration of the retina.

The more extensive the pictorial changes and the larger the masses of scar tissue the more likely was it that they were due to gross pathological processes of an inflammatory nature or of vascular origin; while the smaller indefinite changes in the choroid associated with fine pigment migration were more likely to be due to gradual constriction of the vascular channels.

Myopic stretching of the choroid and retina, with disturbance of pigment and the formation of moles, were referred to merely to show that none of them could truly be called disease of the choroid, and therefore need not be considered at length in the present paper.

Hyaline and calcareous degenerations of the membrane of Bruch were discussed; and in this connexion attention was drawn to the fact that the word "degeneration" as usually applied to certain parts of the fundus (e.g., the macula) had no definite meaning unless the tissue actually involved was also specified.

Pictures of many borderline cases were shown and some difficulties in diagnosis were dwelt upon, but it was urged that an effort should be made to arrive at a conclusion which was quite possible in the great majority of cases. The pictures which did not follow the lines laid down in this paper would often prove to be of congenital origin.

New growths, colobomata, and injuries, were only lightly touched upon.

Juvenile Glaucoma.

By M. S. Mayou, F.R.C.S.

"Juvenile glaucoma" is a term applied to glaucoma occurring between the ages of 15 and 40 years. One of the first observers in England to investigate this condition was A. H. Thompson, who collected five cases and published them in the *Ophthalmic Review* for 1909. He pointed out that the condition was a chronic form of glaucoma with exacerbations, the exacerbations not being of the congestive type. Nearly all the patients present themselves for examination when one eye has become blind, or nearly so. A great many cases have since been recorded, in respect of which it seems that they occur more frequently in males, although the predominance is not as large as it was originally thought to be. In nearly all the cases recorded the anterior chambers are deeper than normal, and in some of the cases congenital remains in other parts of the eye have been found.

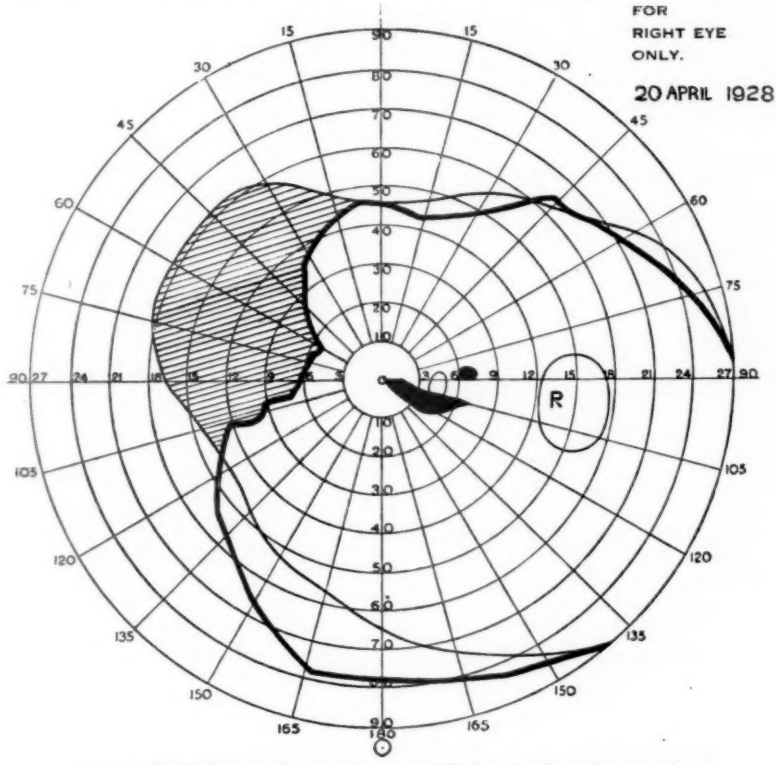
Considerable difference of opinion has arisen as to whether these cases belong to the congenital group, or whether they are an early manifestation of the senile form. Although this is really not of much importance, as they form a group by themselves, the facts that the anterior chamber is deep and that other congenital malformations are found are in favour of the assumption that the underlying cause is congenital in origin. Only one case has been examined pathologically, but the report is not sufficiently detailed to help on this point.

It is curious that in none of the published cases which I have examined have any records of fields been published. The detailed examination of fields in these cases is most important, not only from an ætiological point of view, but also from the point of view of treatment. These cases are rare, but I have been fortunate in having had two cases under my observation, in both of which, in addition to the

contraction of the fields, there were scotomata in the fields apart from enlargement of the blind spot. These scotomata were close to the fixation point and accounted for the loss of central vision in one eye of each patient, whilst the central vision of the other eye was seriously affected, and in one case there were scotomata in other parts of the field.

The following are the case records :—

Case I.—Male, aged 40. Consulted me first on September 24, 1924, complaining that he had lost his vision in his left eye. His Army notes showed diagnosis of glaucoma. On examination, the left field exhibited a large central scotoma with contraction of the nasal field. His vision was reduced to hand movements, so that no accurate chart could be taken. The disc was cupped and white, but although the diagnosis of glaucoma had been made the



CASE I.—Field showing the central scotoma blind spot and nasal contraction.

appearance of the fields and the slightly cupped white disc made it possible that a severe retrobulbar neuritis might have been an alternative diagnosis. Examination of the right field showed no change. He had $\frac{6}{5}$ vision. He was a somewhat neurotic patient and came to see me periodically, as he was uneasy about the condition of his right eye. In January, 1928, he began to complain that he was unable to see to read at times. He would sometimes lose half a word. On examination, there was no scotoma discoverable either for colour or for white. As he was a very heavy smoker, tobacco was stopped. During the next three months he was examined on three occasions, and although no scotoma could ever be detected either at the macula or enlargement of the blind spot, he was still at times unable to read. There was no

contraction of the nasal field. A week before admission to hospital the condition became very much worse, and the loss of half the word when reading became more or less permanent. When he came to see me the field was definitely contracted on the nasal side, and there was a scotoma reaching to the macula and passing down and out in a triangular sort of area entirely separate from the blind spot which did not seem to be enlarged. During all this time he had been using eserine for the eye. On March 17, 1928, the right eye was trephined and a diathermy puncture was performed for the left. During the short period in which he was waiting for operation the scotoma had slightly increased in size. The patient made an uninterrupted recovery and obtained $\frac{6}{5}$ vision with correction. There is still a small scotoma, but not so extensive as before.

Case II.—Glaucoma in a boy, aged 17.—Patient seen by me in 1910. Vision $\frac{6}{9}$ with right $-2 \cdot 5$, left $-1 \cdot 5$. Always delicate; has had large tuberculous glands in the neck. Since August, 1914, his sight began to fail. Vision: right eye $\frac{6}{6}$, left $\frac{6}{12} - 1 \cdot 75 = \frac{6}{9}$. No history of haloes. The anterior chambers are somewhat deeper than normal, but there is no sign of any adhesion in the angle of the chambers. No K.P. or signs of irido-cyclitis, the vitreous being quite clear. The optic disc in each eye presents a deep glaucomatous cup. There is a contraction of both fields. In the right eye the nasal half of the right field is largely affected, and it passes up to the fixation point. In the left eye the nasal half is affected to a much smaller extent, and below the macula there is a large scotoma which is absolute, and again between the 20° and 30° circle there is a smaller one. The patient was trephined in the right eye on January 5, 1915, and in the left eye on January 19, 1915. He made an uneventful recovery from the trephining operation, the areas of which leaked freely after the operation; the only point of interest being that he had severe hæmorrhage from the right iris at the time of the operation, and in both eyes there were repeated small hæmorrhages into the anterior chamber subsequently (no history of hæmophilia). Since the operation, although the tension does not seem to have increased above the normal, there seems to be an increase in the size of the scotoma below the fixation point in the left eye, and it now nearly reaches the fixation point. The patient complains that coloured light obscures his view of objects.

This second case, as above reported, was shown before the Section of Ophthalmology on March 3, 1915.[1] The Chairman (Mr. Lawford), in making remarks afterwards, said that he did not consider the presence of scotoma continuous with the blind spot unusual. Unfortunately I did not point out at the time that this scotoma was not originally connected with an enlargement of the blind spot.

I have examined two cases of buphthalmos under my care which I operated upon in the patients' childhood, and in which the patients have now good vision. They do not exhibit any scotoma in their fields. Similarly in the senile form, apart from the scotoma associated with the blind spot, it is rare to find isolated scotomata elsewhere in the fields of vision. How exactly to account for the presence of these scotomata is a little difficult, unless it be assumed that they are due to pressure on the nerve fibres passing over the edge of the disc by some anatomical distribution of the retinal vessels. The great importance of this scotoma, clinically, is that as a rule patients usually only present themselves when the sight of one eye has gone and the second eye is beginning to show signs of the scotomata in the macula region before the peripheral fields or the blind spot are affected. Therefore valuable vision may be lost before the nasal field or blind spot becomes affected, if the condition is not recognized. Operations for the relief of tension seem to be always successful.

[1] See *Proceedings*, 1915, viii (Sect. Ophthalm.), 92—94.

Section of the History of Medicine.

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Notes on the Early History of the Veterinary Surgeon in England.

By Fred Bullock, LL.D.

The field of Veterinary History has been so thoroughly garnered by Major-General Sir Frederick Smith as to make it difficult to do more than glean here and there. It was my privilege to be associated to some slight extent with his *Early History of Veterinary Literature and its British Development*, and that experience gave me—who am neither veterinary surgeon nor historian—an interest in the subject. In the following pages I have constantly referred to Sir Frederick Smith's work.

When the Latin author Columella wrote his *De Re Rustica* about the time of the second invasion of Britain, he described the medicine of animals under the new term "medicina veterinaria," but makes no mention of the shoeing of horses. By A.D. 110, however, the Roman Army had a *veterinarium*, with a forge attached to it. Its place in the camp is described by Hyginus Gromaticus (*Liber de Munitionibus Castrorum*, 21, §22). It was a hospital for sick and wounded horses and other animals. If, however, before the Romans left Britain in A.D. 400, "medicina veterinaria" was practised in this country, no evidence of it remains; nor did the "veterinarius" or veterinary surgeon, if he existed here in Roman days, answer to that name again for 1400 years.

The Ancient Laws of Wales show that in the tenth century such equine diseases as staggers, strangles, farcy, worms and restiveness, such cattle diseases as scabies, rot and redwater were known. The horse is to be warranted against the staggers for three dew-falls, against the black-strangles for three moons, and against the farcy for one year. In the case of mange the purchaser must swear that he put the animal on a clean pasture and in a stable in which there had not been mangy animals for seven years. One need not suggest that it was a veterinary surgeon who would be called in to give his certificate of soundness in the tenth century, but when the claim under the warranty was made some expert person would doubtless be consulted to settle whether the disease was the one warranted against. It was also specified in the Laws that payment must be made to one who gives a remedy *to a man or to an animal*. This seems to show that human and animal medicine were the work of the same leech.

In the Anglo-Saxon Leechdoms, which contain a number of veterinary references, there is no separation of them from the general body of medical matter.

After the Conquest all is dark for a while, but the growth of chivalry with the Crusades must certainly have given a great impetus to equine medicine and surgery. The Chevalier or Knight was the fully equipped feudal tenant on horseback. He made it his pride that he could shoe his horse and treat it in disease. It is in this period of the heyday of chivalry (1200 to 1500) that the doctor of horses separated off as a specialist from the general practitioner, much as the surgeon and dentist separated off. We now meet with a new term to describe the doctor of horses, namely, *Marshal*, his profession being that of *Marshalsy*. All the great houses of the Nobles would certainly have their marshals and smiths as part of their domestic establishment.

The origin of this word is Teutonic, Old High German *Marah-skalh*, the "horse-groom," which became in Italian *mariscalco*, *maliscalco*, and *maniscalco*, and in Old French *mareschal*, from which we get the English *marshal*. This word ousted the

Latin *veterinarius* for several centuries. Doubtless it came into use during the German invasion, grew into importance during the Crusades, and secured a new vogue under the influence of Jordanus Ruffus, the Master Marshal of Frederick II, the *Stupor Mundi*. This great German Emperor, King of Sicily from 1216 to 1250, a student of zoology and of medicine, the most accomplished sovereign of the Middle ages, was keenly interested in veterinary medicine, and encouraged his Master Marshal to produce a work on this subject. So Ruffus, himself styled *imperialis marescallus major*, wrote, with Frederick's help, a treatise commencing "Izi cominza lu libru di manischalchia compostu do lu maestro Giordano Ruffo . . . mariscalco del imperatore Federicu." This work had an immense vogue. It was copied and translated into many languages, and we possess in the British Museum at least three different mediæval manuscript copies, viz., Harl. 1637, 3535, and 3772. The *veterinarii* of Rome now became *mariscalci*, and *ars veterinaria* became *marescalcia*.

Very soon we meet with "marshals" in England. The earliest reference seems to be in Wace's *Chronicle*, 1330, but the Master Marshals there referred to appear to be no more than horse-grooms. In 1356, however, the Master Marshals of London obtained an ordinance from the Mayor and Aldermen, giving them authority to rule the craft. Riley, in his translation of this Ordinance, writes "Master Farriers" for "les bons genes mestres mareschals," but as the word "marshals" was already in full use, it seems proper to retain it here. The Ordinance itself shows too that there was already a distinction between the craft of shoeing, and the "mestier" of "marchalcye des chivaux."

It is interesting to note in passing that the good folks of the trade agreed with the Masters "that they will well and loyally advise all those who shall ask counsel of them as well in the purchase of horses as in their cure . . . and that no one of the said trade shall commence or undertake any great cure if he does not reasonably see at the beginning that the same cure will be brought to a good end; and that if any person shall undertake any great cure and shall fear in his conscience that the same will take a disastrous turn, then in such case he shall come before the masters and other wise men of the said trade to ask their counsel and aid for the saving of the horse and for the profit of him to whom the horse belongs and the honour of the said trade." . . . "And if the contrary be found or it shall be proved against any person that through negligence he has let such horse perish, then he shall be accused thereof before the Mayor and Aldermen and be punished at their discretion in the way of making restitution for such horse to the person to whom the same belongs."

The rule as to consultations in difficult cases was similar to that found in the contemporary Surgeons' Gild, but the surgeons did not have to make restitution for the patients they lost! Those who took up animal surgery found this great obstacle always before them, and to-day the progress of veterinary medicine is handicapped by the fact that the surgeon is, as a rule, not allowed to treat a case unless it is economically worth treating. On the other hand, there have been those who have been thankful that their patients had no soul to be saved, saying "For if they had, the Lord have mercy on us all!"

Shortly after the Marshals' Ordinance of 1356, we find a case in the Year Books where a Marshal was sued for negligence in treating a horse, by reason of which the horse died.[1] Again, in 1441, we find the record of a writ against R. Mareshall for that he "assuma sur lui a Londres a curer un cheval d'un certein maladie et qu'il adeo negligenter et improvide imposuit medicinas suas, etc., quod equum, etc., interfecit."[2]

In an earlier case, brought in 1374, against a surgeon with the ominous name of J. Mort, who had, it was alleged, negligently treated an injured hand, it was argued,

1 *Y. B.*, 43 Ed. III, f. 33, pl. 38 (1369).

2 *Y. B.*, 10 Hen. VI, f. 49, pl. 5.

"If I deliver my horse to a farrier to shoe, and if he pricks my horse, I shall have a writ of trespass against him, or if my horse is wounded by being pricked with a nail, and I deliver him to a Marshal to cure, and by his negligent treatment my horse is maimed, I shall have a writ on the matter."

It is clear, therefore, that at this date, 1374, there is a distinction between the Farrier or Smith who shoes the horse and the Marshal who treats the horse for injuries.

The name *Marshal* for the doctor of horses, and *marshalcy* for his profession, persisted certainly until the eighteenth century. The *New English Dictionary* gives examples from 1389 and 1430, and one from 1506 in the Lord Treasurer's Accounts, Scotland, shows its use as a verb: "Item, to deif Andro, that marshalit foure horse to the King."

That the marshals were of considerable importance in the life of the City is evident from the precept issued by the Mayor and Aldermen of London in 1431, directing the sheriffs that they must not summon any "marescallos equorum" to serve on juries, except in cases of urgent necessity.

At this period, the fifteenth century, there were in circulation a number of treatises on Marshalsy. In Mrs. Singer's list of Medical MSS. there are several in Latin, French and English. The following titles in English are the most interesting for our present purpose:—

British Museum. Harl. 6398, the Boke of Marchalsi
Cambridge (219) Dd. iv. 44, "This is the marchalcie of Piers Moritz."
Oxford, Ball. Coll. Cod. 354 (vii) "The Book of merchalcie or Treatise on the breaking and managing of horses with medicines and modes of treating them in illness."
Oxford, 8608 (1697 cat.) "The book of merchalsy for curing horses of maladies."

Harl. 6398 is a copy of the same work as Harl. 5086, and these appear to be the earliest English MSS. in which the word "marshalsy" occurs. The work itself is analysed by General Smith in Vol. I of his *History* and judged to be of poor quality, not comparable to that of Jordanus Ruffus.

FitzHerbert, who wrote a celebrated *Boke of Husbandrie* (*circa* 1523), described the veterinary practitioner of his day as "horse-leach," "he that taketh upon him to cure and mend all manner of diseases and soraunces that horses have," but he does not seem to have had a very good opinion of him, or for that matter of the contemporary apothecary. He himself writes well on the diseases of horses, cattle and sheep, and was the first to ascribe liver fluke to the presence of snails in the pastures.

Henry VIII, during whose reign several Acts were passed prohibiting the export of horses, and providing for the better breeding of horses, brought into the country, in order to take charge of the Royal Studs, the famous Italian Master Marshal Hannibal, spoken of for centuries after as a great horse leach. As Serjeant Farrier he would have on occasion to assist the Serjeant Surgeon at the gruesome State punishment by mutilation as provided for by Act 33 Henry VIII, c. 12 (1541).

Under Elizabeth we know that Martin Hollyman or Alman served as Serjeant Farrier, being in charge of the studs at St. Albans, and Richard Snape as Yeoman Farrier.[1] In this reign Thomas Blundeville published his *Fower chiefest Offices of Horsemanshippe* (1565), but does not speak of Marshals, describing the practitioner as "Keper and Ferrer." Neither does he use the word "veterinary" though the work of Vegetius, *De arte veterinariæ sive mulomedicinæ*, had been published at Basel in 1528. Blundeville made use of some of the old English treatises in MS. to which we have referred and borrowed a good deal of information from the aforesaid Martin Alman, whom he describes as "Chief Ferrer to the Queen's Majesty," and "the most skilful ferrer that I know in all England." That "ferrer" was used by Blundeville

1 Letter from R. S., Lambeth Library, dated 30 July, 1594.

to mean a general practitioner in veterinary medicine is clear from the account of diseases which the Ferrer is expected to treat, and from the fact that he still makes a distinction between the smith who shod horses and the Ferrer who treats them for diseases and injuries. But Martin Alman is described in the Accounts of the Progresses and Public Processions of Elizabeth (1559-1569) as "Marshall Ferrer," an account of his for "curing and dressing the Queen's horses" being listed. T. Clifford in his *Schoole for Horsemanshippe* (1585), speaks of that "famous Marshal Martine Alman," and the Duke of Newcastle in his great work of horsemanship (1667), calls him "Prime Marshal to Queen Elizabeth, an excellent Farrier." In the Accounts of the Reign of Charles I[1] is a bill of one of His Majesty's Farriers, for "marshalling His Majesty's Hunting Horses for the year 1626," but by now the title "Farrier" was ousting that of "Marshal."

Our military Annals show that in 1591 the "skilful Ferrar" who could judge of lameness and soundness was needed by the Muster Masters who conscripted for the Army[2]. In the famous law case of Powtuary v. Walton (1597) it was laid down that "si un farrier assume sur lui a curer mon cheval que est gravelled en ses pieds, & puis ita negligenter et improvide heald le dit cheval que ceo morust" there will be an action for negligence. Nevertheless the "Marshal" does not quite disappear for another century.[3] In 1678 Sir W. Hope published his *Parfait Mareshal or compleat Farrier*, and in 1639 Thomas De Grey in his *Complete Horseman* speaks of the "skilful ferrier or marshal" as being one most expert in horseleech craft, as distinguished from the "empyreticall horse leach." Even as late as 1720 Gibson in his *Farrier's Guide* still speaks of Marshals. Only once in this period do we meet with the term "Veterinarian" and that is when Sir Thomas Browne compares "common farriers" with "good veterinarians" (*Pseudodoxia Epidemica*, 1646).

It is clear that the Veterinary Surgeon of this period was both shoeing-smith and surgeon. Hexam (1637) describes the duties of the Army Farrier as "to drench and lett bloude the horse of the troupe, and alwaies, either upon a march or in quarter, to have in readiness his buggett of tools, horse shoes and nailes, whensoever he shall be called upon by his officers, or when any gentleman of the troupe shall have use of him, and for this reason that he must duly attend upon the Troupe he is freed from other duties and hath a greater paye than an ordinary horseman"[4].

In 1674 the Master, Wardens and Fellowship of the Craft and Mystery of Farriers of London secured a charter from Charles II on the ground that their "Art and trade is of great antiquity and of great use and benefit to our subjects for preserving horses, and that divers inexpert and unskilful persons . . . have taken upon them to practise and have destroyed many horses" . . . The Charter established the Company of Farriers of London, with rules similar to those for Barber Surgeons. Power was given to the Company, under certain safeguards, to visit shops, cellars and stables and to search for any defective works, medicines and ingredients used, and to take action against offenders. Andrew Snape (senior), Serjeant Farrier to Charles II, was in 1675 Master of the Company. He is also described in the State Papers as "Marshal Ferrier." His son Andrew, also a Member of the Farriers' Company, who claimed that his family had occupied the position of Farrier to the King for 200 years, was the author of an elaborate *Anatomy of an Horse* (1683), the first work of its kind in English.

During the eighteenth century we find Farriers treating horses, and cowleeches and cattle doctors treating cattle, sheep and pigs. They all, no doubt, were "hereditary" doctors, with recipes handed down from father to son, and they probably also made

1 Hore, *History of Newmarket*, ii, 6.

2 Smith, *Hist. R.A.V.C.*, p. 5.

3 Gervase Markham left on his death in 1637 an unpublished MS. entitled "Le Marescale or Horse Marshal." Smith, *Early History of Veterinary Literature*, 1919, i, p. 236.

4 Smith, *Hist. R.A.V.C.*, p. 6.

use of the wretched books published by Gervase Markham and his imitators. But in this century a number of medical men and others gave some study to the subject, and wrote books mainly on the diseases of horses, though the Cattle Plague of 1714 and 1745 brought out a few writers on diseases of cattle, e.g. T. Bates, F.R.S., 1717, Cromwell Mortimer, M.D., 1745, Richard Brocklesbury, M.D., 1746, E. Litton, 1750, D. P. Layard, F.R.S., 1757. The most important work was probably Sir William Hope's translation of Solleysel's *Parfait Mareschal* at the beginning of the century, in which, by the way, Hope described himself as a lover of Marshalry and Horsemanship.

Those who wrote on the diseases of horses usually spoke of the subject as Farriery, their books being entitled as a rule *The Gentleman's Farriery*. Among these we may note John Wood, who though he claimed "a natural genius for farriery" was not a member of the Farriers' Company and was very ignorant of the subject. He is noteworthy, however, for having in 1758 published a proposal to establish a hospital for horses for the public benefit. Among the subscribers to his book I find, in addition to the names of five London Farriers, the name of Sergeant Surgeon Ranby. Wood's proposal does not appear to have matured. The Farriers' Company was, however, like the College of Physicians at this time, more interested in keeping their circle closed and the entrance narrow, than in enlarging it, allowing the introduction of new men with new ideas. This is shown by their treatment of Wm. Osmer, a surgeon who, with his brother, "undertook the cure of horses in the various complaints, and having them shod in the proper manner." He declares in his *Treatise on the Diseases and Lameness of Horses* (1759) that his intention is to extricate the Science of Farriery from the hands of the ignorant and illiterate. Having issued some chapters of his book dealing with the Foot and Shoeing, to show the utility of his work, he says it was "tried and condemned in an august assembly as a matter of no use or benefit." Sir F. Smith[1] suggests that the august assembly was the Jockey Club. Since that volume was written I have discovered in the Archives of the Farriers' Company evidence which shows that Osmer was also "tried and condemned" by the Worshipful Company of Farriers. Though Osmer was, as General Smith says, probably superior to all his predecessors in his knowledge of the cause of lameness in horses, the fact that he dared to open a forge and to shoe horses as well as to treat them for diseases caused the Court of the Company to resolve on November 10, 1766 to prosecute him for carrying on business as a Farrier in Oxford Street, not being free of the Company. They sold out £25 of their South Sea Stock to meet the costs of the suit, but on April 2, 1767, the Master reported to the Company that they had been non-suited for want of sufficient evidence.

It is rather a humorous situation—Osmer on his side declaring that the farriers are illiterate, hidebound, prejudiced in favour of their antiquated methods, cruel in many of their remedies, and offering them a better way; on the other side the Company, seeing their craft in danger, appealing to the Court, only to be treated like Demetrius the silversmith, at the hands of the Town Clerk of Ephesus, though with not quite so much celerity.

The attitude of the Company to Osmer was but in keeping with a decision which had been taken as early as 1762, when they received a request from several noblemen "and gentlemen of the First Distinction" who had "suffered by the ignorance of unskilful quacks who assume to themselves the name of Farriers" to take action against all "foreign" farriers. Those who, after warning, did not join the Company were compelled to shut up shop. But unfortunately there was no attempt on the part of the Company to institute a better system of education and training for their members, and they were apparently averse to any such move. Edward Snape, who was a member of the Company and Farrier to George III, proposed, however, in 1765 to establish in London a Hippiatric Infirmary "for

[1] Vol. ii, p. 88.

the instruction of pupils in the Profession," but he received no encouragement from the Company. When, in 1778, he did establish his Infirmary, it was not a success.

During the latter part of the eighteenth century there appears to have been a considerable demand for veterinary knowledge, for the works of the writers of *Gentlemen's Jockeys* and *Pocket Farriers* ran into edition after edition. William Taplin, a surgeon, sailed on this tide into a fortune by selling medicine for horses by agencies all over the kingdom and advertising them by means of his books. The calls on the services of the few men who were capable of treating horses' diseases must have been very heavy. The poor horse suffered both from bad roads and bad shoeing, and once the feet were done the horse was done.

But a new era was at hand. When the attempts of Wood and Snape to establish a veterinary school fell through, the Odiham Agricultural Society in 1785 began to discuss "what were the most likely means to encourage the study of scientific farriery." In 1788 and 1789 they published an appeal in which, lamenting "that there is not yet in England any establishment adequate to the desired improvement in Farriery by a regular education in that science on medical and anatomical principles," they expressed their intention, if sufficient funds were raised, to send two or more students to be trained at the Alfort School near Paris. In 1789, however, Charles Vial de Sainbel came to England and issued proposals for reading lectures on the general knowledge of the horse, and in the following year he published a *Plan for establishing an Institution to cultivate Veterinary Medicine.*

It was this plan which ultimately displaced the project of the Odiham Society. It is well to notice that the Odiham Society speaks of "Farriery," Sainbel speaks of "Veterinary Medicine." A general meeting of subscribers to the proposed Veterinary College was held in April, 1791, when Officers were elected and regulations made for the constitution and organization of the College. The Odiham Society, on hearing of the actual establishment of the College, very magnanimously decided to drop their project; they sent on all their papers to the new College, and directed the subscribers to their fund to transfer their subscriptions to the new Institution.

Among the most active supporters of the College was John Hunter, who was described as "the life and soul of the undertaking." He was a Vice-President, and other Vice-Presidents included Sir Joseph Banks, P.R.S., and Sir George Baker, Bt., F.R.S. The curriculum was to cover a three years' course in Anatomy, the Exterior of the Horse, and external diseases, Materia Medica, Pharmacy, Botany, Shoeing, Pathology, Medicine, Surgery, Epizoötic Diseases; and, when the pupils had completed their studies, and had undergone a public examination in the theory and practice of every branch of the veterinary art, they were to receive a certificate if they were considered as perfectly instructed therein.

In the prospectus of the school, issued in 1793, the following paragraphs occur which are believed to be from the pen of John Hunter:

> "The incompetency of the persons to whom (the veterinary art) has been abandoned, has drawn contempt upon the art itself, and few have ventured to concern themselves with a profession that seemed incapable of conferring any honour upon those that exercised it . . .
>
> "It requires the sacrifice of as many years to become a skilful veterinarian as to become a skilled physician; the acquisition of the science and the practice of each is a task sufficient to engage one man's life, and the contrary opinion is a portion of that ancient error which, while medicine was regarded as the province of the learned and the few, supposed the veterinary art on a level with the most ordinary capacities."

The English school, therefore, began under good auspices, but it soon fell on evil days. Sainbel died in 1793, and though John Hunter quickly arranged for the students to attend lectures at his own theatre and those of other eminent teachers in London, the tragic death of Hunter himself speedily followed.

While the London school was limping along under the crippling influence of Coleman another school was founded by William Dick in Edinburgh in 1823. This

grew rapidly, and in many respects outstripped the London school. The rivalry of the two schools did not tend to the growth of a united profession, and very soon the veterinary practitioners began to urge the establishment of a single body to govern the profession. Nothing could be done while Coleman was alive, but meetings to this end began in 1841, and in 1844 a Royal Charter was obtained establishing the Royal College of Veterinary Surgeons, to consist of the graduates of the two existing schools and of any other schools which should be affiliated to the Royal College by Royal Sign Manual. The examinations were to be conducted by the new body, which was to appoint the examiners, control the curriculum, and award the diploma.

A Board of Medical and Veterinary Examiners was appointed, and it may interest this Society to know that the medical examiners included such men as W. T. Brand, Bransby Cooper, Robert Liston, Robert Knox, John Lizars, William Babington, S. Solly, and later Richard Quain, William Sharpey, and Alfred S. Taylor.[1]

Veterinary science was now a speciality. Up to this time I venture the suggestion that in England physicians and surgeons had attended to the injuries and ailments of animals when the occasion arose. All gentlemen were horsemen in those days at any rate, and the educated physician or surgeon would no doubt try his own treatment with his favourite horse, or with his friend's horse, though he might condescend to listen to the advice of the horseleach. The physician would frequently be mystified by the varying response to treatment his animal showed in the case of remedies the effect of which on man he knew well, but it would never have paid him to go very deeply into the matter. He sold his horse and got a fresh one. And so, as Hunter said, "Veterinary Science was abandoned," much as surgery had once been abandoned, to anybody who cared to spend his time unprofitably upon it.

The human subject clamoured for treatment, and the demand created the supply. "Skin for skin," said Satan to Jehovah, "Yea all that a man hath will he give for his life," and so the study of human medicine was profitable. But the dumb animal only got attention when the pocket of the owner was threatened, for it was not until last century that kindness to animals and care for their well-being became a duty generally recognized. After the good beginning made by Frederick II in the thirteenth century, it is somewhat shameful that little more was done in Europe towards encouraging veterinary science till the end of the eighteenth century, and that until the beginning of the twentieth century no veterinary school received any financial assistance either from the public or from Government, apart from the one public effort of 1791.

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[1] Since those days there have been James Syme, Alex. Crum-Brown, W. H. Flower, William Turner, J. Burdon Sanderson, J. G. McKendrick, W. D. Halliburton, Sims Woodhead, and Noël Paton.

Section for the Study of Disease in Children.

[*January* 25, 1929.]

A Mediastinal Teratoma in a Young Infant.

By F. J. Poynton, M.D. (President), and Alan Moncrieff, M.D.

Abstract.—An infant of four months with clinical evidence of a cystic tumour in the upper and anterior mediastinum died at the age of nine months. At autopsy the tumour was found to be a teratoma. The occurrence of such tumours in children is briefly discussed with a full clinical and pathological description of the present case.

Of recent years there has been a renewal of interest in intrathoracic neoplasms, and new methods of diagnosis, such as radiology, have opened up possibilities of removal of these tumours at an early and curable stage. Intrathoracic tumours are not common in children, but when they do occur, early diagnosis is essential if anything is to be done to remove them. The following case, in which surgical treatment was unfortunately not possible, is described in some detail since it is only by a knowledge of such cases that the necessary early diagnosis will be effected.

Clinical Notes.—S. S., male, aged 4 months, was admitted on February 9, 1928, under the care of Mr. Eric Lloyd at the Hospital for Sick Children, Great Ormond Street, for "noisy, difficult breathing since 1 month of age," associated with attacks of cyanosis and gasping for breath. During the fourteen days before admission these symptoms had been aggravated by "a cold." The parents were healthy and there was one other healthy child in the family. Patient had been fed with a cow's milk mixture since birth; weight on admission 10½ lb. The case was transferred under the care of Dr. Poynton. On examination the child was seen to be well-nourished, but cyanosed, with a stridor present both on inspiration and expiration. The upper part of the sternum moved very little and appeared to project forward. The lower intercostal spaces receded during inspiration. There was absolute dullness to percussion all over the front of the chest, except for an area below a line joining the axilla on each side to the xiphisternum. The breath sounds over this dull area were absent and there were no adventitious sounds. Air entry was good in the axilla and all over the lungs behind. Posteriorly the percussion note was resonant all over the chest. The heart sounds were audible faintly at the xiphisternal notch. An X-ray examination showed a large shadow filling the upper and anterior parts of the chest (fig. 1). On gentle tapping over the dull area a fluid thrill was obtained. A needle was inserted into the tumour through the second right interspace one inch from the sternum. A few c.c. of fluid were obtained, slightly blood-stained. This fluid contained hydatid hooklets or scolices, no organisms and only a few lymphocytes. It was sterile and injection into a guinea-pig gave negative results. The liver was felt two fingers' breadths below the costal margin: the spleen was not felt. There were no enlarged veins over the chest and no enlarged glands in neck or axillæ. Later, 13 c.c. of clear fluid (appearance as before) were withdrawn from the cyst and considerable reduction in size was effected, with improvement in the child's condition. Further tapping in a different situation enabled more fluid to be removed, with further improvement in the child's condition. The heart sounds could now be heard over a wide area. The fluid appeared to reaccumulate again quickly and the multilocular nature of the cyst was evident by the different sites at which a needle had to be inserted in order to remove fluid and by the failure of lipiodol injections into the cyst to show any outline. On one occasion 100 c.c. of clear, straw-coloured fluid were removed from the second interspace on the left side, with temporary improvement. Asphyxial attacks now began to be a troublesome feature, associated with signs of bronchitis at both bases behind. At one time pneumonic consolidation was thought to be present, but the child rallied remarkably, only to succumb to a terminal pneumonia, death taking place five months after admission, at the age of 9 months. Undoubtedly repeated aspiration of fluid from the cyst in the chest had played a large part in keeping the child alive so long.

Blood-counts on several occasions showed no abnormalities; the Mantoux reaction was negative and the Wassermann reaction was also negative. A slight but steady gain in weight occurred during the duration of the illness.

At autopsy a large tumour was found occupying the upper and anterior part of the thoracic cavity (fig. 2). The following description has been kindly supplied by Mr. C. F. Beadles from the Museum of the Royal College of Surgeons where the specimen is exhibited.

"The thoracic viscera of a male infant, aged 9 months, with a teratoma occupying the superior mediastinum. The tumour is ovoidal in shape with its long axis placed transversely; it measures $3\frac{3}{4}$ in., and 2 in. in its chief diameters. Its central part is marked by a vertical groove, which is deeper on the posterior surface, and contains the aorta and the origins of the great vessels, and the trachea, all of which are markedly compressed. The upper lobes of the lungs are pressed backwards by the growth, and airless; the lower lobes exhibit areas

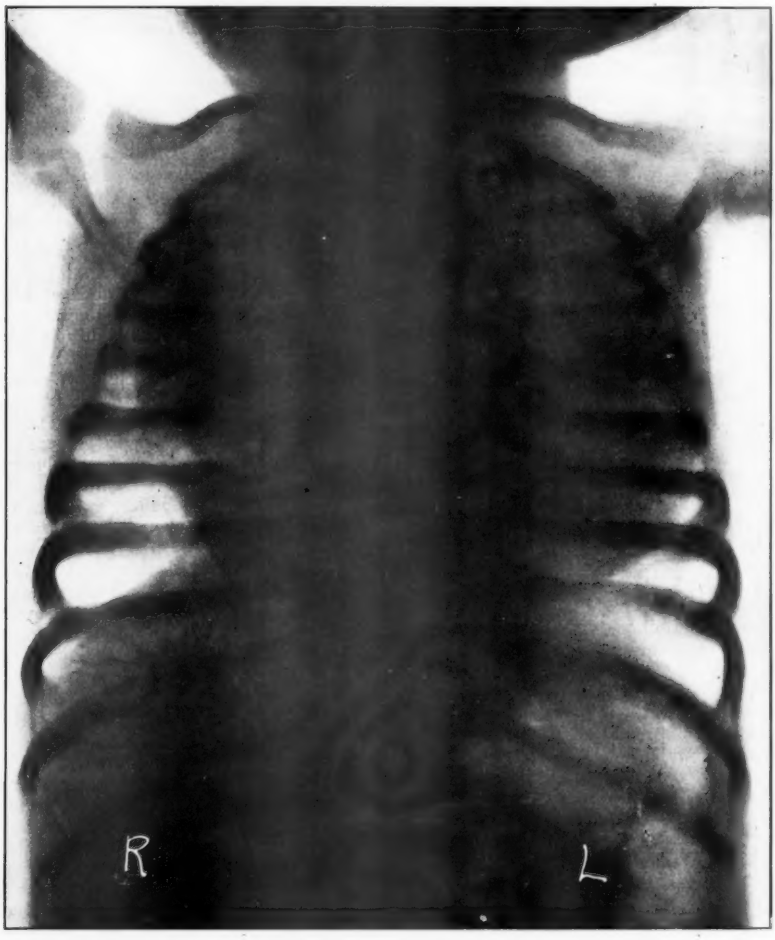

FIG. 1.—Radiogram showing large rounded shadow in upper part of chest.

of consolidation and emphysema. The auricles are much compressed by the lower surface of the tumour. The growth is surrounded with a thin layer of the mediastinal areolar tissue and is not adherent to the adjacent viscera.

"A section carried through the tumour in its longer axis shows that it is composed in part of solid tissue but to a large extent of cystic spaces and cavities. Much of the solid part is of a homogeneous, putty-like character of a yellowish tint, resembling brain tissue. The cysts and irregular clefts vary in size from a millet seed to an inch in length; these have

a smooth lining. One larger cavity, closely situated beneath the capsule on the right-hand side, is devoid of a lining, and contains a small amount of white opaque material; this has apparently resulted from liquefaction of the brain-like material, and may have resulted from the aspiration as recorded in the history. There appears to be no cartilage or bone.

"Microscopic examination of various parts of the solid tissue shows this to be composed of neuroglia with scattered large nerve-cells in some places. The cysts are mostly lined by a single layer of long columnar cells, for the most part stratified epithelium several layers in thickness, and these seem to merge into the columnar type of cell. The larger cavity above referred to is devoid of an epithelial lining: the more or less detached tissue within is necrosed neuroglia. In one section of the solid growth there is a fair-sized area of racemose glands. Plain muscle-fibre tissue occurs in fair abundance in the neighbourhood of the cysts and many blood-vessels and vascular spaces. No fat and no cartilage were observed; and there was no hair or other appendage of the skin. The capsule of the tumour is formed of fibrous tissue."

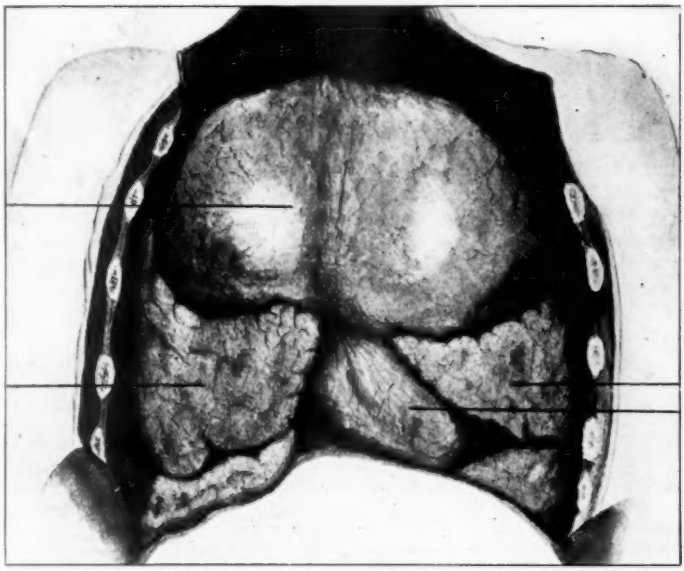

FIG. 2.—Photograph of findings at autopsy (from a painting by Dr. R. A. MacDonald).

Comments.—Teratomata, according to Ewing [1], contain a "*pot pourri* of fœtal tissues." Ewing speaks of mediastinal teratomata as occupying the anterior mediastinum and occurring, usually in young adults, as single or multilocular-cystic growths. He describes two types according to structure: simple dermoids and teratoid tumours. The former are cystic, the latter tend to be more solid. In Abt's "Pediatrics," C. G. Grulee [2] describes "dermoid cysts" beginning in the upper part of the anterior mediastinum and varying in size from that of a walnut to that of a child's head. The contents vary greatly and some contain hair and skin elements. In Osler and McCrae's "System of Modern Medicine," H. A. Christian [3] groups dermoid cysts and teratomata of the thorax together. He says that they may occur in all degrees and variations from a simple cyst, lined by epidermis and its appendages (hair follicles, sebaceous glands, or sweat-ducts), to solid tumours, made up of a variety of tissues in complex arrangement. The upper part of the anterior mediastinum appears to be the common situation for these

tumours, which usually first give rise to symptoms in young adult life. They are benign in the sense that they do not produce metastases and grow slowly, not destroying surrounding structures. Sarcomatous changes may, however, occur in these tumours. Christian gives a summary of the literature on the subject up to 1924 and quotes a total collection of eighty-eight cases.

No attempt has been made by us to obtain a complete list of recorded cases up to the present time. These teratomatous tumours are described under a multiplicity of names, and prolonged search is necessary in most cases to discover whether a paper is dealing with intrathoracic teratomata in children or not. In America R. S. Morris [4] collected sixty-seven cases of "dermoid cysts" of the thorax up to 1905, and of these four were under the age of 10, but exact details as to site and nature of the tumours described are not given, although one is described as occurring "in a suckling." H. Batty Shaw and Gwynne Williams [5] gave an account of a case of intrathoracic dermoid cyst in a woman of 26, who had coughed up hair. In their discussion they quote thirty-five cases of intrathoracic dermoid cyst, where the age of onset of symptoms varied from 20 to 60 years. They consider these tumours of the upper part of the anterior mediastinum may be (*a*) teratomata, including a variety of tissues, (*b*) congenital cysts derived from branchial clefts, and (*c*) combinations of dermoid cysts combined with malignant teratomata.

It will be seen that the case here described well fulfils Ewing's description of a "*pot pourri*." The tissues present include neuroglia, nerve cells, stratified and columnar epithelium, glandular tissue and muscle fibre. It seems to be agreed that such tumours do not usually give rise to symptoms until young adult life, but since they are essentially congenital in origin there is no reason why they should not occur from time to time in children.

Successful removal of the tumour in an infant of this age and in such close relation to the great blood-vessels was not possible.

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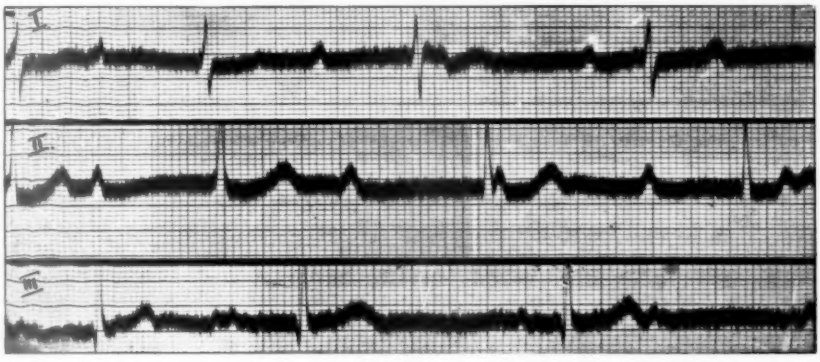
Dr. F. J. Poynton (President) said the case was disappointing clinically for, as the tumour had been slowly suffocating the child, there had seemed to be some hope that a cure might be effected by its removal. An interesting point was the remarkable fluid thrill which was obtained; this must be very unusual in this kind of tumour. The thrill was from side to side. A further remarkable point was the X-ray appearance; the opaque area looked like a pericardial effusion the wrong side up. If the tumour could have been separated from the big vessels he believed the patient would have recovered, as the infant was well grown and strong.

Rheumatic Carditis with Complete Heart Block.—Wilfrid Sheldon, M.D.—Patient, a girl, aged 5 years. Past illnesses: Chickenpox. The child had good health until October, 1928, but since then she has experienced vague pains in the arms and legs. There is no further history of rheumatism. She first came under observation in November, 1928, and since then her condition has remained unaltered.

Present condition: Fairly well-nourished, weight 38 lb. There is no evidence of congenital morbus cordis, no cyanosis, and no finger clubbing. The pulse-rate is 44 per minute, and the beats are regular. The heart is slightly enlarged and there are systolic and mid-diastolic murmurs at the apex. No aortic lesion. Radiological examination of the chest shows the cardiac shadow slightly enlarged. An electrocardiogram, kindly taken for me by Dr. Jenner Hoskin, shows a complete auriculo-ventricular block, the auricles beating at 84 per minute, and the ventricles at 44 per minute. The electrocardiogram also shows right-sided preponderance, and the S-T interval is increased from the normal 0·28 seconds to 0·36 seconds.

Complete heart-block is a most unusual complication of heart disease in childhood. In this case I believe the block is rheumatic in origin, partly because there is no evidence to favour a diagnosis of congenital morbus cordis—which is the alternative cause of heart-block at this age, and also because the murmurs at the apex, together



with the slight enlargement of the heart, are so commonly found in children with rheumatic heart disease.

The ventricular rate of 44 is quicker than is usual with a complete auriculo-ventricular block, but this may be accounted for by the age of the subject, since presumably a child's ventricle beats faster than the ventricle of an adult.

Discussion.—Dr. HOSKIN said the electrocardiogram which he had taken at the request of Dr. Sheldon showed a complete auriculo-ventricular block, also some prolongation of the ST-interval from 0·28 second to 0·36 second. He did not agree with the view that the case was rheumatic. The child had a rheumatic heart, as shown by the murmurs in the region of the apex, but there was, he thought, another murmur over the pulmonary area, or rather in the third space, also precordial bulging was more marked than would be expected in a child aged 5 with rheumatic signs of only this severity. He regarded the condition as congenital, there being a defect in the upper membranous part of the ventricular septum.

Dr. G. M. SLOT said he agreed with Dr. Hoskin's view. Probably this child had had rheumatic fever at some date, with rheumatic carditis. With regard to heart-block, it struck those who saw rheumatic cases how rare it was to meet with heart-block, even in the most extensive rheumatic conditions, though the fever involved the whole system. In this case the murmurs suggested a congenital heart lesion, probably septal, and the heart condition was probably part of the congenital syndrome.

Dr. F. J. POYNTON (President) said he thought it was unusual for heart-block to last so long in rheumatic children. He was impressed by the fact that heart-block was generally transient in acute rheumatism. He had had a case in which heart-block was the first symptom, with sore throat. No one knew what was the cause until about a week afterwards, when a valvular murmur developed, followed later by chorea, pericarditis and endocarditis. The heart-block lasted only about a week.

Some American workers dealing with the subject of the aorta (Kuhl and Epstein) had been finding changes in the pulmonary artery and right ventricle such as Pappenheimer and von Glahn found as a result of rheumatism. If this finding proved correct, it raised the difficult question as to whether some of the murmurs now regarded as congenital were not possibly rheumatic, affecting the pulmonary valve and pulmonary artery, a part of the heart which, it had been thought, was little affected in rheumatism.

Dr. BERNARD SCHLESINGER reminded Members of a case he had shown to the Section a year ago, one of congenital morbus cordis with rheumatism. In the case of that child there

was found to be a partial heart-block—a right bundle branch block. Since then electrocardiograms taken periodically had shown no alteration in the condition. He regarded the present case as a similar one, namely, that there was originally a congenital lesion with a rheumatic affection superimposed.

Familial Amyotonia Congenita.—WILFRID SHELDON, M.D.—Patient, a boy, aged 3½ years, was noticed at 6 months of age not to kick his legs about. The mother recognized the condition as being similar to that of two of her older children, and, in view of their fate, she did not seek advice for this child until he was 3 years old. Past illnesses: Measles, pertussis, chicken-pox and frequently bronchitis.

For the diagnosis of amyotonia congenita I am indebted to Dr. Kinnier Wilson, who kindly saw the case with me.

Present condition: Wasted, unable to sit up, to hold up his head, or to move his legs. The movements of the arms are slow and so weak that, when feeding, he requires the combined power of both arms to raise a fork to his mouth. There is considerable wasting of the muscles of the legs, trunk and arms. The tendon reflexes are absent; abdominal reflexes present; plantar responses flexor. There is well marked scoliosis, the chest is very flattened, and, in addition, the chest wall is curiously springy and resilient. The cranial nerves are normal, and the intelligence is satisfactory.

Family History.—Patient is the fifth child.

The first child, a girl, suffered from the same condition as the patient I am showing, and attended Dr. Pearson for several years at Great Ormond Street, where the diagnosis of amyotonia congenita was made. Eventually the child was admitted to one of Dr. Barnardo's Homes, where she died when 5 years old.

The second child was noticed when 6 months old not to be using her arms or legs, although for a few weeks after birth she had kicked her legs about. Seen when 7 months old she was a fat baby, weighing 16 lb., but was unable to sit up or hold up her head. The muscles were all small and atonic. Except for slight movements of her hands and feet, her limbs were immobile and flaccid. The reflexes were absent. The diaphragm and intercostals were noted to be weak. When 12 months old this child developed diphtheria and died in Croydon Fever Hospital. Her death was considered to be due to her generally weak state, rather than to the diphtheritic infection. At the autopsy the muscles were all noted to be small, but microscopic examination of them, or of the nervous system, was not carried out.

The third child, a boy aged five years, is perfectly healthy, and the fourth child, a girl, is also in good health.

The familial occurrence of amyotonia congenita in this family is established by the appearance of the disease in the first and fifth children. In the case of the second child, no definite diagnosis was made, although the mother is sure that the child had the same condition as her first and fifth children. From the available notes about the second child, it seems likely that the condition may have been a paralysis of the Werdnig-Hoffmann type, in which case the occurrence of this disease and also of amyotonia congenita in one and the same family lends some support to the view that these diseases are really identical.

Dr. W. G. WYLLIE remarked on the fewness of familial cases of amyotonia congenita; not more than about twenty appeared to have been recorded. He was much interested in what Dr. Sheldon said as to the possible relationship of Oppenheim's and Werdnig-Hoffmann's diseases. An instance of twins was on record in which one of the twins was clinically a case of amyotonia congenita, the other being one of Werdnig-Hoffmann's disease. The histology seemed to be the same in both. So, though the clinical picture might vary, it was likely that both were variations of the same disease.

Post-diphtheritic Hemiplegia.—C. WORSTER-DROUGHT, M.D., and I. M. ALLEN, M.D.—G.H., female, aged 9 years.

Complaint.—Inability to use the right arm and leg.

Duration.—Five years.

History of Present Illness.—At the age of four she had an attack of sore throat with feverishness. Result of examination of throat swab was positive for diphtheria. She was then a patient in a M.A.B. hospital for eighteen weeks.[1] On discharge the legs were very weak but there was apparently no difference between the two sides. After a few weeks, when the left leg had recovered, it was noticed that she limped with the right leg. The limb gradually got worse and the arm was affected later. The movements of the face were never obviously abnormal. There has never been any abnormality of speech. The condition of the arm and leg improved for a short time in hospital one year ago but has become worse since her discharge.

The child's education has been interrupted; during the past year she has attended a school for cripples and has made some progress, but is still in the infants' class. She is more emotional than the average child, and is regarded as "spoiled." Before the illness she was left-handed; and since its onset has not used the right hand excepting during the time she was in hospital.

Previous History.—The patient is the eldest child; was a seven months' baby; confinement normal; sat up at the usual time; cut her first tooth at eight months; talked first at twelve months; and walked at fifteen months. Up to age of four her development was normal. She had broncho-pneumonia when twelve months old, measles at three, diphtheria at four and chickenpox at eight years of age.

Family History.—Three other children; two healthy; one died at twelve months of broncho-pneumonia.

Condition on Examination.—The movements of the right side of face are weak, especially in the lower part. The tongue is protruded to the right. Other cranial nerves normal. Sensation normal.

Upper limbs: Left arm normal; right arm slightly abducted, flexed and semi-pronated at elbow, and flexed at wrist and finger-joints; motor power very limited, especially in fingers; muscle tone increased, especially in flexors. Deep reflexes exaggerated on right side. Slight shortening of right arm.

Lower limbs: Left leg normal; right leg held in a position of extension with inversion of foot: motor power poor, especially in flexion, and in eversion of foot; muscle tone increased both in the extensors and flexors, but more in the former. Knee-jerk much exaggerated, and ankle-jerk more active than on left side. Right plantar reflex extensor, left flexor. Slight shortening of the right leg. Abdominal reflexes less active on right side than on left. Gait: Dragging of right foot, swinging of right leg from hip, and rocking from side to side. Spontaneous movements: Spastic tremor of the right arm on effort, exaggerating usual position of the limb. Coördination: Normal on left side; impossible to test on right side. Sphincters normal. Many septic teeth. Other systems normal.

Cerebro-spinal fluid: 2 cells per c.mm.: Lange test, 2221100000; Wassermann reaction negative; other features normal.[1]

Mental state: Attention, perception, orientation and memory normal. Mental age, according to Stanford modification of Binet-Simon scale, is eight years.

Commentary.—Hemiplegia as a complication of diphtheria is a somewhat rare occurrence, of which J. D. Rolleston,[2] writing in 1925, states that barely 100 cases have been recorded since the first example was described by Sir William Gull in 1859. Rolleston,[3] who has recorded six cases, met with only eight examples in 11,313 cases of diphtheria admitted to hospital over a period of fifteen years.

The hemiplegia is vascular in origin and due either to embolism, thrombosis or hæmorrhage. Embolism appears to be the most frequent cause and was present in twelve of eighteen autopsies mentioned by Rolleston. Thrombosis is the probable

1 See "Subsequent Notes."

2 Rolleston, J. D., "Acute Infectious Diseases," London, 1925, p. 28.

3 Rolleston, J. D., *Clin. Journ.*, 1913, xlii, 12.

cause in the present case. In all the recorded cases, the diphtheritic infection was of severe type and the complication usually developed within the first six weeks.

In the case shown, the hemiplegia was not definitely noticed until a few weeks after the patient had left hospital. The fact that the patient was detained in hospital for eighteen weeks suggests a severe illness, and the general weakness resulting may have masked the hemiplegia until she began to get about. There is also some mental impairment present, which has occurred in most of the recorded cases.

Discussion.—Dr. WYLLIE said he wondered whether congenital syphilis could have caused the hemiplegia; perhaps that disease had been latent and had been brought out by the diphtheria. The central incisors appeared to be Hutchinsonian in type. The Lange test was of the parasyphilitic type.

Dr. J. D. ROLLESTON asked if further details could be obtained from the hospital as to the character of the attack of diphtheria, and especially as to whether the child had any cardiac involvement at the time. It would be of interest to know whether she had suffered from any other paralysis.

Dr. WORSTER-DROUGHT (in reply) said that he would endeavour to ascertain from the hospital concerned the severity or otherwise of the attack of diphtheria and add the information to the notes.

The possibility of congenital syphilis had been considered, especially in view of the somewhat atypical history. It was a question of balance of evidence. Personally, he did not attach much importance to a Lange reaction of only 2; if it reached 3 in two or more tubes of the series, it would be more significant. There was nothing in the family history to suggest syphilitic infection, although it would be well to examine the blood of the parents.

Subsequent Notes.—The patient was admitted to a M.A.B. Hospital on June 6, 1924, with a very severe attack of faucial diphtheria, which was confirmed bacteriologically. She received 84,000 units of antitoxin in three doses. On July 7 she developed ptosis and strabismus, and on July 12 paralysis of the palate; on July 19 paralysis of the pharynx supervened and the patient was nasally fed. A few days later there was paresis of the diaphragm and respiratory muscles with occasional vomiting. On July 30 the patient's condition began to improve, and on August 2 she was able to take food by the mouth. The improvement continued and she was discharged from hospital on September 13 apparently free from paralysis. We are indebted to Dr. J. Wilkins for this information.

The examination of the cerebro-spinal fluid and blood has been repeated. Cerebro-spinal fluid: 3 cells per c.mm.; all "small lymphocytes"; total protein = 0·03 per cent.; no excess of globulin; Lange test = 0110000000; Wassermann reaction negative. Blood Wassermann reaction negative. In both the father and mother of the patient the Wassermann reaction in the blood is negative (January 31, 1929).

Swelling of Left Thumb.—E. A. CROOK, M.Ch.—G. W., a girl, aged 12 years.

Twelve months ago she reported a painless swelling of the left thumb, also a painless fluctuating swelling in the right cheek. The appearance of the thumb and that shown by the radiogram pointed to tuberculous dactylitis. No organisms were found in the fluid from the face. The Wassermann reaction was negative. The thumb was treated by rest and Scott's dressing.

Five months ago no change was noted in the appearance of the thumb, but X-ray examination results suggested enchondroma rather than dactylitis.

One month ago the thumb appeared to be slightly less swollen, and the X-ray finding was then in favour of dactylitis.

Mr. Waugh saw the case, and suggested that it might be a syphilitic dactylitis, rather than a tuberculous condition, because of the hard nature of the swelling of the bone, and the fact that it was more palpable at the ends of the phalanges, but having examined the skiagrams he now rather favours the view that it is tuberculous.

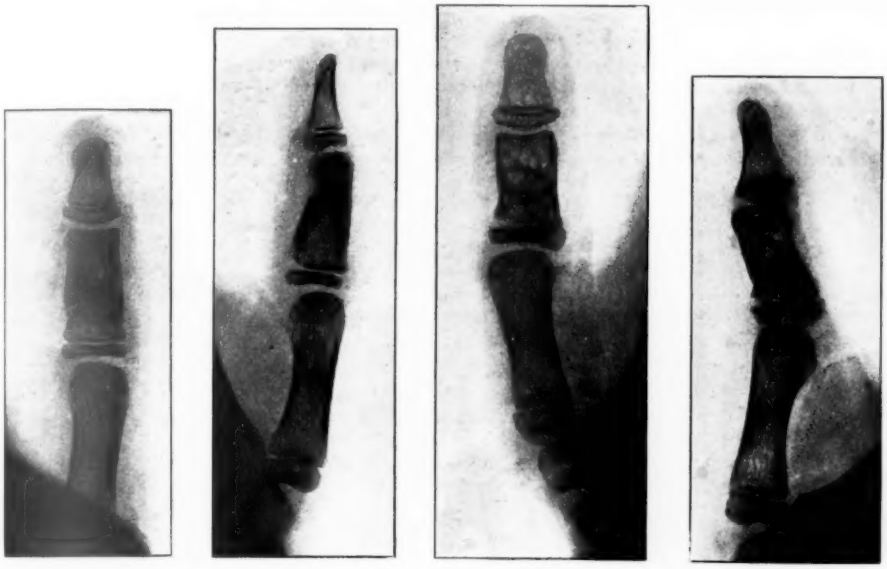


FIG. 1.—February, 1928. FIG. 2.—February, 1928. FIG. 3.—December, 1928. FIG. 4.—December, 1928.

Congenital Deformity of the Arm (Hemiectromelia), with total Absence of Hand and Rudimentary Radius and Ulna.—S. SEGAL, L.R.C.P.Lond., M.R.C.S.Eng.—Patient, a boy, aged 3 years 6 months, born of English parents.

The family history is good. There is no consanguinity in the parents and no history of deformity in any other member of the family. Both parents are highly intelligent. The mother relates a story of a fall during pregnancy, with scalding of the arm. The doctor in attendance during labour informs me that the confinement was in every way normal, no instruments or anæsthetic having been used.

Both lower extremities and the right upper extremity appear normal. The left upper arm is identical with the right.

The left forearm is represented by a fleshy cushion, which the child can flex to the fullest degree and hyperextend, and also rotate slightly. On the distal surface at the radial corner is a small conical projection surrounded by a groove. Internal to this is a deep crescentic groove, from which another small conical projection can be extruded on the sides being drawn apart. At the ulnar corner is a somewhat larger conical projection, which is deeply grooved across its summit. (Fig. 1, p. 50.)

Radiogram of right arm appears normal.

Radiogram of left arm shows a rudimentary radius, 2 in. long, with a well-marked tubercle at its mid-point.

The ulna is also 2 in. in length, with a rudimentary olecranon process. The lower ends of the radius and ulna are in contact and appear to articulate. The carpals and phalanges are entirely absent. (Fig. 2.)

The patient is an only child and is highly intelligent. He displays unusual dexterity with his left arm, and is able to hold a spoon in his left elbow, and to eat with a knife and fork.

The left foot shows a larger space between first and second toes than the right, these two toes being freely mobile and prehensile. Small objects (pencil, fork, etc.) can be easily grasped and manipulated between them.

The possibility of some maternal impression during the mother's pregnancy seems negligible, although the mother says she fell down and scalded her left arm while she was pregnant. There is, also, no indication of any syphilitic inheritance. I assume that no treatment is possible, but is the use of an artificial limb worth considering?

I have to acknowledge my indebtedness to Dr. Donald Paterson for his kind permission to record this case.

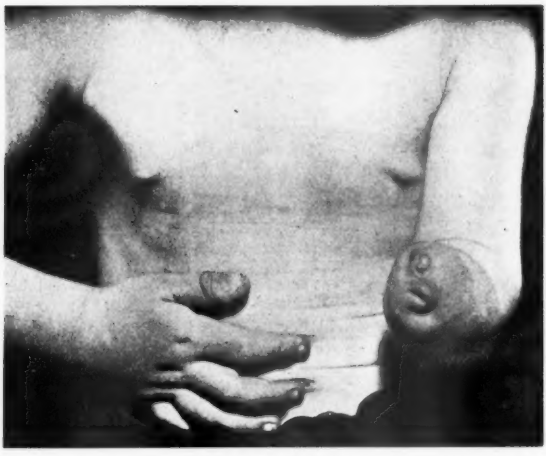


FIG 1.

FIG. 1.—Congenital deformity of arm.

FIG. 2.—Radiogram of left arm (antero-posterior view).

FIG. 2.

Dr. J. D. ROLLESTON said that the case recalled one he showed at the Section in 1915,[1] the photograph of which he now passed round. It was called ectrodactyly because only the hand was affected. Skiagrams showed that the bones of the forearm were not affected. The fingers were represented by five tubercles, the tubercle of the thumb being best developed. They were all movable at will. As in this case there was no history or evidence of syphilis, and this was the only child affected in the family. In these cases one always received a history of "maternal impressions," but he, personally, did not pay much attention to it.

[1] *Proc. Roy. Soc. Med.*, 1915, viii (Sect. Study Dis. Child.), 86.

Still's Disease.—F. C. ROLES, B.Chir., M.R.C.S. (for Dr. JAMES MAXWELL).—E. A., female, aged 12 years.

History.—December, 1927: Joints swollen and painful over a period of three days, those affected being the fingers of both hands and the right ankle-joint. No constitutional disturbance except anorexia. Pain gradually became less, but in March, 1928, difficulty in walking was still present. In hospital, from March to May she was apyrexial with a normal pulse-rate; the heart was not enlarged; there was swelling and limited movement of the right wrist and of several of the proximal interphalangeal joints of both hands. On re-admission in November, 1928, her physical signs were the same except that the proximal interphalangeal joints were now definitely fusiform, and the pain and limitation of movement had spread to the left wrist and to both ankles. The spleen and superficial lymph-glands were at no time palpably enlarged.

Investigations.—March, 1928: *Urine and fæces* showed no abnormal constituent. *Blood culture* sterile.—*X-rays*: Chest, nothing abnormal; joints, no bony change.—*Tooth-socket*: a hæmolytic Gram-positive diplococcus grown from this and a vaccine made.

December, 1928: *Vaginal swab* on two occasions inconclusive, occasional staphylococci and streptococci grown.—*Mantoux test* negative; *Dick test* negative. White blood-corpuscles, 7,200; no anæmia.

Treatment.—*Drugs*: Courses of aspirin, liq. arsenicalis, sodii salicyl., atophan (grs. iij).—*Non-specific*: T.A.B. 5,000,000 intravenously on November 26, followed in December by three further injections. Brisk reactions after first two injections, moderate later. No rigors.—*Specific*: Five bi-weekly injections of vaccine from tooth-socket. No reaction or apparent improvement.

Discussion.—Dr. ALAN MONCRIEFF remarked on the presence of an enlarged right epitrochlear gland which put the case more definitely into the category of Still's disease. Rheumatic nodules were present on the elbows, illustrating the indefinite border line between rheumatoid and rheumatic affections.

Dr. BERNARD SCHLESINGER said that this case did not strictly fall into the category of Still's disease. Still, in his original thesis, insisted that the lymphatic glands and spleen should be palpable; also, in every case he reported an adherent pericardium at autopsy. During life this was rarely proved to be present, although in some of the cases a persistent tachycardia suggested it. Recently he had seen such cases treated by protein therapy. The use of some other vaccine had occasionally been successful where T.A.B. intravenous injections had failed. To do any good the reactions needed to be rather drastic. In the present case, though the reactions were brisk, there had never been rigors. Perhaps the dose of T.A.B. which had been used was not large enough, and it might be well to try another type of organism.

Dr. F. J. POYNTON (President) said that he had a case of this kind which was associated with colitis, and the patient did well on lavage treatment at Buxton. This case was not quite one of Still's disease, and yet it was not quite rheumatism.

Dr. F. PARKES WEBER asked what was the ultimate result in such cases. Did the patients quite recover, or were they subject to rheumatoid arthritis in middle age? He had known a boy who developed a condition somewhat resembling a mild form of Still's disease in the small joints of the hands, but this had followed what might have been true subacute rheumatism (about 1890 and 1893). He afterwards developed into an active man, and enjoyed good health unless it were for a "rheumatic" tendency.

Dr. F. J. POYNTON (President) said he was unable to answer Dr. Parkes Weber's question. He was at Great Ormond Street Children's Hospital when Still first worked out his cases, but he had not yet been long enough in practice—not more than thirty years—for cases then seen to have come to him as adult cases. Some, however, were certainly crippled for life by the damage already done.

Amaurotic Family Idiocy.—LEOPOLD MANDEL, M.D.—V. G., female, Jewish, aged 12 months, first seen December 21, 1928. Mother complained that child seemed backward.

Family History.—Both parents Jewish; no consanguinity, and family history on both parents' side clear. Mother has two children, the elder one, aged six, is perfectly normal, with fundi clear; she has five stepchildren, all healthy; mother and father's Wassermann, negative; fundi of parents normal.

Previous History.—Full term child, normal birth. Breast-fed until admission.

Condition on Admission.—Facies typical of Tay-Sachs disease; well nourished child; hearing hyperacuity well marked; forehead prominent, good growth of hair; sclerotics bluish; pupils react to light; fundi right and left with identical appearances, typical optical atrophy with cherry-red spot in macula region. (Drawing shown on screen.)

Limbs: flaccid paralysis with plus knee-jerks. Wassermann negative. Cerebro-spinal fluid: pressure slightly increased, contents normal, Lange 00000000, Wassermann negative.

A feature of some interest is the risibility: the child laughs and gurgles from time to time, and this, together with its vivacity of head movement—due doubtless to its intense acuity of hearing—gives an appearance of contentedness and happiness. This is a tragically short phase, for as spasticity increases, the final picture of clonus and wasting intervenes.

January 3, 1929.—Limbs more spastic, no convulsions; is taking food well and maintaining weight.

Present Condition.—Legs slightly more spastic, abdomen protuberant. Eye condition *in statu quo.*

In these cases one is always asked by the parents what is the risk of the disease appearing in another baby; and I should like to hear the views of Members on this point. In the last number of the *British Medical Journal* Dr. Stenhouse Stewart [1] reports a case of amaurotic family idiocy in the R— family, originally described by Kingdon, and asks the same question.

In 1923 I showed before this Section a case of Tay-Sachs disease [2] in a non-Jewish child aged 20 months; its ultimate fate was increasing spasticity with recurrent convulsions, and death at the age of 24 months. This seems to be the usual mode of death in these cases.

REFERENCES.

[1] STEWART, D. D. STENHOUSE, *Brit. Med. Journ.*, 1929 (i), 101. [2] MANDEL, L., *Proceedings*, 1923, xvi (Sect. Study Dis. Child.), 55.

Supra-renal Tumour with Metastases and Optic Neuritis.—LEOPOLD MANDEL, M.D.—P. R., female, aged 6 years, first seen October 2, 1928, complaining of slight transient pains in limbs and abdomen, increasing pallor and apathy.

Family History.—Eldest of three, two children stillborn, no miscarriages, no history of tuberculosis in family.

Previous History.—Full term child, normal labour; fed on "Cow & Gate" milk till nine months old; has had chickenpox and measles.

History of Illness.—Has gradually become apathetic and very languid; no history of any pyrexial illness, eye symptoms, vomiting or giddiness.

On admission, October 9, 1928.—Very pale under-sized child, nourishment poor, showing little interest in surroundings. Temperature normal, pulse 104, respirations 20. Heart, lungs, urine, naso-pharynx, abdomen normal; no glands palpable. *Nervous System.*—Cranial nerves normal, pupils react, fundi clear, reflexes normal. *Blood-count.*—Red blood-cells 3,200,000; white blood-cells 8,550; hæmoglobin 55·8 per cent.; normoblasts positive, some poikilocytosis. *Differential*

count.—Polymorphonuclears 39 per cent.; small lymphocytes 52 per cent.; large lymphocytes 5 per cent.; large mononuclears 2 per cent.; eosinophils 2 per cent. Wassermann negative. Cerebro-spinal fluid normal. Von Pirquet reaction negative.

Anæmia treated with liver and later with iron and arsenic injections (Sérum Ferruginous (Fraisse) 0·75 c.c. intramuscularly on alternate days); patient was treated with ultra-violet radiation. The blood-picture a month later was practically unchanged (colour index 0·7).

December 6, 1928.—Examination of the fundi now showed; Right: Disc edges slightly blurred, some slight œdema, veins tortuous, no swelling or exudation. Left: Discs completely obliterated, summit of disc is about 2 diopters above general level of retina. Veins markedly tortuous and full. There is much exudation around disc and macula covering the vessels, the condition being a *true optic neuritis* on the left side.

A careful examination of the nervous system failed to reveal any abnormality; no vomiting or headache, slight pyrexia. There can now be felt above the left clavicle at lower end of sterno-cleido-mastoid muscle a small mass the size of a large pea (? gland), no enlarged glands elsewhere; spleen not palpable, but indefinite resistance in left hypochondrium.

December 9, 1928.—Complains of slight pain in limbs and neck; this was relieved by salicylates; a slight squint discernible. Pain entirely gone.

December 20, 1928.—Gland increasing in size.

January 2, 1929.—Patient seems much brighter. *Blood-count:* red blood-cells 3,380,000, white blood-cells 3,400, hæmoglobin, 50 per cent.; colour-index, 0·7; moderate poikilocytosis and anisocytosis. *Differential count:* polymorphonuclears, 39 per cent., small lymphocytes, 52 per cent., large lymphocytes, 5 per cent., large mononuclears, 2 per cent., eosinophils, 2 per cent.

Skiagram of thorax shows enlarged calcified mediastinal glands, shadows of calcified areas outside hilar zone, with extension of markings towards right apex: suggests presence of tuberculosis.

Present Condition.—General tone improved, apathy much less, slight squint still present but diminishing; gland on neck more easily palpable. The condition of the fundi as reported on by Mr. A. H. Levy on January 18, 1929, is as follows:—*Right:* disc edges much clearer. *Left:* disc edges still blurred and veins tortuous and full, but the swelling has disappeared. Proptosis, both eyes.

As regards diagnosis, transient optic neuritis in children not uncommonly accompanies illnesses due to septic foci; but coming on as they did together, the presence of a squint and the accompaniment of pains in the neck made me suspect either an incipient tuberculous meningitis or a tuberculoma, both of which suspicions proved unfounded. Nor can I find any extracranial cause for the optic neuritis. The blood-picture is, I suppose, that of a secondary anæmia; the change in the white cell count is of interest, namely, the fall from 57 per cent. to 21 per cent. and the rise of the polymorphonuclear leucocyte count from 39 per cent. to 76 per cent. I do not think the anæmia *per se* sufficient to account for the optic neuritis, and the blood picture excludes chloroma.

Note.—Subsequently, on February 7, under an anæsthetic a small portion of the supraclavicular gland was resected and microscopically examined. The pathologist reports: "indicative of a malignant lesion—a fibrosarcoma." Under the anæsthetic an indefinite mass was palpable in lower part of left hypochondrium.

The condition, then, is evidently a sarcoma of the left supra-renal body with metastases of the skull, etc.; this would also account for the optic neuritis. The result of an X-ray examination of the skull was indefinite, but early changes were found in the humerus and tibiæ.

The small mass above the left clavicle is increasing in size and I intend to ask Mr. E. C. Hughes to remove a small portion for microscopical examination.

Dr. ALAN MONCRIEFF said it was difficult to explain the optic neuritis, but there was a very hard gland over the left clavicle, corresponding to the Virchow gland in carcinoma of the stomach in adults; he thought there was something abnormal to be felt in the left hypochondrium. The occurrence of the optic neuritis might be due to the presence of some intracranial deposit, though that, again, would not explain why the optic condition was now subsiding. He suggested that some intra-abdominal neoplasm was present.

Abnormal Dentition.—JEAN SMITH, M.D.—A. B., male, aged 5 years, 9 months; 40 in. high and weighing 33 lb. (normal height and weight 42 in. and 44 lb. respectively). Patient is the first child and was born at full term. He was fed on breast milk supplemented by Nestlé's for ten months. The first teeth, which occupy the position of upper central incisors, were cut at 18 months, the four temporary 1st molars between 2 and 3 years, and the four permanent molars during the past three months. Began to hold things when 18 months old and to walk at 22 months of age.

Was in hospital for six days when 3 months old, discharged well on sixth day with a diagnosis of heatstroke, since, apart from high temperature (107·8° F.) on admission nothing abnormal could be found. He has had nine fits at intervals of a few weeks, during the past nine months; also attacks of severe epistaxis.

Mother states that child did not show any growth of hair for several (? nine months and that he never perspires.

Both testicles descended.

Family History.—An aunt of the patient (aged 23) is reported to have a double set of teeth.

Examination.—Slightly built, thin child; considerably under weight for age. Teeth: two canines occupying the position of upper central incisors, four temporary 1st molars, and four permanent 1st molars posterior to the deciduous ones. Hair fine, very fair and sparse. Eyebrows also thin. Intelligence normal, but a very sensitive child.—*Skiagrams* of epiphyses and sella turcica reveal no abnormality. Dental films show no unerupted teeth.—*Urine* normal: urea concentration normal.—*Blood-*

pressure 80/45 (normal 92·6/67·3).—*Heart:* No abnormality, apart from soft, apical systolic murmur.—*Blood-count:* Hæmoglobin and colour-index low, otherwise normal.—Sugar-tolerance, normal.—Fundus, fields of vision normal.—Blood-calcium: 15·5 mgm. per 100 c.c.

Discussion.—Dr. K. TALLERMAN said that the fits were associated causally with high blood-calcium content.

Dr. KINGSTON BARTON said that the facial aspect of the patient suggested to him progeria, in which condition one was apt to see associated irregularities of the teeth. Was the blood-pressure high because of the fits, and on account of the progeria accompanying renal infantilism? The early development of all those teeth which, at that age, should be normally absent would point to some alteration in the development and growth of the mouth and its neighbourhood at a very early stage of pregnancy. It seemed to him that this was a case of congenital renal disease.

Dr. JEAN SMITH replied that she was unable to explain the high blood-calcium content. At first, progeria was suggested as a diagnosis, but then the blood-pressure was found to be distinctly low, and there was no thickening of the arteries; neither had this child the dried-up, withered skin of subjects of progeria. The intelligence was up to, if not above, the normal standard.

Two Cases of Anæmia showing the Result of Liver Treatment.—BERNARD SCHLESINGER, M.R.C.P.—*Case I.*—W. C., aged 7 months, was first examined on June 29, 1928. She refused to take her food properly but there was no vomiting and the stools were normal. The diet had been repeatedly changed owing to indigestion, until Nestlé's milk was finally found to be the most suitable. This had been given for the last four months with Virol and orange juice in addition. On examination the child was found to be fat and flabby with a large fontanelle and a slight beading of the ribs. There was considerable pallor and the blood-count showed:—

June 29, 1928: Red blood-cells 1,752,000 per c.mm., white blood-cells 7,600, hæmoglobin 20 per cent. Polymorphonuclears 12 per cent., lymphocytes 84 per cent., large mononuclears 1 per cent., eosinophils 3 per cent. Marked poikilocytosis, anisocytosis and polychromasia. Some punctate basophilia.

Liver extract ½ drachm b.d. was given and the diet was gradually changed to citrated diluted cow's milk, groats, rusks, vegetable soup and small quantities of raw meat-juice and orange juice. Under this treatment the child's general condition improved and the blood-count rapidly approached normal.

August 22, 1928: Red blood-cells 4,812,000, white blood-cells 7,600, hæmoglobin 50 per cent.

October 5, 1928: Red blood-cells 5,264,000, white blood-cells 11,000, hæmoglobin 65 per cent., colour index 0·6. Polymorphonuclears 25 per cent., lymphocytes 70 per cent., large mononuclears 3 per cent., eosinophils 2 per cent.

Case II.—G. J., aged 6 years, was first seen on October 9, 1928. He had been noticed to be pale since birth, but had never ailed anything. Family history normal. Wassermann reaction negative. On examination he appeared to be extremely pale. The spleen was easily palpable, but no jaundice or hæmorrhages were observed.

October 9, 1928: Blood-count: red blood-cells 4,664,000, white blood-cells 7,700, hæmoglobin 30 per cent., colour index 0·3. Polymorphonuclears 54 per cent., lymphocytes 33 per cent., large mononuclears 5 per cent., eosinophils 5 per cent.

Liver diet and liver extract were both tried, but with little improvement resulting.

November 6, 1928: Red blood-cells 5,208,000, white blood-cells 14,000, hæmoglobin 40 per cent., colour index 0·4. Anisocytosis, poikilocytosis and some polychromasia. Two nucleated red-cells seen whilst counting 100 white blood-cells.

December 18, 1928: Red blood-cells 5,288,000, white blood-cells 8,100, hæmoglobin 40 per cent., colour index 0·40.

I have brought forward these two cases in order to demonstrate the inconstant effects of liver treatment in secondary anæmia. The wonderful

results obtained by this form of treatment in pernicious anæmia are now well known to all, but we have still to learn which cases of secondary anæmia will be likely to benefit from it. The patient in the first of my two cases, W. C., one of obvious rachitic anæmia, has reacted very well indeed to liver; but the second patient, G. J., an anæmic in whom the condition (associated with splenic enlargement, and not fitting definitely into a particular named group of anæmia) appeared soon after birth, has shown no improvement whatever. The good effect of liver on rachitic anæmia is not an isolated occurrence. Dr. Tallerman has shown a similar case when the Children's Section met recently at the London Hospital. I think one may expect a definite improvement in the blood condition in rickets under liver therapy, and that it should always be given when anæmia is found in association. It is always worth while trying the effect of liver in other unclassifiable anæmias, but in these it is impossible to predict the result.

Discussion.—Dr. JAMES MAXWELL said he presumed that in the rickety case Dr. Schlesinger gave the usual antirachitic treatment in addition to the liver, and so it was difficult to evaluate the latter in its effect on the anæmia.

Dr. PARKES WEBER asked whether actual liver was given, or liver extract. He did not know whether the extract was likely to contain an antirachitic vitamin, but he thought liver had as much of that as cod-liver oil. [Dr. SCHLESINGER: The extract was given.]

Dr. HELEN MACKAY said that, so far as she had seen or had been able to find out from others, the antirachitic treatment had no effect in curing anæmia associated with rickets. That held good whether one gave cod-liver oil or other antirachitic vitamin by the mouth, or whether light treatment was applied. In this case the diet had apparently been almost completely changed, therefore one could not attribute any improvement effected solely to the fact that the child received a small amount of liver extract. The meat, vegetables and fruit-juice would supply extra iron, and it seemed likely to bring about some improvement in such a case.

Dr. SCHLESINGER (in reply) said that in the case of the child with rickets there had been no abrupt change of diet and that it had been a matter of three months before the full diet stated in the notes was being given. It was a fair criticism that much of the improvement might have been due to dietetic factors, although no cod-liver oil or other form of extra vitamin had been given. His own view was that the benefit here was more marked and more rapid than that following simple antirachitic therapy; and in these cases he considered the liver had some effect. He did not know how much vitamin liver extract contained. It would be interesting to try its effect on rickets in place of cod-liver oil.

Retinitis Pigmentosa Albescens.—BERNARD SCHLESINGER, M.R.C.P.—M. L., aged 9 years, was first seen on September 4, 1928, complaining of gradual loss of sight for three months.

There were no previous illnesses and there was no history of any similar complaint in any other members of the family. Two other children are alive and healthy.

On Examination.—Eyes: Pupils reacted very feebly to light and accommodation. Double primary optic atrophy, and fine retinitis was observed. Perception to light only. Nothing else abnormal was detected in the central nervous system. Wassermann reaction negative on three occasions. Cerebro-spinal fluid normal. Sugar-tolerance normal. Blindness is now complete. X-ray examination of the skull, normal.

The condition from which this child is believed to be suffering, retinitis pigmentosa albescens, can be classed as "first cousin" to the more common and better known retinal disease, retinitis pigmentosa. "Retinitis pigmentosa albescens" was originally described by Nettleship, who pointed out that the pathological changes here did not include the usual spidery deposit of black pigment, resembling bone corpuscles, which was originally responsible for the name, retinitis pigmentosa. In its place, fine, glittering, white patches were seen, some as mere specks and others as large as a pin's head. Ophthalmoscopic examination in this case showed this and all the other characteristic changes: a pale atrophied disc, marked diminution of the branches of the retinal vessels, and a yellow, waxy looking fundus, through which

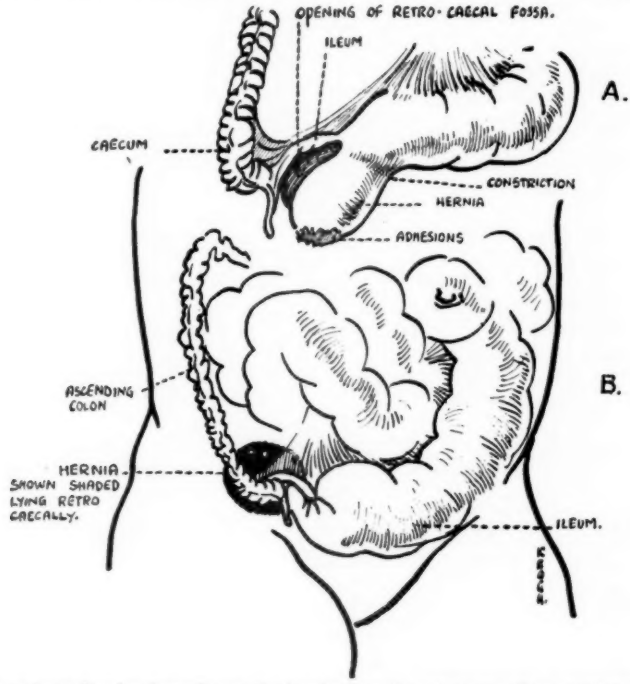
the choroidal vessels were unduly prominent, owing to the general atrophy of the retina. [Pictures were shown illustrating the various types of retinitis pigmentosa.] There is no history of the malady in other members of this child's family, but that would by no means influence the diagnosis, as in Nettleship's original figures the hereditary incidence was only 23 per cent. A curious feature of this case is the degree of blindness which supervened much more rapidly and much earlier than is usual. Unfortunately there is no available treatment.

Discussion.—Dr. PARKES WEBER asked whether this was likely to be one of those cases which developed progressive mental deterioration.

Dr. SCHLESINGER replied that cases had been recorded in which there was mental deficiency together with other abnormalities, but these did not occur in all of them.

Internal Hernia in an Infant aged Three Weeks.—R. C. LIGHTWOOD, M.D. (for Mr. T. TWISTINGTON HIGGINS, O.B.E., F.R.C.S.).—D. L., male infant, now aged seven weeks, birth induced three weeks before term. He was the second child of normal parents, the first having been still-born at full term.

History of Present Illness.—Jaundice appeared on the fourth day, and remained until he was admitted to hospital on December 30. He had been breast-fed. He



A, showing gut immediately after release; B, showing how the gut appeared on opening the abdomen. The hernia is shown shaded lying retro-cæcally, obstructing the lumen of the ileum.

appeared quite well until December 29, when he suddenly refused breast feeding. On December 30 vomiting of bile commenced, and he was admitted to the Hospital for Sick Children, under Dr. R. S. Frew. A yellow stool was passed that day; there had been no constipation previously.

After admission the following was recorded: Jaundiced baby, three weeks old, depressed fontanelle, frequent vomiting of bile-stained fluid. Liver normal, spleen

not felt, urine normal, umbilicus clean. Dr. Frew observed visible peristalsis of the "ladder pattern" type. Van den Bergh's test: Direct negative; indirect = 10 units of bilirubin. Wassermann: negative.

Diagnosis: (1) Icterus neonatorum. (2) Obstruction in small intestine.

Dr. Shires reported: "Bismuth meal arrested just above ileo-cæca valve." (Radiograms shown.)

At operation, Mr. Twistington Higgins found the small intestine distended and the large intestine collapsed. A knuckle of ileum 4 in. from the ileo-cæcal valve escaped from some unlocated area of obstruction in the lower abdomen. On the peritoneal surface of the loop was a slight abrasion as though a soft congenital adhesion had been separated at this point.

Subsequent Course.—The jaunhice disappeared and recovery was satisfactory. The infant is now of normal appearance and takes food well.

We are greatly indebted to Dr. C. A. Keogh for the preparation of the illustrations.

Pneumonia, Empyema, Nephritis and Pneumococcal Peritonitis, with Complete Recovery.—A. G. MAITLAND JONES, O.B.E., M.C., M.D. (shown by Dr. K. TALLERMAN, M.C.).—V. W., aged 5 years, admitted to the London Hospital in October, 1927, with a history of one week's swelling of glands of neck, and feverishness at night, three days' pain in right upper abdomen, and of one day's rapid, embarrassed respiration, with troublesome cough and some vomiting.

Previously the child had suffered from pneumonia when one year old, and had had an attack of influenza and tonsillitis a month ago.

On examination: The child was found to be pale and looking ill. There was no œdema and the heart and abdomen were normal. The left lung was clear, but there was impairment of the percussion note and diminished breath sounds at the left lower lobe. No crepitations were heard. The tonsils were ragged and small, and there was a naso-pharyngeal discharge. No cervical adenitis was present and the ears were normal. The urine contained albumin, granular and hyaline casts, together with blood cells, and was smoky in appearance, acid in reaction, with specific gravity 1020. Temperature 103. Pulse 146. Respiration 48.

On the fifth day after admission pain in the abdomen increased and patient was transferred to the surgical side, under care of Mr. Russell Howard; diagnosis, pneumococcal peritonitis. Suprapubic drainage was carried out. Fluid found in abdomen of a dirty yellow colour, with blood-stained coagulum, from which were cultured diplococci resembling pneumococci.

The child's state improved very much, and, although there were slight variations in temperature, both temperature and respiration had settled down in about ten days' time. The urine, however, continued to show albumin, blood and casts.

She was discharged at the end of November to a convalescent home, but during the following month the temperature was noted to be unsettled, the evening rise being more pronounced. The chest was needled over the right lower lobe and pus was obtained. She was re-admitted to hospital and rib resection was carried out, the patient making an uninterrupted recovery. Pleural fluid on culture was found to contain pneumococci and streptococci.

On December 18, 1928, a catheter specimen of urine contained only a trace of albumin, a few red cells and leucocytes, and was sterile on culture.

Re-examined January 18, 1929, the child was perfectly well in every respect and weighed 49 lb. There were no abnormal physical signs, the blood-pressure was normal and the right lung appeared completely expanded. A non-catheter specimen of urine was amphotrophic in reaction, specific gravity 1004; a trace of albumin was present, there were also a few leucocytes and epithelial cells, but no casts. Blood urea 0·019 per cent.

Section of Anæsthetics.

[*December* 7, 1928.]

Preliminary Medication in Anæsthetics.

By E. S. Rowbotham, L.R.C.P.Lond., M.R.C.S.Eng.

Preliminary medication in anæsthetics is a subject which has not received the consideration which we would expect when we consider the great advances made in the past few years in other branches of anæsthesia. It is a subject on which I have spent much time and thought. For many of my ideas I am indebted to Gwathmey, whose papers on synergistic anæsthesia I first read in 1921.

For the nervous and apprehensive patient, the usual preliminaries to an operation —the journey from bed to operating room, the atmosphere of the theatre, and the taking of the anæsthetic—are fraught with fear and aversion. To bring such a patient to operation without making some effort to blunt his sensibility seems crude and unnecessarily harmful. Even for the most stout-hearted it is an unpleasant proceeding. The customary $\frac{1}{6}$ gr. or $\frac{1}{4}$ gr. (·011 or ·016 grm.) of morphine is an uncertain and usually insufficient dose.

That it might be possible to "temper the wind to the shorn lamb" was suggested by the following well-known facts:—

(1) The ease with which most patients given rectal ether become unconscious.

(2) The more a patient has reacted to the preliminary medication, the less does he seem to be upset by the surgical procedure. The use of hyoscine is nearly always a distinct advantage.

(3) When a patient reacts well to preliminary medication, his nervous system is already partially shielded from harm; he therefore requires less anæsthetic, and hence is less upset.

(4) He passes more quickly out of the influence of the anæsthetic, but remains under that of the morphine and hyoscine. He consequently stays longer unconscious and suffers less early post-operative pain and discomfort. If he is sick he has little or no recollection of it.

I therefore set out upon a routine examination of some of the possible drugs which could be used for preliminiary medication, namely, potassium bromide, chloral hydrate, amylene hydrate, paraldehyde, chloretone, hedonal, morphine. omnopon, heroin, magnesium sulphate, chloroform in oil per rectum.

My chief object was to find a way of protecting the nervous system by producing in a patient such a profound sleep before administration of the anæsthetic, and whilst he was still in bed, that it would be impossible to arouse him no matter how much he was disturbed. This procedure would obliterate all nervous apprehension, and save him that mental distress which sometimes amounts to extreme fear and produces true shock. The patient would have no knowledge of the induction, he should pass easily into surgical anæsthesia, with no second stage, and wake up some hours after the operation with no taste of the anæsthetic in his mouth, no memory of having vomited, and with part of the most painful post-operative period passed and unremembered.

This I have found nearly always possible to secure by the use of paraldehyde, given per rectum, either alone or in combination with hypodermic injections of morphine, heroin or hyoscine. One function of the intestinal mucous membrane is to transmit many substances to the blood; one substance which it appears to transmit quickly and easily is paraldehyde.

Gwathmey found that as a preliminary to giving colonic ether both chloretone and paraldehyde diminish materially the amount of anæsthetic required. Speaking of paraldehyde, he further says, "When paraldehyde is used in connexion with ether it intensifies the action of the drug, and ensures more complete relaxation without

in any way affecting the pulse or respiration, and by employing it as a preliminary with this method (colonic ether) the usual amount of oil-ether solution may be reduced."

One important fact became very evident, viz., that combinations of all drugs acted very much more powerfully and for longer periods than did any one drug alone. "Drugs having the same physiological action summarize their therapeutical effect when given together, but drugs of *different* pharmacological action given together increase their activity *markedly more than the sum* of their action when given separately—they probably act on different cells."

This is Burgi's theory. It is called synergism by Gwathmey, who bases the production of his synergistic anæsthesia upon it.

Not only do the preliminary narcotic drugs reinforce each other, but the anæsthetic following them is also very considerably reinforced. It becomes possible in many cases to obtain complete abdominal relaxation by the use of gas and oxygen alone (without secondary saturation), or with ethylene.

After a very superficial cocainization of the larynx, one may pass a tracheal tube and give an intratracheal anæsthetic, using nothing but gas and oxygen. Ether becomes a very powerful drug, less than half the quantity usually needed to anæsthetize a patient being necessary. Chloroform is so reinforced in its action, that I do not think it safe to use it following complete morphine and paraldehyde preliminary narcosis. Depression of the blood-pressure and respiration is about the same as it is when the same doses of morphine or heroin are given without paraldehyde. It appears that the action of the drugs in this direction is not intensified. I have never found it a contra-indication or had trouble with patients after operation for this reason.

Classification and Dosage.—For determining the dose, patients may be divided into four groups.

Group I.—(*a*) Children under 7, and sick and undersized children over this age. (*b*) Very old and feeble subjects, and those with a very low blood-pressure. (*c*) Toxic and shocked cases, and those which have recently sustained a large hæmorrhage. (*d*) Patients with a high temperature, especially if it has lasted some days.

Group II.—Normal adults and children over 7.

Group III.—Very nervous patients. Alcoholics. Athletes over 25 years of age.

Group IV.—Cases of Graves' disease.

For Group I, I give one drachm (3·5 c.c.) of paraldehyde per stone (6·3 kilos) weight, administered per rectum one hour before operation, and with atropine gr. $\frac{1}{200}$ to $\frac{1}{100}$ (·00032 to ·00065 grm.) hypodermically about half-an-hour before operation. For these cases paraldehyde alone is sufficient to cause complete unconsciousness.

In Group II (the largest) I give gr. $\frac{1}{600}$ (·00108 grm.) of heroin, gr. $\frac{1}{300}$ (·0022 grm.) of omnopon, or gr. $\frac{1}{400}$ (·0016 grm.) of morphine, for every stone (6·3 kg.) weight, one and a quarter hours before operation, followed by one drachm of paraldehyde per stone weight given per rectum one hour beforehand.

Group III consists of cases which require still more narcotic to produce the desired effect; for these, two courses are open: (*a*) To treat them in the first place as Group II, and to follow this with a second hypodermic injection of one-quarter to one-half the original dose of heroin, morphine, or omnopon. This should be given about three-quarters of an hour before operation, i.e., half an hour after the first dose. Or, (*b*) one and a quarter hours before operation to give hyoscine hydrobromide (gr. $\frac{1}{200}$ to $\frac{1}{100}$) (·00032 to ·00065 grm.) in addition to the heroin or omnopon, and to follow this by rectal paraldehyde as before. Hyoscine quiets the cerebrum and produces sleep in most patients, but in a few it may cause wild delirium. It is therefore advisable, when practicable, to give a trial dose of hyoscine two or three days beforehand in order to ascertain the patient's reaction to it.

Group IV (cases of Graves' disease) are notoriously difficult to narcotize. They, however, vary considerably, and as they are practically always operated upon under local anæsthesia, it is very important to have them absolutely quiet and asleep. For them I have used a progressive dosage, stopping short when the desired result has been obtained.

A preliminary trial of the patient's reaction to hyoscine is made some days previously. (*a*) Potassium bromide and chloral (or bromidia) is given the night before, and repeated in the morning, if the operation is in the afternoon. (*b*) One and a half hours before operation heroin, omnopon, or morphine in combination with hyoscine (if reaction is satisfactory) are given hypodermically. A chlorotone suppository is inserted into the rectum. (*c*) One and a quarter hours before operation paraldehyde in olive oil is run into the rectum, one drachm (3·5 c.c.) for every stone (6·3 kg.) of the patient's weight, dissolved in half an ounce (14 c.c.) of oil per drachm (3·5 c.c.). The catheter is clipped and left in position. (*d*) If the patient is still awake three-quarters of an hour before operation, half to two ounces (14 to 56 c.c.) of a mixture of equal parts of ether and olive oil are run into the rectum. This is not always necessary, but when it is it should be given half an ounce (14 c.c.) at a time, at intervals of five minutes, until the patient is asleep. For the very obese I reduce the dose by about 20 per cent. This is not, of course, a hard-and-fast classification of dosage. Experience and judgment are necessary, and the personal factor cannot be eliminated. Many cases fall half-way between two groups, and for them the dose must be modified accordingly. Speaking generally, the action of these drugs is more profound in cases of shock, hæmorrhage, low blood-pressure, and in subjects of long-standing sepsis. In such cases the dose should be considerably reduced, as I have indicated by placing them in Group I.

Technique.—The following is the technique suitable for the majority of cases; modifications can easily be made as required.

Apparatus: Hypodermic syringe; rubber catheter, about size 12; glass funnel; rubber tubing; glass connexion for joining tubing to catheter; tubing clip; one pint bottle and cork; cotton wool.

Solution.—Paraldehyde in normal saline, one drachm (3·5 c.c) of paraldehyde for every stone (6·3 kg.) of the patient's weight, dissolved in ten drachms (35 c.c.) of saline for each drachm (3·5 c.c.) of paraldehyde (solubility one in ten of water). The solution should be at blood heat, but not hotter, or the paraldehyde volatilizes. In cases of Graves' disease the paraldehyde is dissolved in warm olive oil, half an ounce (14 c.c.) for each drachm (3·5 c.c.), as it may be necessary to follow with oil ether.

Preparation of the Colon.—The colon should be emptied by a soap enema the day before operation, unless the bowels have been well opened by aperients. The night before, or on the morning of the operation, *but not less than three hours before operation*, a saline wash-out is given, to ensure that the bowel is empty. The thorough cleansing of the colon recommended by authorities as a preliminary to colonic ether is not necessary; in fact it is harmful, as it exhausts the patient.

Administration.—One and a quarter hours before operation: (1) Give hypodermic injection of heroin, &c. (2) Chloretone suppository (gr. v to x) (0·3 to 0·6 grm.) should be inserted into rectum if thought necessary. (*Note.*—Chloretone is a local anæsthetic and is very useful in nervousness or irritability of the rectum. Gwathmey appears to use it for its narcotic properties as well, but I have not found its effect in that direction at all marked. It should not be used for a laparotomy; I am sure it tends to increase paralytic ileus.) (3) The patient is blindfolded and wool placed in the ears. (4) A lifter is placed under the patient or he is put on the trolley ready to be transferred to the theatre. (5) He is placed on the left side, hips on a pillow, or the foot of the bed is elevated on blocks.

One hour before operation.—(1) Pass the catheter, well lubricated, into the rectum with a twisting motion, and lightly pack round the anal margin with cotton wool. (2) Fill the funnel and tubing with water, expelling all air, and clip the distal end of the tubing. (3) Connect to the catheter, release the clip, and run in the solution slowly. This should take at least ten minutes. The paraldehyde solution should be vigorously shaken before refilling the funnel, to ensure solution. In cases of Group I only: if the patient goes to sleep during introduction of the solution, it is advisable to stop. Further doses of paraldehyde will be unnecessary for a time, and it can be repeated after operation if desired. (4) When all the solution has been run into the rectum, the catheter is removed, leaving the wool packing in place. A pad of wool is placed on the perineum, and the patient made to lie on his back, with his legs together. This aids retention of the solution. Pressure over the perineum by the nurse is a great help if difficulty is experienced in retaining the solution; the patient rapidly becomes drowsy and tolerates the injection quite well in about five minutes. Skill and tact on the part of the nurse who administers the rectal injection are essential—especially with children. One nurse who frequently prepares children for me has never had a failure, that is, she has always persuaded her patients to retain the injection; whereas with others not gifted with the art of gaining a child's confidence, failure has been fairly frequent.

Duration of Narcosis.—If undisturbed the patient will usually go to sleep in about fifteen minutes, but often in much less time. This sleep gradually deepens, until, in from twenty to forty-five minutes, he is so deeply narcotized that it is impossible to arouse him. The conjunctival reflex is frequently lost, but the corneal remains. The muscles are flaccid, but he is not insensitive to pain, for if he be hurt, he will move, and will remain rigid for some minutes. I have on several occasions failed to give a spinal anæsthetic because of this, and it has been very disappointing, for such a combination is ideal. I now, very cautiously, infiltrate the skin with novocain before attempting the lumbar puncture.

The length of time the patient remains unconscious after operation varies considerably with the particular diathesis of the patient, the anæsthetic, and its duration. An average case, after one hour's administration of gas and oxygen, will remain asleep without moving for twelve hours; will move, and talk very drowsily for another six to eight hours, and will not become fully conscious of his surroundings until about twenty-four hours after the paraldehyde has been given.

On return to bed three courses are open, any one of which should be taken as deemed necessary: (1) The patient can be left to absorb what paraldehyde remains in his rectum and "sleep it off." (2) A rectal tube may be inserted, and the remaining solution run off. This tube should be twice as large as the catheter used for introduction. The colon should be gently squeezed and massaged, and, if a more rapid return to consciousness is desired, the bowel may be washed out with saline. (3) Paraldehyde solution may be repeated when the patient shows signs of return to consciousness; 25 per cent. of the original dose is sufficient. I have kept a patient asleep for two and a half days after operation by means of repeated doses of paraldehyde. She awakened with nothing worse than a headache.

It is a common occurrence for a patient, who has not been told the purpose of the rectal injection, to awaken some hours afterwards asking how long it is before the operation is to take place. I occasionally use paraldehyde to keep the patient asleep after operation, where for some reason I have not been able to get it given beforehand. Only ½ drachm (1·8 c.c.) per stone (6·3·kg.) is necessary, the narcotic (as apart from the analgesic) properties of the anæsthetic are intensified by the paraldehyde, and such patients are seldom sick.

The method may also be used with a local anæsthetic. I found it possible for a laparotomy to be performed in this way without using a general anæsthetic.

After-effects.—The after-effects are slight. Sometimes a patient will complain of

headache, or is confused for some hours, but more often his sleep gradually becomes less and less deep, and his conscious periods longer, until he finally awakes feeling very little worse for the anæsthetic. There is seldom nausea or sickness, or any of the unpleasant sequelæ of a general anæsthetic. Toxic cases remain unconscious longer, and require smaller doses of heroin or omnopon; this is well illustrated by the case of a lady with extensive and long-standing infection of the nasal sinuses, whom I wrongly regarded as belonging to Group III, whereas she should have been placed in Group II or even Group I. This lady remained asleep for two whole days. On the evening of the operation, her breathing became deep, automatic, and about 40 per minute, as if she were under gas or ether. I never understood the reason for this (unless it was due to the hyoscine—she had gr. $\frac{1}{1\,5\,0}$ [0·00043 grm.] before operation); however she recovered, and was none the worse for it.

Contra-indications.—The method is contra-indicated in diseased conditions of the bowel, rectal fissures and hæmorrhoids, and in operations on the lower bowel. When a spinal anæsthetic is used the sphincter relaxes, and the paraldehyde may be returned. When the operation necessitates trauma, or much handling of the abdominal contents, a chloretone suppository is inadvisable as it tends to increase paralytic distension; but I do not think that paraldehyde alone is contra-indicated for abdominal operations.

I have endeavoured to tackle the problem of minimizing the ill-effects of anæsthetics and pre-operative fear. That there is ample room for improvement is manifest. From the patient's point of view it is more often the anæsthetic than the operation which is feared. If we can abolish apprehension immediately beforehand, and vomiting afterwards, we have accomplished something which is well worth while; and if we can carry through the administration successfully (from the surgeon's point of view) using much less anæsthetic, or a less toxic anæsthetic, we have gone a long way towards the solution of the problem.

This account is put before you not by any means as a perfected method or technique, but to show where I at present stand in the matter.

Paraldehyde is a safe drug (and safety is above all the most important thing), and when it is used to reinforce other drugs, such as heroin—their effects, though increased, leave the heart and respiration unaffected. So far it is the only combination I know of which will do this, although undoubtedly other more satisfactory drugs will be produced, e.g., avertin.

Discussion.—Mr. I. W. MAGILL said that his experience with paraldehyde given per rectum in the manner described by Mr. Rowbotham had been particularly gratifying. He (Mr. Magill) had had several communications from patients expressing enthusiasm over anæsthetics preceded by preliminary medication with paraldehyde. The method was of great value in selected cases, particularly in nervous patients. He thought that if the paraldehyde was mixed with an equal quantity of olive oil and warmed, it was unnecessary to insert a chloretone suppository before injection. He had not taken instrumental readings, but thought that blood-pressure was lowered.

The use of paraldehyde was, in his opinion, definitely contra-indicated in chest conditions associated with much sputum. He referred particularly to the operation of thoracoplasty for bronchiectasis. It was essential in such cases for the patient actively to endeavour to expel accumulations of sputum as soon as possible after the operation.

He (Mr. Magill) cited four cases in which there had been some departure from the usual condition of patients after administration:—

(1) Adult woman suffering from non-toxic goitre. Profound nausea and retching before operation.

(2) Adult woman. Correction of nasal deformity. Nausea before operation as in the first case.

(3) Child, aged four years. Dissection of tonsils. Usual course during operation. Patient slept for twelve hours. Thirty-six hours after operation severe and almost continuous vomiting set in and lasted for about seven hours. No acetone could be detected. The condition responded to small doses of calomel.

(4) Adult male. Extraction of impacted wisdom tooth. The patient weighed eleven stone and was in normal health. Seven drams of paraldehyde, without morphia or hyoscine, were sufficient to produce the desired pre-operative and post-operative paraldehyde effect. This would show that body weight and apparent condition were not absolute guides to dosage in every case.

Mr. FRANKIS T. EVANS asked whether Mr. Rowbotham had found some patients very restless after rectal paraldehyde. He (the speaker) had experienced extreme restlessness in one case, a small boy of 7 years.

Mr. RAMSEY PHILLIPS cited the case of a male athlete, aged 25, weighing about 11 st., and in perfect health. Previous to operation (removal of tonsils) 1 oz. of paraldehyde was given without other preliminary medication. Anæsthesia was then induced by a mixture of one drachm of chloroform with eight drachms of ether. This produced a very low arterial pressure, with slowing of the pulse to 35 per minute, dilatation of pupils and a brisk corneal reflex. This condition was rapidly and permanently corrected by a few breaths of carbon dioxide. The airway was free and oral throughout, and respiration free and sufficient, but slightly reduced in depth and rate.

Dr. MENNELL expressed his preference for giving an anæsthetic by the mouth. He had a great personal dislike to the smell of paraldehyde, and this drug could certainly not be considered absolutely safe: two deaths had been mentioned in this discussion [not reported] and he knew of two others which had occurred some years ago when this drug had been used intravenously. Some patients were more susceptible than others, and when a dose had been given it was difficult to recover any of it. The prolonged sleep negatived its use in certain cases, such as operations about the air passages in which concealed hæmorrhage might occur. In large hospitals the additional work and timing of operations beforehand and the necessity for special nursing afterwards made the routine use of this drug impossible. Rectal trouble occurred when a watery solution was used. If it was thought desirable to procure amnesia in any particular patient before a general anæsthetic was used, this could more easily be secured with twilight sleep.

Mr. ROWBOTHAM (in reply) said he had met with no cases of idiosyncrasy to paraldehyde; if it existed it was rare, and could no more be regarded as a contra-indication than it was in the case of other drugs. The smell was certainly objectionable—to everyone but the patient, who was never conscious of it. He was convinced of its absolute safety when used in the way he had described.

For nose and throat cases with endotracheal gas and oxygen, the laryngeal reflex returned before the patient left the table; he remained asleep for some hours (which he, the speaker, regarded as an advantage), but depressed respirations did not occur unless the heroin had been overdone in dosage. Vomiting was usually entirely absent, but he had seen a few cases in children and these he ascribed to acidosis. Rectal trouble never occurred. It was not intended as a routine method for use in hospitals where the accurate timing of the dose and extra nursing were impossible.

Section of Odontology.

[*January* 28, 1929.]

Three Cases of Compound Composite Odontomata.

By A. T. Pitts, D.S.O., M.R.C.S., L.D.S.Eng.

The compound composite odontome is undoubtedly one of the rarest types of odontomata, though, in the aggregate, a considerable number have been recorded while other cases occur which are not published. It is highly desirable that the rare varieties of odontomata should be placed on record, for only in this way can we collect sufficient data to enable us to work out thoroughly their evolution and pathology.

Case I.—The first case occurred in a boy of nearly 15, whom I saw in December, 1927. There was a hard, irregular swelling about the size of a filbert nut between the right upper canine and second premolar. The first premolar was present but displaced lingually and rotated. The swelling was painless; the gum over it normal and freely movable. I was unable to obtain any satisfactory history of the duration of the swelling. In spite of its size neither the boy nor his mother could say anything more than that it had been present some considerable time, and so far as I could gather no alteration in size had been noticed. The

Fig. 1.

Case I.—Radiograph (by Miss D. Allen); showing compound composite odontome. Right upper premolar region. Male, 15 years.

mother assured me that the boy had possessed a normal deciduous dentition. The boy brought with him a radiograph taken by Miss Allen (fig. 1). This showed an irregular, conglomerate mass of calcified tissue between the canine and second premolar. The diagnosis lay between a compound composite odontome and a complex composite odontome, for I could not be certain from the film whether the mass was fused, or consisted of separate denticles, though I suspected the latter. I arranged for the boy to be admitted under my care at the Middlesex Hospital, and under a general anæsthetic I operated on him in January, 1928. On reflecting the gum I found that one denticle was projecting partly through the bone as though it was in active eruption. The bone was very thin and on removing it I found that the tumour consisted of a number of denticles closely packed together. It was necessary to remove the first premolar in order to be sure that all the denticles were removed. I paid especial attention to the question as to whether there was a capsule present. There appeared to be some soft tissue round the denticles but nothing in the nature of a tissue wall. After removal of the tumour I sutured the gum into its place. The boy made an excellent recovery without any pain and the wound healed by first intention.

Description of the Tumour.—The tumour consists of at least 18 denticles, though some of them are so minute in size that it is quite conceivable that others were lost at the time of operation. Some of the denticles were embedded in fibrous tissue while others appeared to have an investing membrane, though in no case were any of the denticles adherent to the bone. Sections were made through two of them. In one, enamel, dentine, and cement were all present. The enamel was somewhat granular but the dentine was well-formed. In the other section no trace of cement could be found.

One of the calcified masses was much larger than the others. It consisted of four or five denticles fused together in an irregular fashion. Three others were of considerable size but smaller than the largest mass. Each of them appeared to be a single denticle, though irregular in shape, thus differing from the largest mass which was formed by the fusion of several denticles. Of these three masses, two, as already stated, were used to make sections, whilst the third has unfortunately been lost. Of the remaining denticles some of them had conical crowns with distinct roots, whilst the smallest fragments were minute calcified bodies of about the size of a pin's head. The total number of denticles found was 18 of which 15 have been preserved (fig. 2).

A radiograph was taken of the denticles, which showed that in the larger of them there was a root-canal present and distinct indications that they were composed of both enamel and dentine.

Case II.—Patient, a Jewish boy, aged 3 years 8 months, whom I saw in the Dental Department of the Hospital for Sick Children, Great Ormond Street, July, 1928. The mother

FIG. 2.

Case 1.—Photograph (natural size) of denticles removed from compound composite odontome; right upper premolar region. Male, 15 years.

FIG. 3.

Case 2.—Radiograph of compound composite odontome; right upper deciduous central region. Male, 3 years 8 months.

brought him because of the failure of eruption of the right upper deciduous central incisor. All the other milk teeth were present in normal position. On examining the boy I found a small, rounded, hard swelling present above the gap. The mother had not noticed its existence, so that details as to its duration or whether it had increased in size were lacking. A radiograph was taken which showed that the permanent incisors were present and that lying above the space between the left central and right lateral incisors was a somewhat triangular mass of denticles. The diagnosis of compound composite odontome was clear (fig. 3). The boy was admitted to hospital and a few days later I operated under a general anæsthetic. On reflecting the gum I found the bone very thin and on removing it I found several denticles closely packed together. When these were removed an incisor tooth was exposed which was displaced to the distal side of the middle line. At the time I took this to be a permanent right central incisor, but, as a subsequent radiograph showed, it was in reality the missing deciduous central incisor. There was a thin capsule partly

investing the denticles and continuous with the tooth sac of the incisor. As I was anxious to preserve this tooth I did not attempt to remove its sac. The gum was replaced and sutured. Healing was uneventful. A few weeks ago I saw the boy again. The gum was soundly healed but as the mother stated that he occasionally complained of slight tenderness in the region of the wound I had him radiographed. This showed, rather to my astonishment, that the deciduous central incisor was present and appeared to be in process of eruption. Its pressure had caused the deciduous lateral to swing toward the middle line. There was also another denticle which I had overlooked at the time of the operation, probably because it was concealed by the central incisor.

Description of the Tumour.—The tumour consists of nine denticles removed, and one still awaiting removal later. Six of the denticles are of fair size measuring about 8 mm. in length. The remaining three are extremely minute. The largest mass consists of a tri-tubercular crown with a single stunted root. The other five have a general resemblance to conical teeth. A radiograph showed a suggestion of a pulp-canal in the larger masses. No sections were made of these denticles (fig. 4).

FIG. 4.

Case II.—Photograph (natural size) of denticles removed from compound composite odontome.

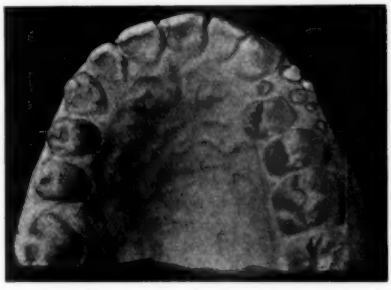


FIG. 5.

Case III.—Photograph of model showing six *conical* supernumerary teeth erupting in left upper first premolar region. Male, 14 years. (Mr. Marais's case.)

Case III.—The third case is one of six supernumerary teeth which erupted in the left upper first premolar region in a boy of 14. I have included the case as an example of a compound composite odontome of which the component denticles had erupted, and I am prepared for adverse criticism in so classifying it (fig. 5). The model with the extracted denticles was sent to the Museum of the Royal Dental Hospital in 1927, by Mr. Marais, of Zastron, South Africa, a former student of the hospital. The history is as follows:

The patient was a boy of 14, with six supernumerary teeth. Five of them were placed labially to the first upper left premolar which is displaced distally and lingually. One supernumerary tooth is situated between the premolar and canine. The boy stated that he himself had removed four other supernumerary teeth from the same place. This was confirmed by his elder brother. Thus, a total of ten denticles had erupted in the same region. It was not possible to radiograph the jaw so that it is just likely that there may still be

other denticles to erupt later. The denticles are well-formed, conical, though diminutive teeth.

This case is parallel with two others of a similar character which have come under my notice. One was that of a young man who attended the Royal Dental Hospital under my care some years ago. I removed a supernumerary tooth from the incisor region. He informed me that some years previously he had attended the hospital under Sir Frank Colyer who had removed others from the same region. The old case-sheet was found, which bore out his statement. Though the data are incomplete there is no doubt it was similar to my case. The other example is one of three supernumerary teeth which had erupted in place of an upper first molar. The denticles were sent to the Royal Dental Hospital some years ago. No further history was obtainable, and the specimen appears to be, temporarily, I trust, mislaid. No doubt similar examples of multiple supernumeraries in the same region are on record.

Commentary.—Within the limits of a casual communication a full discussion of the points suggested by these cases is not feasible. I shall limit myself to a brief consideration of three problems connected with these tumours.

(1) The possibility that some, or all, of the component denticles of a compound composite odontome may undergo eruption does not appear to have been considered. There is no mention of this phase in the evolution of these tumours in the classic monograph by Gabell, James, and Lewin Payne.[1] Yet I would suggest that it may occur.

When it happens the tumour, as such, disappears and is replaced by the eruption of a crop of denticles which may occur over a prolonged period. Whether eruption of the denticles would occur in every case if the tumour were left alone we cannot say, since, when such a tumour is discovered, it is usually removed.

The possibility of eruption must depend considerably on the position of the odontome and in some cases it is not likely to happen. It is significant that in my first case I found, on operating, that one of the denticles had pierced the bone as though it were attempting to erupt. If such an odontome is seen for the first time in its second phase when eruption of the denticles has occurred, the fact that the denticles may have had a previous existence as a tumour in the jaw, thus coming under the category of the odontomata, may be overlooked, and I believe that it is for this reason that apparently the connexion between the typical compound composite odontome and the cases of multiple supernumerary teeth erupting in the same region, as being two phases of the same condition, has not been realized. Indeed, it may not be straining the argument too finely to suggest that the single supernumerary tooth represents the simplest expression of the composite odontome; that the compound composite odontome is the quantitative evolution and the complex composite odontome the qualitative evolution of the simple supernumerary tooth.

(2) This leads to my second point, namely, that the first case also provides a link between the compound and the complex odontome. The largest mass consisted of several denticles fused together. Indeed the tumour might almost be looked upon as consisting both of a compound and a complex composite odontome. While some complex composite odontomes are amorphous masses, others have been described in which the denticles are embedded on the surface, suggesting that the difference between the two varieties is one of degree rather than kind and that the compound and complex odontomes, while clinically distinct, are ætiologically closely related.

(3) The first two cases I have described were supernumerary to the normal dentition, both deciduous and permanent. The case recorded by Mr. Harborow and the late Mr. Howard Mummery[2] arose in the same way, for both dentitions erupted normally in the region of the tumour. It is probable that most examples recorded have also originated from the tooth-band independently of the normal teeth-germs.

1 D. P. Gabell, W. W. James, and J. Lewin Payne, "Report on Odontomes," London, 1914.
2 G. Harborow and J. Howard Mummery, "A Compound Composite Odontome," *Proc. Roy. Soc. Med.*, 1921-22, xv (Sect. Odont.), 8.

But I would suggest that the compound composite odontome may also arise as an aberration of either the deciduous or permanent tooth-germ, though I cannot produce unimpeachable evidence for this. In the *Revue Belge de Stomatologie*[1] for March, 1928, there is a brief account of a compound composite odontome which arose in place of a deciduous canine. It occurred in a child of seven. It was stated that the absence of the deciduous canine was confirmed radiographically. A possible example of the development of a compound composite odontome at the expense of a permanent tooth is the example I quoted of three supernumeraries which appeared in place of an upper first permanent molar. In the absence of a full history and radiographs I am not prepared to give this case as an example of an odontome arising as an aberration of a permanent tooth-germ, but I would suggest tentatively that such an origin is possible and even probable, though the commonest site of origin is from the tooth-band independently of the normal tooth-germs. I have for some time been investigating the odontomes with a view to trying to work out their evolution. I have therefore seized on the opportunity presented by these cases to offer some preliminary observations which I hope to amplify at a later date.

[*February* 25, 1929.]

Fractures of the Mandible in, and Posterior to, the Molar Region.

By W. Kelsey Fry, M.C., L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

Although there have been no revolutionary changes of late years in the treatment of fractures of the mandible, I feel that a discussion on this subject is needed for the following reasons:—

(1) Such fractures are becoming more frequent owing to traffic expansion and development, and to the difficulty often experienced in extracting unerupted wisdom teeth.

(2) Although during the war we learnt a good deal on this subject, it still appears to be a "no man's land" between the territories of the general surgeon and of the dental surgeon, with the result that in many cases necessary treatment is delayed.

(3) It is necessary to emphasize the importance of beginning treatment as early as possible for these cases. Cases often occur in which the patient, having a fracture of the mandible, also suffers from concussion and perhaps an open wound on the face; the medical practitioner or general surgeon will treat the concussion and perhaps administer an anæsthetic in order to stitch up the wound, postponing the obtaining of expert advice on the fracture until a later date. The inevitable result of this procedure is that displacement of the fragment occurs, and the treatment in many cases is thus rendered more difficult and prolonged.

(4) To recall the importance of splinting in acute and subacute infection of the mandible in order to prevent pathological fractures, and, if such fractures do occur, to prevent displacement.

(5) To clear up the recent discussion concerning the necessity of splinting fractures in this region. I hope to point out that, although in simple fractures, with little or no displacement, and in many cases of edentulous mandibles, splints may be dispensed with, yet in more difficult cases complete immobilization is essential. In making this statement I am aware of the teaching of Lucas-Championnière, which I interpret as not antagonistic to splinting, but as emphasizing the necessity of procuring natural function as soon as possible.

[1] Raison and Ramorino, "Un cas d'odontome chez un enfant de sept ans," *Rev. Belge de Stomatologie*, Mars, 1928.

Owing to the wide range of this subject I shall limit my remarks to fractures in, and posterior to, the molar region.

Causes.—Before discussing the direct causes of these fractures, we must not forget predisposing conditions, such as subacute infection, and pathological conditions of the mandible requiring extensive operations, when surgical fractures are liable to occur. The former condition is most commonly found following the extraction of a molar. The common history of such a case is that the patient had a tooth extracted; this was followed by a swollen face, treatment was delayed and acute osteomyelitis occurred; the case was not splinted and a pathological fracture followed. The lesson to be learnt from this is to splint all cases of extensive osteomyelitis so as to avoid fractures. Another type of case is that of chronic syphilitic necrosis. The history of this is rather different, i.e., the patient, generally an adult, has a chronic infection of the mandible of long standing with numerous sinuses, and a yawn or a fit of coughing fractures the jaw. I have met two such cases in recent years. Fractures resulting from operation are frequent and call for the closest coöperation between the general surgeon and the dental surgeon. When it is found that a fracture is likely to occur (X-ray evidence will usually determine this beforehand) splints should be fitted before operation so that the fragments may be retained in good position. It usually happens in these cases that the dental surgeon is not consulted until after the operation, when the fragments have become displaced and bound down by scar tissue, thus rendering a simple case complex. Complete coöperation in these cases is the deciding factor for success. Other predisposing causes of fractures in this area are unerupted wisdom teeth, and the inherent weakness of the mandible in the region of the necks of the condyle.

The immediate cause may be either a direct or indirect injury, the latter being more common than would at first be suspected. For example, a man receiving an unexpected blow in the right canine region will often sustain not only a fracture in the left canine region but also in the right molar region, especially if there is an unerupted tooth present. On the other hand, if a patient falls upon the point of the chin and instinctively clenches his teeth, the probability is that he will fracture one or both of the necks of the condyles, no fracture taking place at the site of contact. With regard to the more common causes of fractures, such as blows, kicks, etc., a case which came under my care six years ago must be very rare. This was a fracture of the neck of the condyle at birth due to forceps. The case was interesting not only from the cause but from two other points of view as well: (1) The marked lack of growth on the affected side, and (2) the extraordinary way in which the teeth accommodated themselves to the condition, the mandibular and maxillary teeth being in good occlusion. "Good" refers to the functional and not necessarily to the orthodontic aspect.

Diagnosis.—Diagnosis of fractures in the molar region is usually simple, the well-known signs generally being present; in fractures posterior to this region diagnosis is often more obscure. The signs and symptoms to look for are: (*a*) deviation of the mandible from the mid-line to the affected side, and (*b*) crepitus—this is best ascertained by grasping the mandible with the thumb and index finger of each hand, one being placed on each side of the suspected fracture. In fractures of the upper part of the ascending ramus, crepitus can be obtained by placing the fingers of one hand well back inside the mouth and the other hand outside, or it may be heard by auscultation. Fractures of the neck of the condyle can sometimes be determined by placing the little fingers in the external auditory meatus and asking the patient to open and close the mouth, when there will be difficulty in feeling the movement of the condyle on the affected side, owing to the fractured condyle being displaced in a forward, inward or outward direction. If in an outward direction, diagnosis is obvious at once. Diagnosis can always be confirmed by X-ray plates taken in the postero-anterior and the right and left lateral positions.

Displacement.—The degree of displacement depends on the direction and force of the injury and upon the muscular contraction. The muscles attached to the mandible may be divided into three main groups—the elevators, the depressors, and the rotators (external pterygoid). The direction of the displacement of the fragments depends upon the predominance of one of these muscular groups. In fractures of the molar and angle regions the posterior fragment is displaced forwards and inwards by the external pterygoid, and upwards by the elevator muscles. The larger fragment is depressed and the mid-line of the mandible is diverted to the injured side. The amount of displacement depends upon: (1) the line of fracture, (2) the presence of teeth on the smaller fragment and with occluding teeth on the maxilla, and (3) the amount of loss of bone.

(1) *Line of Fracture.*—If the fracture runs from above downwards and forwards, as in No. 1*a* (fig. 1), or directly downwards, as in No. 1*b*, the forward and upward tilting of the posterior fragment will be resisted, whereas if the line of fracture runs from above downwards and backwards, as in No. 3, no resistance is offered to the displacement. Again, if the line of fracture runs from within and outwards, as in No. 2, resistance will be offered to the inward displacement, but if the fracture runs from without forwards and inwards, as in No. 4, no resistance is offered. Therefore, as will be seen from fig. 1, it may be stated that lines of fracture Nos. 1*a*, 1*b*, and 2

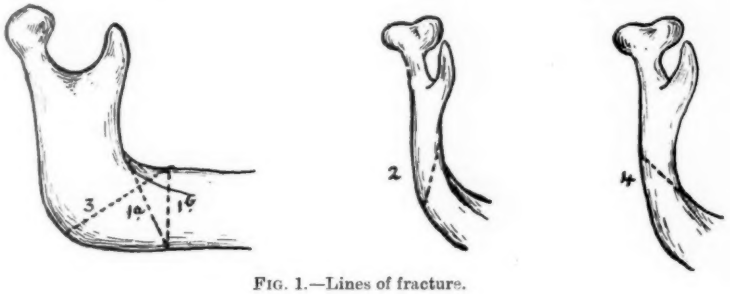


FIG. 1.—Lines of fracture.

are favourable, and Nos. 3 and 4 unfavourable so far as preventing displacement is concerned.

(2) *Presence of Teeth on the Smaller Fragment with Occluding Teeth on the Maxilla.*—When one or more teeth are present on the posterior fragment with occluding teeth on the maxilla, the tendency of the posterior fragment to be displaced is lessened owing to the teeth preventing its upward movement. The tendency of displacement is in proportion to the number of teeth present, i.e., there is more tendency to displacement if there are no opposing teeth on the maxilla, and still greater if there are no teeth on the posterior fragment.

(3) *Amount of Loss of Bone.*—The greater the loss of bone the greater the chances of displacement, other conditions being equal. When there is only a small loss the forward displacement of the posterior fragment brings it into contact with the main fragment, and advantage may be taken of this to obtain good functional results in these cases. Care must be taken, however, that the posterior fragment does not swing too far upwards and lose contact with the main fragment. Fractures of the coronoid process are always due to direct injury, the fractured process being displaced upwards and slightly backwards by the pull of the temporal muscles. In fractures of the neck of the condyle the amount of displacement is again largely dependent upon the force and the direction of the line of fracture. When the force of the blow is partly taken by an accompanying fracture of the horizontal ramus, the fractured

condyle is displaced forwards, inwards and slightly downwards on to the eminentia articularis by the action of the external pterygoid. This is the most common position of the condyle after fracture. If, on the other hand, all the force is taken by the condyle, as in a fall on the chin with teeth clenched, the condyle is forcibly displaced externally so that it may be felt with the fingers, or it is displaced directly inwards, the former being the more common. In all cases of fractured condyles on one side only the main fragment is displaced towards the injured side, but in the case of a double fracture the main fragment is depressed and the patient presents the obvious deformity of the open bite.

Treatment.—The general principles underlying the treatment of fractures of the mandible are the following: (1) Time to commence treatment; (2) Correct alignment; (3) Reduction of sepsis; (4) Immobilization of the fragments.

(1) *Time to Commence Treatment.*- The mortality in cases of injury to the mandible is very low and, except where the patient is suffering from other injuries which endanger his life (when the treatment of the jaw injury is a secondary consideration), treatment should begin as soon as possible. Dental splints should be fitted to immobilize the fragments at the earliest possible moment. By so doing the pain is relieved, the chances of sleep enhanced, the danger of sepsis considerably reduced, and the taking of suitable food made easier. Early treatment is specially necessary and almost essential when the soft parts are involved in the injury, otherwise cicatricial contraction of the soft parts rapidly produces increased displacement of the fragments, making the treatment not only prolonged but more difficult, whilst the disfigurement may be considerably increased. Early surgical intervention for wiring or plating, etc., is contra-indicated, because of the great difficulty of obtaining correct occlusion by this method, and because these injuries are nearly always compound, except those high up in the ramus: these cases respond well to ordinary treatment.

(2) *Correct Alignment.*—In simple fractures of the mandible without loss of bone it is usually easy to get and keep normal alignment and so obtain bony union of the fracture, but where the fracture involves loss of bone correct alignment is more difficult to obtain, as it depends on the position and the amount of bone lost. Where the amount of bone is not more than half an inch, union can usually be obtained by means similar to those used for simple fractures. Where the loss of bone exceeds two inches the chances of primary union are very slight, and the treatment is to hold the fragments in correct alignment whilst awaiting surgical interference in the nature of a bone graft. The chief difficulties in the treatment of fractures with loss of bone occur where this loss is between the two amounts above-mentioned, i.e., between half an inch and two inches. The method of treatment is largely based on the number of teeth lost, either before or at the time of injury, or extracted owing to sepsis. It is obvious that where many teeth have been lost, it is advisable to approximate the fragments (provided the approximation is not so bad as to interfere greatly with the cosmetic appearance), thus retaining the grinding power and obtaining the grinding surfaces by means of artificial dentures so designed and fitted as to furnish the necessary contact with the teeth of the upper jaw. When the patient retains sound teeth, it would not be justifiable, in the light of the recent great advances in the technique of bone-graft surgery, to approximate the fragments and lose natural occlusion, for artificial substitutes can in no way be compared with the natural teeth. The fragments should be held in correct alignment, reliance being placed on bony union being obtained through the possibility of small pieces of bone and periosteum remaining, to form osteo-genetic centres for the supply of new bone to fill up the gap. If the desired bony union is not obtained by these natural means, the case will fall under the category of those suitable for bone-graft operations.

(3) *Reduction of Sepsis.*—For successful treatment of these cases, it is of the greatest importance that sepsis in the mouth should be reduced to a minimum. This should be effected by the following means:

(*a*) *Constant irrigation.*—For this purpose it has been found that chloramine T (½ per cent. to 1 per cent.) is the most satisfactory antiseptic, and should be used three-hourly by means of a Higginson's syringe, especially after meals. (*b*) *Efficient drainage.*—It is of the utmost importance in severe injuries in the molar region to establish submaxillary drainage. Should the position of the injury to the soft parts, if any, not be suitable for the purpose, a fresh opening must be made and a drainage tube inserted, care being taken to make the incision well under the mandible to avoid scarring. (*c*) *Removal of septic teeth, and teeth in the line of fracture.*—It is essential, in freeing the oral cavity from sepsis, to remove all septic teeth. The treatment of the teeth in the line of fracture has been the subject of much controversy, but it is now almost universally accepted that it is advisable to extract all teeth in the line of fracture as soon after the injury as possible. Lines of fracture are open pathways for the spreading of an infective process, and when teeth are in this line a secondary inflammatory condition of the periodontal membrane is bound to follow, and the epithelial attachment between the buccal mucous membrane and the teeth is broken, this resulting in pockets being formed which always prove a source of sepsis and deter bone formation. In a large number of cases under observation it has been remarkable to notice the rapidity with which fractures have united after the teeth involved have been removed; on the other hand it has been equally noticeable, in examining by X-ray old injuries which have not united although the fragments have been in good alignment, that a tooth has been found in the line of fracture, on the removal of which union has rapidly taken place. Fractures will sometimes unite with teeth in the line of fracture even when there is considerable sepsis, but the time taken is prolonged and the chances of pseudo-arthrosis much increased, while the teeth have eventually to be removed because of resulting chronic periodontitis. It has often been observed that when a tooth has been left in the line of fracture, it has resulted in an increased loss of bone from sepsis, with the result that the adjacent tooth becomes involved. Sometimes, however, a tooth is retained on the posterior fragment of the mandible which at first examination would require removal, but it must be remembered that it is extremely important to have a molar tooth left for splinting purposes, and the benefit of retaining it, even for a time, outweighs the septic risks of keeping it. Skiagrams should be taken at intervals during treatment, and the condition of the area round the tooth and the progress of repair carefully observed.

(4) *Immobilization of the fragments.*—As previously stated, considerable discussion has recently taken place as to the advisability or otherwise of the splinting of these fractures; prolonged splinting is contrary to the teaching of Lucas-Championnière, and it not only delays union, but frequently causes trismus when the splints are removed. It must be remembered however that splinting is used for two purposes—(1) to aid reduction of the displacement, and (2) to immobilize the parts.

Displacement can be reduced by (*a*) the immediate method, or (*b*) the slow method. The immediate method is to take an impression of the mouth and divide the lower model at the line of fracture, articulating in sections. Splints are then made for the reconstructed position, the fracture reduced and the splints fitted under a general anæsthetic. This appears simple, but my experience is that except in the simplest cases it is very unsatisfactory. What generally happens is that the fracture is difficult to reduce completely, and sometimes, even if this has been possible, one of the fragments is very liable to slip the splint either when the patient is recovering from the anæsthetic or immediately afterwards, this being due to the powerful muscles acting on the fragment before the cement has had time to harden. I therefore suggest that in all difficult cases the slow method be adopted. For the slow method, splints for the mandible are made in sections; a splint is also made for the maxilla. The splints are cemented in position at least twelve hours before the anæsthetic is given for the reduction of the displacement. Under the anæsthetic

the displacement is reduced as far as possible, the two sections being brought into as good an alignment with the maxilla as possible. Any further reduction can be effected at intervals by gradually tightening the wires between the mandibular and the maxillary splints.

The methods of immobilization are: (*a*) Bandages and external supports, e.g., four-tail, barrel bandage (fig. 2), Hamilton's strap support and skull cap and chin support. (*b*) Inter-dental wiring.—There are four varieties of inter-dental wiring (fig. 3). The disadvantage of the direct method (fig. 3A) is that there is a direct upward and downward pull on the corresponding teeth, which is liable to loosen them and to pull them out of their sockets. In the other methods there is less

FIG. 2.—Jaw bandage.

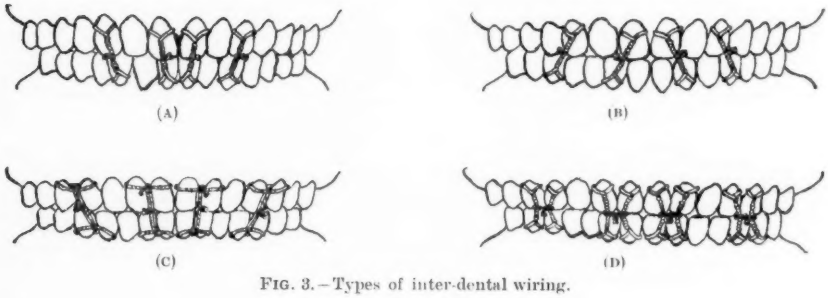


FIG. 3.—Types of inter-dental wiring.

direct pull and the strain is extended over a greater number of teeth. (*c*) Dental splints.—Much ingenuity has been expended in devising dental splints, but the form I favour is the metal cap splint and its variations, except in edentulous cases where the vulcanite Gunning splint is used.

I shall not discuss these splints in detail, but shall refer to them in describing a series of cases of fractures in these regions.

Case 1.—Cause: Boxing blow, right premolar region. Fracture in right molar region through unerupted wisdom socket. Secondary fracture in left premolar region. No displacement. Small double metal Gunning splint fitted and worn for a week. Wisdom tooth not removed, but careful watch kept for signs of sepsis.

Case 2.—Cause: Kick. Fracture in right molar region. Tooth on posterior fragment. No displacement. Single cap splint fitted for three weeks.

Case 3.—Fracture in right molar region. No tooth on posterior fragment. No displacement. Single cap splint with horizontal flange fitted and worn for six weeks.

Case 4.—Cause: Extraction of tooth. Fracture in right molar region. No tooth on posterior fragment, which was displaced upwards and forwards. Double metal cap splint fitted to hold main fragment in its correct relation to the maxilla, but as it was impossible to control the posterior fragment, further muscular action increased the displacement and non-union resulted. Operation for wiring at a later date gave good result.

Case 5.—Cause: Motor accident. Fracture in left molar region with teeth on posterior fragment and marked displacement. Sectional splints fitted and the fragments brought into alignment with the maxillary teeth by the slow method of reduction, and held in position for a month, after which the two sections of the mandibular splint were united in their corrected position, and the resulting single splint worn for another month.

Case 6.—Cause: Rifle bullet. Comminuted fracture in molar and premolar regions. Submaxillary drainage established and no pieces of bone removed. Main fragment brought into occlusion with the maxilla by a double cap splint, by slow method. Splint worn for six months. Result: No trismus and good union with correct alignment.

Case 7.—Cause: Extraction of wisdom and molar teeth. Fracture in left molar region. Three-quarters of an inch loss of bone from suppuration, remaining teeth good. Main fragment held in correct relation to maxilla by lower cap splint with vertical flange to await bone graft. Result of bone graft good.

Case 8.—Cause: Motor accident. Complete loss of left condyle, coronoid process and ascending ramus, including socket of wisdom tooth. This portion of the mandible was stripped of all soft tissues and was picked up twenty-five yards from the scene of the accident. A metal cap splint with a vertical guiding flange to maintain the remaining part of the mandible in its correct position was fitted for nine months; after this patient had complete function of the mandible; a radiogram showed that the whole of the ascending ramus, together with the coronoid process, had been restored.

Case 9.—Cause: Direct blow. Fracture of left coronoid process and zygomatic arch. No splints fitted, but various appliances used to encourage movement and patient put on chewing gum exercise.

Case 10.—Cause: Fall from cycle. Fracture at neck of left condyle. Displacement favourable: metal cap splint with flange fitted and barrel bandage worn for one week, after which movement was encouraged.

Case 11.—Cause: Fall on chin. Fracture at neck of left condyle. Head of condyle displaced directly outwards. Treatment.—Metal cap, with vertical flange fitted, and later excision of the condyle for æsthetic reasons. Normal functional result.

Case 12.—Cause: Fall on chin. Fractures of both condyles, both of which were displaced inwards. No treatment. Good result; and in spite of there being no condyle in the glenoid cavities, the only functional disability was that patient could not protrude the mandible.

Case 13.—Cause: Motor accident. Fracture of both condyles, left coronoid process and left premolar region. Marked displacement, mandible gagged on last molars; patient presented typical open-bite appearance. Upper and lower metal cap splints fitted; displacement reduced by slow method during two weeks. Complete immobilization for a further two weeks, when splints were removed. Result—no trismus and good function.

Discussion.—Mr. LEWIN PAYNE expressed his agreement with Mr. Fry's methods, and considered that the emphasis which he had laid upon the importance of early coöperation between the general surgeon and the dental surgeon should be stressed, with a view to the dental surgeon being called in before a considerable amount of damage had been done. In illustration of the point, he (Mr. Payne) mentioned that only recently he had had sent to him a boy, aged 6 years, with a swollen lower jaw, with a request for information as to the nature of the trouble. It was found that the boy was suffering from osteomyelitis, and, on the patient being radiographed, an old fracture, which had been in existence probably for some six to nine months, was discovered. The surgeon who had had charge of the case had overlooked that condition altogether. On the question of splints, he (Mr. Payne) claimed that the open method he employed ensured that the splint went down absolutely into its correct position, so that the teeth would occlude in a proper manner. On a point upon which Mr. Fry had touched very lightly, namely, that the length of time during which a splint

should be applied should be as limited as possible, he agreed that within a fortnight or three weeks most splints could be removed.

Mr. F. St. J. Steadman said that he agreed with Mr. Fry in many of his contentions with regard to the necessity of splinting in cases where there was marked displacement of the fragments. In a large number of cases, however, splinting, in his opinion, was quite unnecessary. He never splinted unless absolutely obliged, and failed to see how splinting would prevent a pathological fracture occurring. Cases in which the occlusion was not satisfactory improved strikingly if left to nature.

Mr. H. H. Kenshole said that the case shown by Mr. Kelsey Fry, in which a large portion of the mandible, including the whole ascending ramus and condyle, had been forcibly avulsed, and in which a new ramus had developed, raised the question of regeneration of bone in the mandible. Anatomists and embryologists stated that this bone grew by a process of "ossification" in membrane, and not by periosteal development, and some general surgeons in consequence went so far as to state that no involucrum was ever formed following wide-spread loss of this bone, such as occurred in acute osteomyelitis. In practice, however, it was found that a complete involucrum was formed, and it remained for the anatomists to explain the source of this regeneration.

He (Mr. Kenshole) further asked whether there was any alternative to open operation and wiring in a case similar to the following: a fracture of the mandible ½ in. anterior to the ascending ramus, with both jaws totally edentulous. Such a case was treated by a double Gunning vulcanite splint, but the pressure upon the alveolar ridge of the posterior fragment was such as to cause sloughing of it, accompanied by considerable neuralgia.

Mr. F. N. Doubleday referred to the fact that Sir Francis Palmer had used no other means of splinting but silver wire, with which he had obtained good results. He (Mr. Doubleday) expressed great belief in the covering of the whole of the occlusal surface with the splint, and also, in the taking of impressions in plaster, he used a tray which had been cut down to about one-third of its usual depth. He also advocated the removal of third molars when the fractures occurred through the line of those teeth. Union had taken place within a fortnight in a case of his which, in his view, would not have occurred unless the third unerupted molar had been removed.

Mr. Percival Cole said that he fully agreed with Mr. Kelsey Fry's plea that the necessity for coöperation between the general surgeon and dental surgeon should be more generally recognized. At the same time he contended that the general surgeon was not always at fault; it was possible that the dental surgeon was at times responsible for failure in this respect. He instanced a case of pathological fracture due to extensive osteomyelitis which had been assiduously treated, without invoking a surgical opinion, in the dental department of a large London hospital. That Mr. Kelsey Fry's protest against the attitude taken up by the general surgeon, particularly in operations on the jaw and the mouth, was generally justified he was quite prepared to concede, and he regarded it as a lamentable condition in view of the overwhelming advantages to be obtained from team work, as evidenced in the evolution of maxillo-facial surgery during the war. He suggested that it was high time that stomatology should be recognized in this country as a specialized branch of surgery. He agreed with Mr. Kelsey Fry that fractures of the condyle were due to indirect violence brought about by such an accident as the impact of the chin on the steering wheel of a motor car. It was important to be certain that a fracture of one condyle only had been determined, and he urged the necessity of bilateral X-ray examination in all such cases. He had himself accepted the evidence of a unilateral examination, made in a case referred to him from a provincial town, and was annoyed to find some days later that a more extensive fracture existed on the other side. He was particularly interested in those cases illustrated by Mr. Kelsey Fry, in which fracture was associated with the removal of an impacted wisdom tooth. Correlating these with the very unusual fracture occurring at birth owing to damage by forceps, he could not help commenting on the fact that similar injuries would appear to be determined in adult life by a similar mechanism. He agreed that displacement of the posterior fragment in angle fractures was usually upwards and inwards, but the direction of the violence might bring about a displacement in which the posterior fragment lay outside. He quite agreed with Mr. Kelsey Fry as to the overwhelming advantages of immobilization at the earliest moment. The results which a waiting policy were alleged to attain,—diminution of swelling, diminution of pain and discomfort, and control of sepsis,—were effected more speedily and surely by immediate action, and to this must be added the

additional advantage that early and effective immobilization was the best safeguard against the supervention of necrosis and consequent loss of bone, and possible non-union. Mr. Kelsey Fry had demonstrated several cases exhibiting a loss of bone which in his (the speaker's) experience was exceptionally met with in civil practice. He suggested that this condition had actually been brought about by faulty treatment, and would ask Mr. Kelsey Fry whether the suggestion was well founded or not. He agreed that in the treatment of recent fractures, operative measures were to be deprecated, but he maintained that surgical interference of a discriminating nature would enable the dental surgeon to obtain immediate reduction in every case—an objective which he, in conjunction with dental colleagues and particularly with his co-worker, Mr. C. H. Bubb, had never failed to attain. The author of the paper had revived, as he was in duty bound to do, the old controversy as to the retention of teeth in the line of fracture. He felt that Mr. Kelsey Fry had to some extent misrepresented the attitude of that small minority of practitioners who had urged retention. Retention had been advocated only when a tooth or the teeth in question were of vital consequence in assisting the operator to obtain a result which their sacrifice would make impossible, or at any rate problematical. He noted that Mr. Kelsey Fry had expressed himself fundamentally in favour of these views, as evidenced particularly by his attitude towards the single tooth in the posterior fragment. He maintained that the author, as representing the majority, had been converted. Mr. Fry had laid special emphasis on a lantern slide illustrating a case in which an extreme degree of comminution had been followed by rapid consolidation and an excellent result had been obtained. He (the speaker) suggested that provided that one abstained from meddlesome interference, the outcome of such a condition could safely be regarded as favourable, in fact so impressed had he been with results obtained during the late war in such cases that he had come to regard comminution as a favourable factor in proportion to the amount exhibited.

Mr. GEORGE NORTHCROFT (President) asked Mr. Fry whether, in one case mentioned, where the tooth was fractured through the pulp, the fracture had given rise to considerable neuralgia while it was being splinted.

Mr. KELSEY FRY (in reply to questions raised) said that his usual period for splinting was about two or three weeks. Mr. Steadman had said that the simple fracture required no treatment. Mr. Fry considered that the fifth of the reasons stated for reading the paper met that point. In one of his cases of fractured mandible, Mr. Steadman had referred to the patient as training himself into producing a good occlusion. If a splint had been used, the patient would have had it, and there would have been no need for training. In another case Mr. Steadman had trusted to nature to improve the occlusion. In the paper it had been stated that if the splint were removed at the psychological moment the patient would himself adjust any slight irregularity. He could not agree with Mr. Doubleday on the question of plaster. In difficult cases its application was not advisable, and another reason was to be found in the difficulty of securing a fit with splints, as compared with the use of composition for taking the impression. Mr. Percival Cole had referred to a case of pathological fracture. It so happened that an instance of this had been illustrated on the screen to show what should not happen. Mr. Cole had then dealt with immediate reduction, claiming that, in every case undertaken with the surgeon's collaboration, the result was successful. He (Mr. Fry) could not agree with the operation of opening up fresh areas, when reduction could readily be obtained without it. The loss of bone he considered was due to lack of treatment. In the case referred to by the President, the tooth fortunately had not given rise to much pain. He (Mr. Fry) considered that in Mr. Kenshole's case open operation and wiring was the correct treatment.

Fractured Roots of Central Incisors with Live Pulps.—F. N. DOUBLEDAY, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.—Mr. Doubleday showed a skiagram revealing fracture of the apical third of the roots and the incisal edge of the crowns of both upper central incisors in a patient aged 32. There was a definite history of the fractures resulting from an accident sixteen years previously, but the pulps were alive and gave normal thermal and electrical reactions at the present time.

The Use of Cement Linings in Cavities.—WILLIAM RUSHTON, L.D.S., R.C.S.Eng.—At a meeting of this Section in November last the use of oxyphosphate cement as a lining for cavities was severely criticized. My own opinion is that the

use of a cement lining is one of the most beneficial proceedings of conservative dentistry for the following reasons:—

(*a*) It acts as a non-conducting layer between the pulp and a metal filling. By the omission of this precaution, innumerable pulps have been killed by thermal changes.

(*b*) It seals up the cavity by its adhesive properties as no other material can do; thus preserving the pulp when the metal filling or inlay has become defective or has fallen out.

(*c*) In weak-walled cavities it strengthens the walls by adhesion; it is especially useful in the incisor region.

(*d*) It prevents discoloration of the tooth under a metal filling.

(*e*) It is valuable in starting a cohesive gold filling; the first pellets are imbedded in the soft cement, packed in the undercuts as it sets and then malleted without fear of rocking.

(*f*) It is possible by its means to retain a metal filling or a silicate filling, even with insufficient undercuts.

(*g*) As it is introduced into the cavity in a liquid state pressure is avoided. When it has set hard the pressure of any overlying filling will do no damage.

In condemning the use of cement lining, it was stigmatized as "a stinking sponge." These words seem to me to be not only unjust but singularly inappropriate. The main feature of a sponge consists in it being highly absorbent; cement on the contrary is very non-absorbent, as you can prove for yourselves by immersing a pellet in aniline dye. The same non-absorbent quality makes it a very clean filling, especially on a self-cleansing surface. Where it sometimes fails is at the cervical edge of a non-self-cleansing surface. But even there, or under a leaky crown, it is not the cement which stinks but the organic matter which has found lodgment and has decomposed. The earlier method of using the cement was to wait until it had set fairly hard, when it was trimmed up and the metal filling introduced. The more modern method, introduced by Sir Harry Baldwin, is to insert the metal filling while the cement is in the semi-liquid state; by this means he claims that certain advantages over the older method are forthcoming.

Section of Otology.

[*December* 7, 1928.]

DISCUSSION ON MENINGITIS.

Mr. G. J. Jenkins.

ABSTRACT.—(1) Meningitis: two groups of cases. (2) A method of washing out the subarachnoid space in cases of septic meningitis secondary to infection of the ear. (3) Discussion on the value of maintaining a positive pressure of the cerebrospinal fluid when operating on a septic region communicating with the subarachnoid space. (4) Leaking cerebrospinal fluid from the region of the ear: operative treatment.

IN a paper read at the International Otological Congress in Paris (1922) I explained the importance of recognizing two groups of cases of meningitis, one in which the infection was primarily of a trabeculated part of the subarachnoid space and the other in which the infection was primarily of the cisterna pontis. An appreciation of this is of great assistance in understanding and recognizing the early symptomatology of various types of meningitis and also in the rational treatment of the disease. Many cases of meningitis of otitic origin end fatally, and it is for us to consider how we can lessen that mortality.

In an occasional case, even with micro-organisms present in the cerebrospinal fluid, the patient has recovered after one or two lumbar punctures. This seems not to indicate this procedure as a treatment, but to prove conclusively that if we can help in some degree, the cerebrospinal fluid can overcome the inflammation, whereas without our assistance it may fail to do so.

How then can we help? In many cases of meningitis the organism has not reached the subarachnoid space, and the symptoms and signs are due to infection in close relation to the outer surface of the dura—the "méningite de voisinage" of the French. In these cases the complete removal of the external inflammation will effect a good result. The difficulty is to recognize this state of affairs, and only in very early cases we can be content with this procedure. We arrive at a conclusion by evaluating the symptoms, the site of otitic infection and the general clinical condition, and by examination of the cerebrospinal fluid. The changes in the fluid can be estimated only when considered in relation to the clinical feature.

Lumbar puncture is a serious procedure and should only be undertaken to obtain information as to the state of the meninges, which may help in deciding the line of treatment; even so, not more than 6 c.c. of fluid should be removed, and that very slowly. Lumbar puncture should not be performed for diagnostic purposes when septic meningitis is suspected.

Clinical and cerebrospinal fluid examinations will enable us to recognize both the early and the advanced affections, but between these extremes there is a wide, doubtful group, which I think we shall never be able to diminish, and I incline to treat these cases as probable infections of the subarachnoid space.

If we take meticulous care in cleansing the wound after completing the external operation and before opening the subarachnoid space, the risk from the whole procedure is much less than if the patient is left without the latter operation.

Obviously, the general principle of the surgical treatment of meningitis must be that of the treatment of a similar inflammatory process elsewhere. We should drain the meninges from the point of maximum infection and encourage the flow of normal cerebrospinal fluid over the infected area.

In most cases, in addition to simply draining the subarachnoid space, I wash out the space when possible and introduce a small quantity of specially prepared iodoform. This method has given better results than simple drainage; obviously the absence of an experimental control makes definite comparison impossible.

The following is a short description of the procedure, e.g., in a case of meningitis secondary to septic labyrinthitis.

Neither atropine nor morphia should be given. The most suitable anæsthetic is gas-and-ether, or ether only. While the anæsthetic is being given, the "washing" apparatus is arranged on a separate table. The iodoform is washed free from the perchloride with modified Lockes's solution.

The patient is then put on his side on the table and 5 or 6 c.c. of cerebrospinal fluid are drained off by lumbar puncture and is given to the pathologist, who has everything ready for a rapid cytological and chemical examination. The patient is then turned on his back and the superior and inferior labyrinthotomy operations are completed. Care must be taken to remove thoroughly all mucous membrane and tissue likely to be grossly infected. The wound is syringed out with hydrogen peroxide and saline, and lastly syringed with 2 per cent. alcoholic solution of iodine. A fine needle is passed into the labyrinth and the interior is well douched with the iodine solution. Fresh instruments, towels, etc., are made ready for the next stage, namely that of opening into the internal auditory meatus. By this time the surgeon should have the pathologist's report and will decide whether drainage is necessary or not. Before further steps are taken, the patient is again rolled on to his side and a lumbar puncture is performed. It is important that not more than one or two drops of cerebrospinal fluid should escape—just enough to make certain that the needle is in the subarachnoid space.

Next, the opening into the internal auditory meatus is made through the inner wall of the labyrinth in the manner first designed by West and Scott. When the flow of the cerebrospinal fluid has been definitely established, the lumbar puncture needle is connected with the reservoir of modified Locke's solution, with a fluid head of about 1½ feet.

I usually put a drain of pleated silkworm gut into the internal auditory meatus to help the flow of cerebrospinal fluid at this stage and leave it after the operation is completed in order to maintain the flow. When a few ounces of the Locke's solution has passed through, the serum, carrying about 8 gr. of specially prepared iodoform, is injected through the lumbar-puncture needle and the Locke's solution is again allowed to flow until fluid is returned clear at the ear.

It is important that the lumbar puncture should be performed before the opening is made into the subarachnoid space above; otherwise, with escape of cerebrospinal fluid from the ear, the tension of the dura may be so diminished that it may be difficult to puncture in the lumbar region. Also, only very few drops should be drawn off by lumbar puncture, as diminished pressure may interfere with the escape of the most infected material through the internal auditory meatus and also may diffuse a grossly infected fluid in and above the meatus.

Sometimes when the internal auditory meatus route does not afford drainage, owing to blocking by swelling, etc., the cisterna pontis may be reached satisfactorily in front of the cerebellum. For this route it is advisable to remove a considerable portion of the bone behind the lateral sinus so that the drain may be passed easily along the posterior surface of the temporal bone through an opening made in the dura, internal to the sinus. The lateral recess of the cisterna pontis extends a very little beyond the opening of the internal auditory meatus and is tapped there by the drain. The wound is then lightly packed with specially prepared iodoform gauze. When the patient is put back to bed bromides and glucose are usually given by the rectum.

These patients almost always have a low blood-pressure, and endeavour should be made to raise it, as a low blood-pressure diminishes the flow of cerebrospinal fluid. As much liquid as possible should be given, and also glucose. Pituitrin helps for a time but there seems to be more improvement with digitalis and ephedrin. Atropine or morphia should not be given at any stage as these drugs also tend to diminish the flow of fluid. The sedatives mostly used are bromides and paraldehyde.

If the cerebrospinal fluid ceases to come away within twenty-four hours and the patient appears to be relapsing, a re-establishment of the drain should be attempted. If this is not possible by the internal auditory meatus it may be begun again by the pre-cerebellar route.

The first few dressings are done under gas anæsthesia with full aseptic precautions. The wound is douched with 2 per cent. alcoholic solution of iodine as the dressing and drain are removed. As a rule the drain is not replaced at the first dressing which is carried out on the second or third day after operation. The wound is repacked with iodoform gauze.

The subarachnoid space is irrigated from the lumbar region because there is thus less likelihood of interference with the flow of the most healthy cerebrospinal fluid than if the irrigation starts from the cisterna magna or other part.

Some patients have been washed through two or three times. Two patients who were washed out three times recovered and are now quite well. I have detected no ill-effects attributable to the washing out, but it is obvious that the least slip in any detail of the procedure may result in failure and in serious harm to the patient.

Apparatus and Materials used in Washing Out the Subarachnoid Space.—(1) (*a*) A graduated glass cylinder such as is commonly used for saline infusion. It should be of

smallish calibre so that small movements of the Locke's solution may be readily detected. (*b*) Two yards of india-rubber tubing with a metal connexion to fit the lumbar puncture needle. This is to carry the Locke's solution from the cylinder to the lumbar puncture needle. (*c*) Glass cap to cover the top of the cylinder. In hospital the cylinder is clamped to an upright rod to give 1½ to 2 ft. fluid head.

This apparatus is boiled and the parts wrapped together in cotton cloths. The parcel is then put in a padded bag. The bag and contents are autoclaved.

(2) *Modified Locke's Solution.*—The sodium bicarbonate element is omitted from the original Locke's solution as it has been found that the carbon dioxide is given off in the autoclaving and throws down the calcium content. A sterile concentrated solution is put up by the dispenser in 100 c.c. sterules. The sister in charge of the theatre puts the concentrated solution into a sterile litre flask and adds distilled water to a litre. The flask is then kept in boiling water for twenty minutes and covered with a rubber cap. The flask is covered in the upper part with a cloth separate from the other covering. The whole is again covered with cotton cloth, put into a padded cover, and sterilized in the autoclave.

Normal horse-serum is generally used as a vehicle for the iodoform. The capsule is put in alcohol for some time before the surgeon touches it. Serum from the patient's own blood is used when conditions are favourable, but experience proves this to be very rarely possible, even in hospital.

(3) *The Iodoform.*—The finest iodoform powder is still further ground by hand in a mortar and 10 gr. are put in a sterile half-ounce glass bottle. The powder is soaked in absolute alcohol, and immediately afterwards 1 in 1,000 solution of perchloride of mercury is added so as almost to fill the bottle. The alcohol ensures the watery solution getting into close contact with the particles of iodoform. The iodoform is not altered in the perchloride solution.

Before the operation the bottle with contents is placed in alcohol. Before use, at the operation table, the iodoform is freed from the perchloride solution by washing several times with the modified Locke's solution.

The lumbar puncture needle should be small in calibre and is conveniently autoclaved in a tube, within another protecting tube.

(4) *Iodoform Gauze.*—The iodoform powder is washed in alcohol and 1 in 1,000 solution of perchloride of mercury is added immediately. It is made in large quantities and rubbed into the ribbon gauze as required at the operation.

Leaking Cerebrospinal Fluid from the Region of the Ear.

This is not a common condition but when it does occur it is a cause of anxiety. We are familiar with it as a feature of fracture of the base of the skull, but have probably also found it in other conditions, though I do not recall having heard anything about it at meetings of the Section.

During the last two years I have seen leaking cerebrospinal fluid in three cases of temporo-sphenoidal abscess; in two the leaking occurred during the process of healing some few days after opening of the abscess, and in the third some weeks after apparently sound healing. In a fourth case the leakage was from a hernia cerebri (cause indefinite) some weeks after healing.

When the leaking occurs during the first few days after the opening of the abscess, probably all that can be done is to keep the part clean and use iodoform gauze until granulation closes the opening. When the leak occurs in a late stage, I think that the plastic operation—moving a large flap of the temporal and sternomastoid muscles over the leak—is a sound procedure.

The following are brief notes of the three cases shown:—

In the case of F., cerebrospinal fluid came through the abscess cavity and the ependyma in a few days had grown out of the opening on to the external surface of the brain. The opening was so large that we could see into the ventricle. I put some serum-carrying iodoform into the ventricle, cauterized the channel of communication with pure carbolic, cut away more bone and gently pressed the more or less rigid tissue in order to close the opening.

In the cases of Mrs. S. and Miss P. E., the leak was from the hernia cerebri, and in both the opening was closed by muscle-flaps. In the latter case pressure of the cerebrospinal fluid was maintained by using the apparatus described.

In the case of Mrs. H., the patient not shown, the leak was at the lower margin of the opening in the dura mater. The granulations eventually closed the leak.

If it should be necessary to operate in a septic area through which cerebrospinal fluid

is leaking, it may be an advantage to have the fluid under a positive pressure and flowing continuously rather than ebbing and flowing in the bottom of the wound.

I employed the wash-out apparatus with this object in two cases, one being a fracture of the base of the skull with leaking cerebrospinal fluid, and the other a leaking cerebral hernia. In the fracture case, at the suggestion of my house surgeon, Mr. Faraby, I used a sucker to take away the excess of fluid from the bottom of the wound. In both these cases there was reason to believe that the leak was into a septic- or doubtfully aseptic-area.

Mr. T. B. Layton.

On Otitic Meningism.

ABSTRACT.—Meningism of hæmatogenous origin in certain general infective diseases.—Definition.—Symptoms.—Diagnosis.—Does such a thing occur with a local infection in anatomical relation with the meninges?—If so, it must always be considered as early meningitis.—Rigidity of neck only symptom of importance, if more, the matter grave.—Treatment.—Paracentesis.—Better to open mastoid (Case I) even though local symptoms are only those of otitis media.—When fully developed mastoiditis present leave wide open and douche (Case II)—Evidence that it is actually early meningitis (Case III).—Do *not* do lumbar puncture.—Theoretical arguments for this.—Analogy from peritonitis.—Confirmed by these and other cases.—Diagnosis may be wrong (Case IV), but a live patient is better than an accurate diagnosis.

AT the *Congrès Français de Medecine Interne* held at Lyons in October, 1894, M. M. Dupré, of Paris, read a note on a condition which he called "Meningism" (*La Semaine Médicale*, 1894, xiv, 485). He applied the term to "The syndrome of symptoms aroused by the irritation (souffrance) of the meningo-cortical regions (zones méningo-corticales) and independent of any anatomico-pathological change (alteration)." Referring to this communication in Allbutt and Rolleston's "Systems of Medicine" (vol. viii, p. 199, 1910), Dr. F. G. Batten says: "Although unendowed with a name, this condition has been long recognized as occurring, especially in children, during the specific fevers or almost any febrile condition." He describes the clinical features as being those of meningitis, namely, headache, vomiting, twitching and convulsions, retraction of the head, squint and rigidity of the trunk and limbs. All the concomitant symptoms of meningitis—slow pulse, irregular respiration, tache—may be present." The prognosis is described as good. The diagnosis depends upon lumbar puncture. "The fluid obtained by lumbar puncture may flow out under pressure, but is perfectly normal, and free from all cells and organism."

When the physician is dealing with this type of case, he has not yet decided whether the symptoms are due merely to toxins in the fluid, or whether this is the earliest stage of invasion by micro-organisms. In either circumstance the route to the cerebrospinal fluid is a hæmatogenous one. Is the same condition present in a localized infection about the meninges, such as that met with in our ear cases?

My object in these remarks is to urge that there is no such condition as "meningism" under these circumstances; and that so-called "meningism" should be regarded as the earliest stage of meningitis. In any ear case, if the patient's symptoms went so far as those I have quoted, we should call the condition a definite meningitis. If we did lumbar puncture in such a case the fluid would certainly not be perfectly normal; it would show cellular changes at least, even if it did not show the presence of organisms.

I maintain, however, that there is an earlier stage than this, and that the only symptom of any importance to the ear surgeon is that of neck rigidity. By the time that there are manifestations in the abdominal or other reflexes, the case has passed into a stage in which the chances of recovery are very small, despite even such brilliant results as Mr. Jenkins has achieved. Therefore, in my opinion, whenever there is rigidity of the neck, in a case of otitis, the condition should not be described as meningism, but should be regarded as meningitis, and lumbar puncture should *not* be done. By lumbar puncture such organisms as have already invaded the cerebrospinal cavity are sucked from the region of the ear down to the region of the foramen magnum, and thus the infection is distributed, and the patient's chances of recovery are reduced. It is analogous to pounding the right iliac fossa with a heavy fist in a case of appendicitis in which there is a suspicion of early, but not yet localized, peritonitis.

Some years ago I was called in to such a case by a physician who wished me to perform a mastoid operation; I did not think there was any evidence of mastoiditis. I did a paracentesis, and the child recovered, but I do not think I was right; it was taking too great a risk. I believe that the mastoid should always be opened.

The first of the patients whom I showed to-day was a small boy with double acute otitis media and a rigid neck. I opened each mastoid as quickly as I could. If there was any change in the bone, it was only hyperæmia of the osseous tissue. You can call such operation post-aural drainage if you like, rather than an attempt to eradicate the diseased bone.

The next case was that of a child who had measles and had been very ill with that disease. That child had double otitis media, with pus pouring from each ear. There was a suspicion of tenderness over each mastoid, and we discussed whether we should do a mastoid operation or not. We lost twenty-four hours over the case because I argued that there was no need for immediate operation, and that if we waited for twenty-four—or even forty-eight—hours we might know on which side there was acute mastoiditis and so avoid a double operation. During the next twenty-four hours rigidity of the neck set in. We found pus in each mastoid bone. We did not do lumbar puncture. In such cases I believe in repeated douching of the wound. In this case we had thoroughly laid open the wound and we douched away the débris of bone and blood-clot with a quart of saline or more on each side. We kept up the same procedure for three or four days afterwards. I do not know whether that after-treatment made any difference, but I think it is a useful line along which to proceed.

The third case produced evidence that the so-called "meningism" is always meningitis. It was that of a boy who had been operated upon for acute mastoiditis, and there had been pus against the lateral sinus. Next day rigidity of the neck came on. We did not perform lumbar puncture. Mr. R. J. Cann laid the wound widely open, washed it out with saline, and the boy recovered from his meningeal symptoms. Afterwards symptoms developed suggesting a thrombosed sinus; an operation was performed for that condition and again the boy became much better. Later he had symptoms suggesting cerebral abscess and Dr. Symonds advised an exploratory operation. I was reluctant to proceed with this because I felt that, if cerebral abscess should not be present, we should be taking away the patient's one chance of recovery by submitting him to further operation. The exploration was carried out, however, and in spite of our operation the patient recovered. This was the kind of case which Dr. Symonds described to us eighteen months ago as "encephalitis of the cortex."

I have brought a slide from a fourth case in which a mastoid operation was performed on a child who had a rigid neck and acute otitis media. After the operation the child went steadily downhill, and at the post-mortem examination miliary tuberculosis was found, with a tuberculoma in the parietal region of the brain, on the same side as the ear trouble. Careful examination at the autopsy showed that there was no question of spread from the ear to the meninges. This illustrates the fact that in cases of otitis media and rigidity of the neck you will sometimes make mistakes if you do not make a lumbar puncture. It is, however, better to have a live patient than to be certain of your diagnosis.

Illustrative Cases.

Case I.—P. S., boy. Double acute otitis media with rigid neck. Temperature 103° F. Operation: each mastoid opened. Bacteriological examination: *Staphylococcus albus*. Temperature remained raised for a week, then subsided; rose again and the child was very ill for about a fortnight.

Case II.—G. W., girl.—Admitted April 16, 1928, suffering from measles.

April 28, 1928.—Profuse left and right otorrhœa.

April 29, 1928.—Tongue coated, septic looking. Slight rigidity of neck on bending the head forwards. Looked ill. Tenderness over right mastoid. Operation: mastoids opened and both lateral sinuses exposed. No tracks through bone or granulations on meninges. Wounds syringed with saline daily for following week. Neck rigidity for a few days.

May 30, 1928.—Patient discharged.

Case III.—Shown by R. J. CANN, M.B. (introduced by Mr. T. B. LAYTON).—A. C., boy, aged 12. Chronic suppurative otitis media (right) following measles at age of 4.

Admitted to Guy's Hospital, April 28, 1928, complaining of pain and tenderness over right mastoid; vomiting and drowsiness.

Operation.—Right mastoid opened; pus in antrum and cells; pus in lateral sinus groove released; sinus collapsed. Dura middle fossa exposed; found normal.

April 29, 1928.—Temperature 104° F., pulse 100. Complaining of frontal headache; marked photophobia. Paralysis of right external rectus; marked neck rigidity; Kernig positive; abdominal reflexes normal; knee- and ankle-jerks not obtainable. Cavity syringed with saline.

May 5, 1928.—10 p.m. rigor. Temperature 106° F.

Operation.—Right internal jugular vein exposed; found collapsed; ligatured. Lateral sinus incised, contained pus, further exposed backwards till free bleeding obtained, wound packed.

May 9, 1928.—Neck rigidity diminishing; still photophobia.

May 12, 1928.—Temperature 104° F.; complaining of frontal headache. Examined by Dr. C. P. Symonds; marked papillœdema; visual fields good; paralysis sixth nerve (right); left abdominal reflex absent; other reflexes normal; Kernig doubtful; cerebellar tests negative. Cerebral abscess suspected.

Operation.—Exposure of middle fossa above mastoid; dura appeared normal. Exploration with needle and syringe; first puncture upwards gave flow of cerebrospinal fluid under tension at depth of 5 mm. only from dilated lateral ventricle. Exploration forwards and backwards of middle fossa negative. Exposure of posterior fossa behind lateral sinus. Exploration of cerebellum in three directions negative. Wound packed. *Diagnosis*: probably basal meningitis. Report on cerebrospinal fluid from ventricle: "Few red cells, no white cells; protein 0·03 per cent.; cultures sterile after seventy-two hours' incubation."

Recovery with good vision and no squint.

Case IV.—Microscopical Specimens of Tuberculoma of Meninges.—E. S., girl, aged 4.

July 5, 1928.—Drum injected, central perforation.

July 29, 1928.—Looked ill and thin. Drowsy, irritable, meningeal cry. Resented flexion of neck.

Mastoid operation (left) with exposure of middle fossa. Glairy fluid, no pus. Left myringotomy. After operation patient became steadily worse and died.

Post-mortem.—No purulent meningitis. One small tuberculoma of meninges; miliary tubercles in spleen and kidney.

Mr. E. D. D. Davis.

The Morbid Anatomy, Prevention and Early Diagnosis of Otitic Meningitis.

ABSTRACT.—Paths of infection in cases of otitic meningitis.—Vulnerable points in the dura mater.—Distribution of suppuration and its spread in basal cisternæ of the brain.—Consideration of methods of drainage from anatomical point of view.—Clinical types of infection of the ears which have produced meningitis, and prevention of spread of infection of the meninges.—Early diagnosis of spread of infection and indications for drainage of the meninges.

THE dura mater is known to resist—and to form a barrier to the spread of—infection from local cranial suppurations to the meninges and brain, but this barrier is weakened where the dura is pierced by nerves and vessels or at a cranial suture. As the dura mater of the middle fossa is not penetrated by nerves or vessels from the ear, otitic meningitis in this area is uncommon except in the case of children when an entry is gained at the petro-squamous suture. On the other hand, owing to the passage of nerves and vessels through the petrous bone, otitic meningitis is usually basal and the suppuration is found in the basal cisternæ and in the posterior fossa.

The recognized paths of infection are the following:—

(1) Through the internal ear and its various inlets, particularly the internal auditory meatus: (2) by lateral sinus and perisinus suppuration, also by the fossa subarcuata; (3) the less frequent passage through the middle-fossa dura mater; (4) a general blood-infection or septicæmia which includes those cases in which the path of infection is untraceable.

Infection by the internal ear rapidly spreads from the internal auditory meatus, the aqueduct of the vestibule or the fossa subarcuata to the cisterna pontis then to the cisterna basilis, to overflow along the cerebral vessels to inaccessible parts of the subarachnoid space.

The cisterna magna, or cisterna cerebelli medullaris, is the last of the cisternæ to be infected, hence the drainage of this cistern by Haynes' operation, or any other method, is too

late and of little value. In fact, when the suppuration has extended to the basal cisternæ, satisfactory drainage must be almost impossible, and if success is to be obtained, the diagnosis of the spread of infection and attempts to arrest the infection must be made before the inflammation has reached the cisternæ.

The dura mater of the fossa subarcuata is pierced by a vessel which arises in the cancellous bone surrounding the semicircular canals and labyrinth. This cancellous bone is in direct continuity with the retro-facial mastoid cells and also with the sinus tympani of the middle ear. The retro-facial cells may escape notice during the mastoid operation and continued suppuration in this area is a source of infection of the meninges.

Suppuration by way of the lateral sinus spreads to the posterior fossa and an oyster-like abscess covers the under-surface of the cerebellar hemisphere and finally spreads to the cisterna magna. When the path of infection is through the internal ear, translabyrinthine drainage by the internal auditory meatus and also drainage through the inner wall of the mastoid antrum in front of the lateral sinus should be carried out early.

Similarly, when the path of infection is by the lateral sinus, the posterior fossa should be drained both in front of, and behind, the lateral sinus. In the too-late cases, when the cisternæ are infected, Jenkins and Eagleton have advocated drainage and irrigation with Ringer's solution and serum (1) by the insertion of a cannula into the cisterna magna or spinal theca, and by a counter drain of a soft rubber catheter inserted from the middle fossa beneath the temporo-sphenoidal lobe to the cisterna basalis, or (2) by the insertion of the catheter from the anterior fossa along the superior longitudinal fissure or falx cerebri to the anterior end of the cisterna basalis. Irrigation is begun from the cannula in the cisterna magna to the rubber catheter. I have not had an opportunity of carrying out this operation on a patient but I have experimented on recent post-mortem subjects by irrigating the basal cisternæ of the brain with an aqueous solution of methylene blue passed by a coarse lumbar puncture needle in the cisterna magna to the catheter in front. It was disappointing to find that the methylene blue diffused throughout all the basal cisternæ, and also that a quantity of it was found free in the posterior fossa and only a small proportion of the fluid dripped from the catheter.

This experiment indicates that irrigation may spread infection, and this was what used to be found when the peritoneum was washed out with saline solution. However, the results of this irrigation during life may justify its use.

It is readily understood that the earliest possible diagnosis of the spread of infection and its path is essential for successful treatment. There are certain clinical types of suppuration of the ear which have resulted in meningitis and require a careful watch for symptoms indicating a spread of the infection. It is significant, on looking through the records of cases of meningitis following acute suppuration, that nearly all the patients are children or young adults, and this is particularly so in nasal sinus suppuration.

Acute suppuration of the middle ear or mastoid after scarlet fever has been followed by meningitis. The same may be said of measles and of that acute hæmolytic streptococcal infection of rapid and sudden onset occurring in children.

When unaccountable pain, headache and temperature persist more than two days after the mastoid operation for acute suppuration, a careful investigation should be made and the mastoid reopened. Sometimes an unopened deep retro-facial or zygomatic cell may be found to contain pus, and it is the failure to discover such an unopened cell which is the cause of the disaster and not the want of drainage of the wound which has often been blamed. The persistent headache, temperature and paralysis of the sixth nerve, known as Gradenigo's syndrome, is a serious warning of the spread of infection and requires a careful search for pus in the zygomatic cells.

A neglected lateral sinus infection or thrombosis has produced a few cases, but the larger proportion of cases of meningitis have followed an exacerbation of a chronic suppuration or cholesteatoma of long duration, and it is in these cases that the path of infection is frequently through the internal ear.

The records of cases of meningitis following chronic suppuration reported by Logan Turner, Fraser, Jenkins and myself, have shown that symptoms and signs of labyrinthitis were present in a very large proportion. Vertigo, tinnitus, nystagmus, a marked degree of deafness accompanied by headache and fever with an increase of cells in the cerebrospinal fluid, call for translabyrinthine drainage of the meninges without hesitation. Signs of labyrinthitis with headache and a temperature emphasize the urgency for drainage of the meninges. These symptoms and signs are often more or less absent, and changes in the cerebrospinal fluid may be the only indication. The cerebrospinal fluid should always be examined when the slightest suspicion of an intracranial infection arises.

There are certain signs on the road of the spread of infection which forecast coming events. Labyrinthitis is such a sign, and requires careful investigation and prompt action as soon as it becomes apparent. Continued headache and a rise of temperature are early symptoms, but drowsiness, torpidity, rigidity of the neck, and vomiting mean that meningitis is well established and that it is probably too late for successful treatment.

In conclusion, I may say that I have never had reason to regret having drained the meninges too soon, but there are many cases in which such measures have been neglected or carried out too late. The success of the surgery of peritonitis has been mainly due to the early recognition and prompt treatment of those conditions causing a diffuse or general peritonitis, and the same success should be obtainable in the surgery of meningitis.

Mr. Eric Watson-Williams said that the point which most impressed him about otitic meningitis was the urgency of the condition. It was necessary to make the diagnosis and get on with the treatment at once; one could not wait for elaborate investigation, and if no special equipment was at hand one could not afford time to procure it. A delay of only a few hours might make a great deal of difference in the result.

His own case notes for the last three years showed that during that period he had operated in seven cases of meningitis, and in one which he classified as a case of meningeal reaction. Lumbar puncture was, he believed, of invaluable assistance in deciding—not merely the diagnosis, as that might remain obscure even after the puncture—but the line of treatment. If the cerebrospinal fluid was clear it was probably safe to refrain from opening the meninges. His own impression was that if pus cells could not be centrifuged from the fluid, the patient had a fairly good chance of recovery. He had known only two recoveries in cases of otitic meningitis in which streptococci were growing in the cerebrospinal fluid, and in neither of those cases had the meninges been opened. Probably if he had a case of the kind now he might open the meninges. In his seven cases of definite meningitis—excluding the one which was clinically meningitis but in which the fluid was apparently normal—there had been three recoveries and four deaths. The four fatal cases did not properly come under this discussion because none of them appeared to be cases of otitic meningitis. Three were cases of pneumococcal meningitis, and in one the meningitis was tuberculous. In the other three the condition was definitely otitic meningitis. One case he had shown at a meeting of the Section eighteen months previously, because at the operation of drainage through the internal auditory meatus the facial nerve had suffered, and he had made a glossopharyngeal-facial anastomosis. He had seen the patient again recently, and she was very well. She had been admitted to hospital with a high temperature and complaining of severe headache; her head was held well back, and she would not allow it to be pushed forward. She was mentally confused, and there had been a distinct delay in answering questions, a feature which he (the speaker) had always considered to be evidence of a grave involvement. Her cerebrospinal fluid was under high pressure, it was turbid, not milky, though more than opalescent, and was reported to contain many pus cells. The case was seen at 1 a.m., and no cultures were obtained nor was a further investigation made. There was a "dead" labyrinth, and at the operation he had found pus coming through an open foramen ovale. He had performed complete labyrinthectomy, and drained the meninges through the internal auditory meatus. There was a good flow of cerebrospinal fluid. The patient had been better on the next day, but on the third day she had become worse. During the night the fluid ceased to flow, and the next day he had feared that the patient would die. All the cerebral symptoms had returned. He did not "wash-out," but he put a curette down the internal auditory meatus, scraped out débris and granulations, and re-established drainage of the cerebrospinal fluid. After that there had been an uninterrupted recovery.

At the beginning of this year he had had a patient whose trouble had begun with "biliousness" a week previously. (In parenthesis, he might remark that in almost every case of suppurative labyrinthitis that came to him the diagnosis had been "bilious attack," or "gastric influenza.") There was double suppurative otitis media, of many years' standing, with severe pain in the back and in the head, and pronounced neck rigidity. The cerebrospinal fluid was purulent, and contained polymorphonuclear leucocytes; no organisms were found on culture. He had operated on both sides—on one side doing the Schwartze operation, on the other a labyrinthectomy—and had not opened the meninges. The patient was a little better during the next two days; she then had a rise of temperature and severe occipital headache, and the head retraction was increased. He had then diagnosed a saccus endolymphaticus abscess, and he found and opened a large one. After repeated lumbar punctures the patient had recovered.

He believed that the cerebrospinal fluid had some antibacterial influence and that one should retain it. One could drain through the internal auditory meatus if one wished, but the cerebrospinal fluid was probably a better defence than an artificial fluid. The protein content was possibly responsible for some bactericidal action. He did not see how one was going to disinfect the subarachnoid space by introducing disinfectants. He was sceptical as to the value of disinfectants in contact with living tissues. To cause the death of bacteria by disinfectants, high concentration and a long time were required. Few tissues of the body would tolerate this. He confessed, however, that he had not tried the method of washing out, though, if he had a late case which was not doing well, he might become a convert. He had had one or two successes by the drainage method, and he felt inclined to adhere to that practice.

The essence of the matter in any case of meningitis, with indications of aural trouble, was to get the mastoid opened and then to do whatever further the findings indicated. In this way many cases might be taken in hand sufficiently early to spare the patient the enormous risk of a late suppurative meningitis, with bacteria growing in the cerebrospinal fluid.

Mr. Somerville Hastings (President) asked Mr. Jenkins if he would give the exact formula for the modified Locke's solution; and if he would say how much horse-serum he put in with the eight grains of iodoform. He (the President) had not tried this technique, but wished to do so, and desired to obtain the correct details.

Mr. F. W. Watkyn-Thomas asked, with regard to superior vestibulotomy, how far one should proceed in such cases. Usually the patient was very ill, and an exenteration of the posterior labyrinth by the Neumann method might be a long operation. Did Mr. Jenkins think it sufficient to open the horizontal canal for its whole length?

He (the speaker) had sometimes had difficulty in breaking into the internal auditory meatus from the vestibule. There was a useful device which Mr. Sydney Scott had taught him and which he hoped Mr. Scott would describe fully.

It was rare to find signs of meningitis in the irritative stage of labyrinthitis, but he had seen such a case. The caloric reaction was poor, but it was present; in view of the stiffness of the neck was one justified in opening a live labyrinth and establishing translabyrinthine drainage?

Mr. Holt Diggle said that meningitis was still a serious condition. Were the results of treatment to-day any better than those of, say, fifteen years ago? If so, was it because of earlier diagnosis? He had only had two cases of meningitis in which the patients had recovered when there had been organisms in the cerebrospinal fluid, and both these patients had been treated by repeated lumbar puncture. He had studied all types of operation for the condition and had tried drainage of the basal cisternæ, but without success.

He was inclined to think that by the method of washing out from below upwards described by Mr. Jenkins, it was possible to diffuse infection—originally localized to one cistern—throughout the whole system, into the Sylvian fissure and on to the vertex. In these cases he (the speaker) had washed from the lateral ventricle to the lumbar puncture route.

When translabyrinthine drainage had been carried out and there was a copious escape of fluid, it was important to pump into the cerebrospinal system a sufficient quantity of fluid, otherwise the whole space would become dry, and would probably be infected from the ependyma. The practice of washing out the abdomen had been abandoned. In an established case of diffuse peritonitis the hope of recovery depended on draining at several points. One did not hesitate to remove fluid from the pleural cavity in cases of severe pleurisy when the lung was impeded. In the case of the ear, might not hope of recovery depend upon repeated lumbar punctures?

He agreed with Mr. Layton that so-called "meningism" was really early meningitis but did not agree with him regarding lumbar puncture, though Weed, of the Rockefeller Institute, had shown experimentally that this procedure might cause an infection of the meninges by osmosis. During the last three weeks he (the speaker) had had three cases of this class; two patients had recovered, the other was still in bed. Both the patients who recovered had violent headache and a little retraction, with an acute exacerbation of chronic middle-ear suppuration. On lumbar puncture, the fluid in all three cases was under such pressure that it shot across the room. He drained it drop by drop. The fluid was sterile, and there was no increase in the cells.

Apart from lumbar puncture, in cases of meningitis, one had no means of assessing the treatment, or of knowing how far the meningitis had progressed.

Dr. Dan McKenzie asked whether Mr. Layton had ever blamed himself for having performed lumbar puncture in a case that looked like a threatening meningitis.

With regard to diagnosis, he (the speaker) had been in the unfortunate position in the last year of having operated upon two cases in which the condition was supposed to be meningitis, but which he finally concluded were cases of encephalitis lethargica. No allusion had so far been made to that possible mistake. In most cases of suspected meningitis a lightning diagnosis was necessary. There was the patient who had undergone the mastoid operation for otitis media, followed a day or two later by a temperature higher than the previous ranges, accompanied by headache and definite neck rigidity. One thought at once of meningitis and proceeded to treat accordingly. In the cases mentioned he had found that the cerebrospinal fluid was turbid, and he had drained: he did not dare to wait some hours for the microscopical examination. The report when it came was that there were no leucocytes and no bacteria, but that there was a lymphocytosis, and it was that lymphocytosis which distinguished encephalitis lethargica from septic meningitis. These cases, in both of which the patients had died, might constitute a warning to other surgeons.

Concerning treatment, during many years' experience he had found that cases varied a great deal, as did the result of treatment. During the last few years he had encountered case after case of failure, failure when drainage was carried out, and failure when he had tried to follow Mr. Jenkins' method of washing through the meninges from the other side or from the spine. When drainage took place the clinical picture of the disease was profoundly modified. The patient remained conscious, frequently until the end, but he still suffered pain and headache, the latter in recurrent and often very severe attacks. He (the speaker) questioned, indeed, whether the condition was better after drainage than it would have been without it. A year or two ago, in a paper in the *Journal of Laryngology* it was maintained that the best treatment of meningitis was to remove the primary focus and to leave the meninges alone. That statement agreed with the opinion forming in his own mind, that drainage was by no means a treatment upon which reliance could be placed for the cure of meningitis.

Mr. M. Vlasto said that, in his opinion, speakers were not taking into sufficient account the type of labyrinthitis which might ultimately spread to the meninges. There were, broadly speaking, two main forms, the circumscribed and the diffuse. In the first type the rapid component of the nystagmus was usually directed towards the affected side, whereas, in the diffuse form, in which the labyrinth might be assumed to be "dead," the nystagmus was always directed towards the sound side.

Recently he had had a case of mastoid disease, with severe infection of the labyrinth, in which the nystagmus was violent and directed away from the diseased ear. Here was a case in which drainage of the labyrinth appeared to be indicated. For one reason or another he had allowed the disease to run its course, with ample drainage provided for the middle ear tract. The patient had made a complete recovery, with total loss of hearing on the affected side.

He had recently discussed the question of labyrinthine drainage with an otologist from Vienna, who said that better results were obtained from conservative measures than from the interference suggested by the Neumann school.

Mr. James Harper said that the question of operative interference in meningitis had caused him much anxiety. Many cases were on the border line and one feared to interfere with the meninges.

This year his department had dealt with fourteen cases of meningitis, and in seven of these recovery had followed the clearing out of the local infection in the ear. As regarded the other seven cases, in which the patients had died, he did not think that any form of washing out or draining of the meninges would have made any difference. Three of the patients had been unconscious when admitted to hospital, two in a state of complete coma. One of the patients who had recovered, had had a mastoid operation performed. The course had been satisfactory for a few days, and then the symptoms of meningitis returned. The mastoid was again examined and a few infected cells were found which had been overlooked at the previous operation. The meningeal symptoms cleared up and the patient did well. Meningitis was diagnosed from the usual clinical symptoms. Lumbar puncture was carried out, and so far no harm had been traced to it. In the cases which had recovered, no micro-

organisms had been found in the cerebrospinal fluid, except in one case in which a streptococcus was present, though it could not be cultured. In the seven cases which ended fatally micro-organisms had been found in the cerebrospinal fluid, and in six of these the pathologist was able to culture them. He thought that meningitis could be divided into two stages: one in which micro-organisms were not found in the cerebrospinal fluid, and the other in which such organisms were present. In the first stage there was a possibility of recovery; in the second stage this possibility was very remote.

Mr. Broughton Barnes said he had had only two cases of the condition in which recovery had taken place when there had been infection of the cerebrospinal fluid, and in both these there had been an infection of the brain; one was a case of cerebellar abscess, the other of suppurative encephalitis in the temporo-sphenoidal lobe. In both there had been streptococci in the cerebrospinal fluid removed by lumbar puncture. The first case had also a Gram-negative bacillus. He had only dealt with the brain condition. As a result of that experience, he thought one should be on the look-out for brain infection, and if such infection was present he would not wash out or drain the meninges by operation, but would deal only with the brain. The prognosis of meningitis, if there was brain infection as well, was better than if no brain infection was present.

Mr. Sydney Scott said there must be a consensus of opinion that in combating meningitis, the seat of local infection in the region of the middle ear should be thoroughly dealt with first. From that, some might infer that a radical operation should be done, but this was not necessary in acute otitis media. In cases of meningitis patients recovered after Schwartze's operation and with perfect hearing. He recalled a case which he believed had some bearing on Mr. Layton's experience. The patient had had meningitis quite early in the course of acute otitis media. He had the usual signs—rigidity of the neck, fixed pupils, absence of abdominal and tendon reflexes, intense headache and frequent vomiting. He (the speaker) had opened the mastoid (by Schwartze's method) and had punctured the lumbar theca at once. Dr. Harold Pritchard had found streptococci visible in the centrifugalized cerebrospinal fluid, as well as in a platinum loop of fluid-exudate in the mastoid antrum. Curiously enough, the apical and all superficial mastoid cells were absolutely normal, and he might have stopped the operation had he been uncertain about the diagnosis. The patient recovered after repeated lumbar punctures, without the injection of any agents into the theca or veins.

In another class of case, Mr. Jenkins had laid stress on the surgical principle of draining the meninges in the region of the original infection. He (Mr. Scott) would like to hear more about the normal flow of cerebrospinal fluid. It was understood that the fluid was secreted by the ependyma and passed from the ventricles to the base and surface of the brain and posterior fossa, entering the systemic circulation through the venous sinuses and the cranial and spinal nerve sheaths. But it was physically possible to inject fluid in the reverse direction from the lumbar theca to the ventricles. Many years ago he had obtained a gelatine cast of the lateral ventricles in this way by injecting a saturated solution of gelatine and urea into the lumbar theca. He had not injected fluids therapeutically, as he himself believed in withdrawing fluid by lumbar puncture.

He agreed with Mr. Jenkins that the ideal site of drainage was local, if that could be established, as in the translabyrinthine operation, when the labyrinth was already infected.

In answer to Mr. Watkyn-Thomas, he recommended inferior vestibulotomy rather than Neumann's operation, when translabyrinthine drainage was intended, for one could then obtain a landmark, namely, the bi-meatal trans-cephalic line straight along the external auditory meatus, below the seventh nerve into the internal auditory meatus.

Patients who recovered were in his experience those in which, after early operation, there was a free escape of cerebrospinal fluid, either by direct drainage or by repeated lumbar puncture. When drainage was inadequate owing to swelling of the pia arachnoid with presence of lymph, the patients died. He had never succeeded in obtaining adequate drainage by merely incising the dura mater.

Mr. Vlasto had asked whether one should open a "dead" labyrinth in operating for acute otitis media. Personally he (Mr. Scott) would do so, but he would first make sure as to whether the labyrinth was alive or not by spraying ethyl chloride directly on to it while the patient was under the influence of the general anæsthetic. General anæsthesia did not prevent deviation of the eyeballs, though it prevented nystagmus even when the labyrinth was alive. By this means the great mistake of opening a live labyrinth could be avoided.

Mr. Musgrave Woodman said that with regard to the advisability of lumbar puncture, there was a *via media*. As a general principle, of course, one should not drain the lowest part of the cerebrospinal cavity and draw fluid from the infected area. On the other hand, lumbar puncture was distinctly valuable in certain cases. He (the speaker) had had a similar case to that quoted by Dr. Dan McKenzie. The patient had meningitis, clearly of mastoid origin, and was unconscious. Lumbar puncture had been done and the fluid had been somewhat turbid. He carried out a mastoid operation and translabyrinthine drainage, which seemed to have been successful, but a few hours later the patient died. He was informed that the cerebrospinal fluid was packed with hæmolytic streptococci; had he known that at the time he would not have done the translabyrinthine operation. The important point was that there was a great difference between drawing off a large quantity of fluid by way of treatment, and drawing off 1 c.c. merely for diagnosis. Withdrawing this small quantity and having it examined bacteriologically, chemically, and microscopically would obviate many a false diagnosis and frequent confusion with encephalitis lethargica; it would also indicate whether the case should be treated surgically.

Mr. Holt Diggle said that with regard to diffuse meningitis, successful treatment in the future would depend not only on drainage but also on the osmotic effect of drugs put into the circulation. During the last two years he had been experimenting on lines similar to that of treating parasyphilis by salvarsan and drainage of the theca. He had tried urotropine and various preparations of arsênic, as yet without success, but he believed that this method offered a field for further exploration.

Mr. H. V. Forster said that the problem was whether one would do too much or too little. Had Mr. Jenkins been able to effect recovery in cases of pneumococcal meningitis? In his (the speaker's) first case of meningitis the patient had recovered after the focus had been removed by a mastoid operation. In another case in which the patient had been moribund, he had drained through the internal auditory meatus and performed lumbar puncture, a procedure which at least had enabled him to bring the distressed parents to a fully conscious child. This patient, however, had died about ten days afterwards. In a later case the patient had also been moribund, and he had performed a radical mastoid operation, but had only uncapped the promontory. Recovery had followed. In this case the cerebrospinal fluid had been exceedingly turbid. He had had difficulty in ascertaining if the labyrinth was dead, but he would remember to employ the test described by Mr. Scott, which could be applied to the anæsthetized patient. In this connexion one of Mr. Layton's cases had particularly interested him, because he (Mr. Layton) had naturally been anxious as to whether he would make this patient's condition worse by exploring for cerebral abscess. Dr. Dan McKenzie had spoken of Mygind's paper on meningitis. Without wishing to stray from the main subject under discussion, he might mention another paper by a writer from Copenhagen, Robert Lund, who laid stress on the value of exploration for brain abscess by needle puncture, draining by a very fine tube. On one occasion he (Mr. Forster) had been fortunate in seeing two cases of temporo-sphenoidal abscess within twenty-four hours, and, following Lund's conservative technique, he was delighted to find that both patients recovered. In one of these cases 2½ oz. of pus were drawn off. He had inserted the finest rubber catheter as a drain.

As an example of the case in which one feared doing too much he would quote the following: A fortnight ago he had been called to see a child in convulsions. The parts affected were the left side of the face, left arm and left leg. There had been discharge from the right ear for three weeks, and there was a suspicion of nephritis. What was he to do? Was one to shelter under the diagnosis of uræmia and take no action, or should one deal with the right ear immediately and explore for brain abscess? He decided to operate. The mastoid on the right side was full of pus, the dura of middle and posterior fossæ was normal. He punctured the temporo-sphenoidal lobe in three places, using a needle and syringe. He also punctured the cerebellum in front of and behind the lateral sinus, but nothing came away. On lumbar puncture he found clear fluid under pressure. He decided that there must be an abscess somewhere and that the needle puncture had failed. This, however, was not the case; the child made a perfect recovery. Before the anæsthetic was given the convulsions had ceased, giving place to paralysis down the left side. Later, the presence of nephritis was confirmed.

Sir James Dundas-Grant said that a form of treatment had been initiated by Maloens, of Brussels, in view of the rapidity with which any antiseptic injected through the spinal theca by lumbar puncture was at once eliminated by the kidneys. The method was to perform lumbar puncture and then inject the antiseptic, say, septicémine, into the veins.

In meningitis the choroid plexus (gland) was affected in such a way that the direction of the flow was reversed. The principle was to introduce such anæsthetic into the arachnoid spaces, and more could be got there by injecting through the vein, strange though that might appear.

Dr. T. B. Jobson said that some years ago he had had a case which threw some light on pneumococcal problems. It was that of a middle-aged man with chronic otorrhœa. He was brought into hospital unconscious, and having all the classical signs of meningitis. He (the speaker) did a lumbar puncture, and found the fluid milky. The count showed 3,400 cells, and the culture was a pure growth of pneumococcus. A complete radical mastoid operation was carried out, and a perisinus abscess was discovered, No further operation was done, but large doses of anti-pneumococcic serum were administered and the patient made a complete recovery.

Remembering such a case, one hesitated to do a more extensive operation. Only accurate statistics over a wide field would enable one to decide whether to open the labyrinth or not. The relation of a few cases by different operators did not afford much guidance, as there might have been special circumstances in connexion with each of them.

Mr. E. Watson-Williams said he had had experience of the method which Sir James Dundas-Grant mentioned.[1]

Mr. G. J. Jenkins (in reply) said that many otologists could recall cases of infective meningitis in which the patients had recovered after minor procedures such as lumbar puncture. He protested against being content with these occasional almost spontaneous recoveries, and regarding all other cases as hopeless and necessarily fatal.

Draining from the region of maximum infection must be a sound procedure and washing away infected material, if safely done, would help still further.

In his paper read in 1922 he had dealt with the obvious difficulty of deciding whether there was an invasion of the subarachnoid space or not. This could not be settled at the time of operation, and action must be determined on clinical experience and valuation of the results of examination of the cerebrospinal fluid.

In the same paper he had endeavoured to show that the symptoms and signs of early meningitis largely depended on the track of infection from the ear. In the later stages of the disease the symptoms and signs were more or less similar, whatever the course of infection. The difference in symptoms of onset depended largely on whether the infection was primarily of the cisterna or of the trabeculated subarachnoid space.

In reply to Mr. Davis, he had never used the cisterna magna as the point for draining the subarachnoid space or for introducing fluid. The aim of all operative procedures should be to drain from the point of maximum infection and encourage the healthy fluid to flow over that region. Any operative measure on the cisterna magna would interfere with this principle.

The post-mortem appearances in a case of septic meningitis were apt to be discouraging. It must be remembered, however, that in the early stage the meningitis must be localized, if only for a very short time, though the region affected might not have been a definite boundary. It was in this early stage that there seemed to be a probability of rendering assistance.

The superior labyrinthotomy (partial) did not add much to the operative severity and it helped to clean out that part of the labyrinth, through the large inferior labyrinthotomy wound, more thoroughly.

Statistics were of no value, as yet, because of the difficulty of classifying cases so that a comparison might be made. He had no doubt that in a few cases with extreme symptoms and signs the patients would have recovered without operative treatment, but the majority of cases with slight symptoms and signs went on to fatal termination. Operating in the early stage might be an unnecessary procedure in some very few cases, but he (the speaker) thought it would ensure the recovery of some cases that would otherwise fail to resist the infection.

[1] See account of a case of Dr. P. Watson-Williams, *Proc. Roy. Soc. Med.*, 1920, xiv (Sect. Otol.), 74.

Bacteriology did not help us in a decision on this point, since either severe or slight conditions might be found when streptococci or pneumococci were present. Again, it was often impossible to find an organism in the cerebrospinal fluid in a case in which the patient was obviously dying from meningitis even only a few hours before death.

Mr. T. B. Layton (in reply) said that there had been recounted in the discussion cases in which recovery had taken place, but, in which, by all the rules of surgery, the patient should have died. He himself was not a "lucky" surgeon and such cases did not come his way. Treatment must not be based on such cases as these, but on the general run of cases, and he believed that the less he did, in these early cases, the better were the results that he obtained.

In answer to Dr. Dan McKenzie, he remembered a rhinological case in which the patient might have had a greater chance of recovery if lumbar puncture had not been repeatedly carried out, or would have died in relative comfort in a day or two instead of having her agony prolonged over a period of three weeks. Dr. McKenzie had, however, answered his own question, when he had said that the principle on which to act in these early cases was to treat the local infection and leave the meninges alone. It was his (the speaker's) contention that in all cases where there was a good chance of recovery the best operative procedure was to deal with the local infection and to leave the meninges alone; or, in other words, not to do a lumbar puncture. Certain speakers had suggested that a drop or two of fluid only should be removed: but if this were done in an early case the condition of the fluid at one end of the space would not be necessarily an indication of its condition at the other end.

With regard to the late type of case, he agreed with Mr. Jenkins that one would not ensure recovery merely by removing the infection. He had never had a case in which recovery had taken place when organisms had been found in the cerebrospinal fluid on direct examination, or subsequently cultivated by the bacteriologist.

Mr. Jenkins' treatment was on the right principles in endeavouring to wash out the infecting material locally, instead of sucking it down to contaminate the rest of the cerebrospinal fluid. In that type of case lumbar puncture was necessary, and the examination of the fluid might afford some guidance as to what ought to be done.

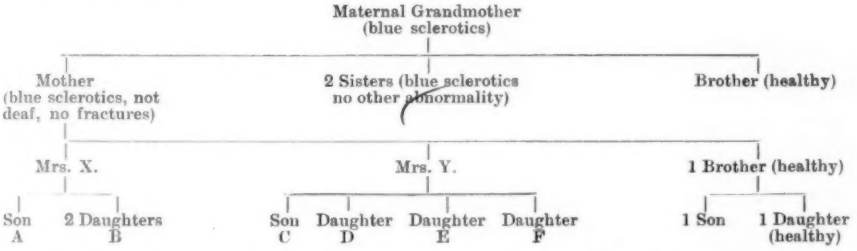
Mr. E. D. D. Davis (in reply) said he was in sympathy with Mr. Jenkins; the surgeon should try to do something for the cases described. Better results would be ensured if the diagnosis was made early. An early diagnosis often depended on obtaining 5 c.c. of cerebrospinal fluid by lumbar puncture for examination. In his experience, lumbar puncture had never done any harm.

CASES AND SPECIMENS.

Lasting Artificial Perforation.—Sir JAMES DUNDAS-GRANT, K.B.E., M.D.—Patient, female, aged 30, suffering from otosclerosis with high degree of deafness, distressing tinnitus and sense of fullness in head. Myringotomy by means of crescentic incision between the umbo- and the antero-inferior margin of the tympanum was performed, first on left side, then on right; operation on left side produced great amelioration of the subjective discomfort. On this side there is still a small pin-hole perforation, but on the right side there is an oval perforation of considerable size through which the promontory and niche of the round window have been visible since May, 1928. Hearing for high-pitched tones of Galton's whistle is well preserved. Compression of air in right external meatus causes extreme vertigo with falling to the left.

The site of the incision was that recommended by Schirmunsky (*Journ. Laryng. and Otol.*, 1927, xlii, 779) and was in the least vascular part of the membrane as shown by Luscher (*Journ. Laryng. and Otol.*, 1928, xliii, 891). The perforation on the right side remains large and shows no sign of diminishing. The pin-point perforation on the left side has, however, closed up.

Family with Blue Sclerotics, Fragilitas Ossium and Otosclerosis.—Sir JAMES DUNDAS-GRANT, K.B.E., M.D., F. C. ORMEROD, F.R.C.S., and F. A. H. SIMMONDS, M.B.



Mrs. X., aged 45, attended Brompton Hospital on account of her chest, and was observed by Dr. Simmons to have typical blue sclerotics. On further investigation, prevalence of the condition in the family, as shown in the accompanying tree, was ascertained. Patient is noticeably deaf; the deafness, however, was only perceived after an attack of influenza in 1921; hers is a typical case of otosclerosis. She fractured three ribs at the age of 10.

Her son (A), aged 18, has extreme deafness which developed at puberty; he also has interstitial keratitis; the Wassermann reaction, tested in January, 1928, is mildly positive. He has sustained numerous fractures. His sclerotics are blue and suffused with congestion of the conjunctiva.

Two daughters (B) are alive and well, aged 14 and 12; they have blue sclerotics. (A second child—male—died at the age of 6 months, said "to have had only one lung.")

A sister (Mrs. Y.), aged 32, has definite blue sclerotics; no fractures, but otosclerosis developed in 1918 after influenza.

Her children, four in number, have blue sclerotics but at present show no signs of otosclerosis, hearing the tuning-forks as low as sixteen vibrations. The eldest (C), aged 10, has broken his collar-bone, right leg three times, right arm three times, left arm once; the second (D), aged 8, has had no fractures; the third (E), aged 4, falls easily, has fractured right leg three times; the youngest (F), aged 2, has had no fractures yet and is a very strong child.

The blue sclerotics increase in depth of colour with any illness, in all the last two generations. Several members of the family have rather poor vision. In no case was there any fracture at birth, and no fractures occurred until the children began to crawl about, etc. Blueness of sclerotics was present at birth and does not seem to have increased in depth with age.

Discussion.—Sir JAMES DUNDAS-GRANT said that Dr. Adam, at an Edinburgh meeting, had reported another such family.

Mr. SOMERVILLE HASTINGS (the President) said that Mr. Cleminson had also shown a similar family.

Mr. G. J. JENKINS said that the association of blue sclerotics and fragilitas ossium with otosclerosis was well established, but the otosclerosis was a separate disease grafted on the others which apparently favoured its incidence.

Carcinoma of Middle Ear, apparently secondary to Carcinoma Mammæ.—W. M. MOLLISON, M.Ch.—Miss R., aged 59, referred by Dr. Charsley, in September, 1928. Right breast removed for carcinoma three years previously; no local recurrence. Eighteen months ago attack of neuralgia in left side of face;

the eye twitched and facial paralysis followed; this decreased under electrical treatment, but nine months ago it was again evident and has continued ever since. Five months ago patient was seen at a hospital for nervous disease, but no cause was found for the paralysis.

Six weeks ago a small amount of blood came from the left ear and this has occurred again since. Fourteen days ago pain was felt down the neck. Actually there has been pain in the left ear now and then for three months past, much worse lately.

On examination granulations were seen deep in the left meatus. They bled readily on being touched with a probe; there was a little pus. The mastoid process was tender, but the whole of the left side of the head was also tender. There was complete facial paralysis (left).

Examination of scrapings from the ear showed carcinoma (Dr. Nicholson).

X-ray examination of the mastoid did not show any thickening of the bone.

On account of the pain a radical mastoid operation was performed October 25, 1928. The mastoid was cellular; when it was opened some clear fluid escaped, and there was some doubt as to whether this might have been cerebrospinal fluid. The antrum showed slight granulation; the bone overhanging it seemed infiltrated; the bridge certainly seemed invaded; the incus appeared only slightly affected, but the malleus had attached to it a piece of tissue which resembled new growth, and proved to be such on section. The dura mater above the attic was exposed and far forward showed a layer of growth infiltrating it.

A flap was cut; as far as could be seen, the skin of the meatus was not the site of the growth.

Sections of (1) the piece of bone from the bridge, and of (2) the granulation attached to the malleus both showed carcinoma.

The patient has been treated by some exposures to deep X-rays and this caused cessation of pain.

Case which Simulated Lateral Sinus Thrombosis seven days after Operation for Chronic Mastoiditis.—H. NORMAN BARNETT, F.R.C.S.—D. B., female, aged 22, admitted to hospital September 26, 1928, suffering from chronic discharge from right ear, of long duration. Operation performed on morning of September 28, 1928. Bone, cellular in type, extensively diseased. Lateral sinus uncovered, owing to abnormal position, but not injured. Wound drained with gauze, from lowest point; remainder being sutured and dressed with hot saline. Stitches removed three days later and case ran normal course till seventh day when patient had a rigor, temperature rising to 104° F. and pulse to 92. Two rigors on following day; temperature 102° and 103° F. Wound reopened in the evening and lateral sinus carefully examined: it appeared to be quite normal, and there was full pulsation. Jugular vein seemed normal: nothing could be felt along its course.

Careful general examination to find if there was any other cause for rigor. Apex of right lung found to be affected, being dull on percussion; breath sounds abnormal.

Next day patient was much better; temperature fell to normal but on following day rose to 103° F. and there was another rigor. For several days temperature kept up, but patient was fairly well. Temperature became normal on October 12, and was normal in morning of October 13; in the evening it rose to 105° F., and there was a rigor. Patient after this progressed well, with varying but not high temperature for twelve days; no further rigors.

October 27, 1928.—Swelling appeared on right side above and below the clavicle. This gradually spread to shoulder, arm, and interspaces of upper ribs. Little or no pain. Meanwhile, there was a profuse discharge of greenish-yellow pus from the ear. This was found to contain streptococci in short chains of very profuse growth.

November 1, 1928, sudden attack of acute pain in throat, with cough; right tonsil swollen. Patient began to expectorate foul yellow-green pus and complained of great sickness. Swelling and discharge decreased on November 2. On November 4, only a little brownish discharge from ear. During afternoon of that day patient vomited twice, on both occasions blood was present in the vomited matter. Pain in ear with discharge of blood; pain in neck; pulse rapid and weak. Pus and blood in nasopharynx. Condition gradually became worse for next two days, leading to fatal termination on November 6, 1928.

This case is somewhat similar to one which I reported earlier in the year, but the infection was not so general and the result not so satisfactory. In both cases there was a possible tuberculous family history.

The present case is a notable example of the need for exploration of the mastoid in cases in which there is a long standing history of discharge from the middle ear but of no other symptoms usually said to be associated with mastoiditis. Here there was found extensive bone disease, which could have been prevented by an early operation, and a higher resisting power of the patient would have been ensured.

Blood examination showed no growth. Pus from the ear contained profuse growth of streptococci in short chains.

The main question is as to the nature and cause of the swelling of chest and arm, and as to whether there was any direct infection or infection through the blood-stream. I incline to think that there was an abscess of the anterior mediastinum, which was caused by blood—or by direct—infection and to which the patient was predisposed by the previous lung trouble. In these circumstances, would operation have been justified, and if so, what operation? The ordinary procedure for anterior mediastinitis, or some other operation? Unfortunately no post-mortem examination could be obtained.

The charts show the first rigor on the evening of the seventh day after operation, followed by four more at intervals of one day, one day, two days, and six days respectively, and no rigor for the next twenty-four days, and a general improvement in the condition till the last few days. Why was this, in view of the final result? The four-hourly charts covering the period from the first rigor are also shown, though I have not been able to deduce anything from them except that they bear some resemblance to those of lateral sinus thrombosis.

Primary Carcinoma of Glands of Neck: Symptoms suggesting Primary Focus in Ear. (*Case shown for diagnosis.*)—LESLIE POWELL, F.R.C.S.—Miss T., aged 63, first seen November 14, 1928. Eight weeks previously had "a cold" (not subject to colds) with pain and deafness in the right ear, followed after a few days by discharge from ear, without relief of pain, which was very bad at night. The tongue and mouth on the same side seemed "dead," and there was a large swelling on the right side of the neck.

On examination, the right ear was found to be full of thick pus and when this was swabbed away a mass of hard granulations, which were very tender and bled easily, was seen on the floor of the meatus. A watch could not be heard even on contact. The right tonsil was inflamed and an abscess was opened by the cautery, but this abscess was not enough to account for the symptoms.

November 22, 1928: Seen again; feeling better; glands now discrete, but firm and fixed behind the right sterno-mastoid.

November 29, 1928: Pain severe again; hearing better; glands smaller: throat normal.

December 7, 1928: Complete paralysis of right side of palate and tongue and right vocal cord. Difficulty in swallowing solids, but not in swallowing liquids.

Discussion.—THE PRESIDENT said that the paralysis might be due to an ingrowth of the malignant disease through the walls of the skull into the cranial cavity. He suggested that a small part of the granulation tissue should be removed for microscopical examination; he had never seen a similar case.

Mr. H. V. FORSTER said that one could see the choana and the Eustachian orifice on the left side, but not on the right, for alteration had occurred on that side possibly because some swelling was pushing the Eustachian cushion inwards. He thought the trouble was affecting the base of the skull. A patient who had been treated for some time for lymphadenoma had been sent to him. She had right-sided deafness and right trifacial neuralgia, and in her case he found that the Eustachian cushion was being pushed inwards. She had died shortly afterwards.

Postscript.—Primary carcinoma of glands of neck found post mortem.—L. P.

Section of Electro-Therapeutics.

[*January* 18, 1929.]

Some Factors Influencing Epiphyseal Growth and Union.

By R. S. Paterson, M.D.

Some time ago I investigated 1,000 cases of normal adolescents with a view to tabulating the dates of appearance and fusion of the epiphyses. I now wish to deal with certain facts which became evident, and to draw some conclusions that seem justified from these and subsequent cases which I have seen.

I will discuss first the epiphyseal centres found at the upper and lower ends of the humerus. Great diversity of opinion exists as to the time of appearance of these epiphyses, and as to their number. Gray's "Anatomy" gives two centres—one for the head and one for the greater tuberosity, but quotes Béclard as stating that there is an additional centre for the lesser tuberosity. Morris's "Anatomy" and Cunningham's "Anatomy" both give three centres. Choyce gives three centres, which appear at 6 years and join at 25 years. Roberts and Kelly also mention three. Cohn, in a series examined radiologically, came to the conclusion that there was only one epiphysis for the upper end of the humerus, and that the two tuberosities were ossified as downgrowths from this centre, much in the manner that the tuberosity of the tibia is ossified. He quotes Poland and Stimson as supporting this view. I found that three of my cases showed unmistakably separate epiphyses for the head and the greater tuberosity. This led me to collect further cases, and ultimately I found that about 60 per cent. of them showed this separate centre, but that in the normal position in which radiograms of the shoulder are taken the shadow of this epiphysis is overlapped by that of the head, and appears very much like a downgrowth from it. Radiograms taken at different angles, and including one with the arm fully extended above the head, may show that it is completely separate. It appears at the end of the second and the beginning of the third year, and joins the epiphysis for the head about the fifth year. The remaining 50 per cent. of cases showed no separate centre for the tuberosity, in which case it appears to ossify by extension from the head. In no case was there a single instance of a separate epiphysis for the lesser tuberosity; this points to the view that ordinarily there is no such separate centre, and that it cannot be regarded as the normal method of ossification.

It would appear, therefore, that the upper end of the humerus ossifies in about 50 per cent. of cases from a single centre for the head, and in the remaining 50 per cent. from two centres, one for the head and one for the greater tuberosity. The centre for the head is either present at birth or appears within the first few months of life. The centre for the greater tuberosity, when present, appears at the end of the second or beginning of the third year. At about this time the lesser tuberosity is ossified as a downgrowth from the head and both centres become fused at the end of the fifth year.

The lower end of the humerus is usually described as having four centres of ossification—one each for the capitellum, the trochlea, the internal condyle and the external condyle. The epiphysis for the external condyle, however, does not seem to be invariably present. Cohn has stated that its appearance is the exception rather than the rule. I cannot agree with this as I found it present as a separate epiphysis in 70 per cent. of the cases. In the remaining 30 per cent. it appears to be formed as an extension of the capitellum. When present, the date of its appearance is very varied, but usually it occurs at the age of fourteen. I found it, however, in several cases in the tenth year. It joins the shaft two or three years

after it has appeared. In some cases its shadow is so near the outer margin of the capitellum that it is difficult to see, and in some instances it appears to fuse with the capitellum. More frequently it maintains a separate existence until it joins the shaft at the age of seventeen or eighteen.

For the moment I will deal no further with individual epiphyses, but will point out certain factors which influence the date of their appearance and fusion. The first and most important of these is sex. By separating the cases of each sex a very persistent and definite difference in the times of appearance and fusion is evident, and for some reason which I cannot explain the difference is much more marked in the epiphyses of the elbow-joint than in any other bones of the body. In girls all the epiphyses of the elbow-joint are invariably fused at the age of 15, whereas in boys fusion is rarely complete before the end of the nineteenth or beginning of the twentieth year. Investigation showed that elsewhere there was a sex difference of one to two years in the time of fusion of the epiphyses. This has been recognized before in general principle, but it seems so marked that its importance is very great in any estimation of age from the epiphyses; also if the sexes are separated the average dates of the appearances and fusions of the epiphyses are more nearly constant than appears from many of the anatomy text-books, where figures have presumably been arrived at from series of cases of both sexes. Adair and Scammon have shown that the same relationship holds in the intra-uterine ossification of the primary centres, and that in the fœtus ossification proceeds more rapidly in females, even though their weight and dimensions are less than those of the males. The older the individual the more marked is the difference between the two sexes as regards their epiphyses, so that the disparity in the dates of fusion is greater than that of the dates of appearance. As Pryor expresses it, "the bones of the female ossify in advance of the male. This is measured at first by days, then months, then years."

The influence of the endocrines on growth is well known, especially that of the anterior lobe of the pituitary and the thyroid, and the inter-relation of the endocrines is being worked out. There is no doubt that the sex glands can affect the thyroid, and their connexion is witnessed by the physiological enlargement of the thyroid at puberty, marriage and pregnancy. Dixon, in 1923, showed that injection of ovarian extract stimulated the action of the anterior lobe of the pituitary. It has been shown that castration delays ossification, even if it does not inhibit it. Puberty in both sexes has a very pronounced effect on ossification and announces one of the most active phases of the process. As puberty and sexual maturity occur earlier in the female, and as ossification is associated with both these events, there is no anomaly in the earlier ossific changes in the female. Lipschutz quotes several cases of sexual precocity which showed complete fusion of the epiphyses at an abnormally early age. All his cases were associated with endocrine disturbances. I have seen three cases of sexual precocity as evidenced by enlargement of the breast, menstruation, the growth of pubic and axillary hair, etc., one at the age of twelve, and another at fourteen. In one of them menstruation commenced at the age of eleven and the other signs preceded it. In this case there was hyperthyroidism at the age of two; this was treated radiologically and presented some very interesting endocrine disturbances. The relevant feature here, however, is that, associated with the early sexual maturity, there was early epiphyseal union. At the age of twelve most of the epiphyses were joined and the others were almost fused. The other case was less marked, showed some sexual precocity and an enlarged pituitary fossa. Here also epiphyseal growth was in advance of the normal.

The third case was that of a boy of sixteen whose mother had sought advice, as, although he was normal, he was not as tall as she thought he should be. Mentally he was quite normal, though not brilliant; he was thick-set, short, had a deep voice, and had been shaving for some time. He had the appearance and the manner of an

adult of 22 to 24. On examining his epiphyses I found complete fusion in all the long bones, that is about four years earlier than normal.

In all these cases of accelerated epiphyseal union, therefore, there was an associated accelerated sexual maturity, and such disturbances of the sexual or other endocrine glands, are the commonest causes of early union.

Shinz, Baensch and Friedl have examined the dates of epiphyseal union in various classes of society, and found it earlier in the children of peasants and artisans than in the children of more aristocratic parentage. In a series of hospital cases at the Manchester Royal Infirmary, consisting mostly of adolescents employed in manual labour, the average date of fusion of the epiphyses was distinctly below that quoted in the standard text-books, though whether this was really due to their occupation or whether the dates in the text-books were wrong I am not sure. On general principles one would imagine that development would be more rapid in manual workers. Unfortunately I have no opportunity for following up this interesting investigation.

Two other factors said to influence epiphyseal growth and union are climate and race. It is stated that in tropical climates and in primitive races epiphyseal growth is more rapid. Here, again, I am unable to confirm or disprove this statement; I am inclined to think, however, that the early epiphyseal union is secondary to the early sexual maturity which tropical races exhibit, and that it is not due to direct external influences.

In order to investigate further the times of epiphyseal union in various types of adolescents I have examined a number of boys from the Manchester Grammar School. Dr. A. A. Mumford, the medical officer of the school for twenty years, has made a study of the different types of boys and their physical measurements, and he kindly sent me various physical types for X-ray examination of their epiphyses.

By means of a study of the large numbers of boys that have passed through his hands, Dr. Mumford has compiled tables of average measurements, of height, of length of limbs and their proportion to trunk measurements, of chest measurements and dentitions. These he has correlated with their mental ability and natural aptitude for various types of study. His analysis of the vast numbers of boys whom he has examined over this period is contained in his book entitled "Healthy Growth." It might appear that certain types of growth would reveal different epiphyseal changes, particularly the type in the boy who has "outgrown his strength," and who is unusually tall for his age; also the type that is physically precocious, often good at games, well developed and with the appearances of a boy older than his actual age. In contrast with this is the boy who is physically normal, but mentally precocious. All these types were examined and radiographed.

The first series examined contained those of excessive physical development. Taking the average height, weight and chest measurement for certain ages, the boys in these cases were two to three years ahead of normal in their height, two years ahead in their weight, and about three years ahead in their chest measurement. Their ages were from 12 to 14. In every case, however, on radiological examination normal epiphyses for their age were found, and there was no instance of excessive growth associated with early epiphyseal union.

The second category examined consisted of cryptorchids and boys who had had one testicle removed either as a result of trauma or of disease. Unfortunately there has been no case of double orchidectomy so far. The ages in this series varied from 12 to 14, and the orchidectomy had been performed five years or more before my examination. I had anticipated in this series that there would be some delay or alteration in the condition of the epiphyses, but in all these cases the epiphyses were quite normal for the age, and in no case was there any apparent retardation of epiphyseal growth.

A limited number of cases of congenital dwarfism were available for examination, the most striking being the case of a boy aged 16. The family is of Eastern extraction,

and, though physically and mentally normal, all the members are diminutive. The boy aged 16, whom I saw, had the physical appearance of a child of 12, though his mental capacity corresponded with his actual age. The epiphyses of his long bones proved to be those of a youth of 16, and in spite of his dwarfish and under-developed appearance his epiphyseal age was normal.

Under the same type I may mention another case of two brothers, C. and H. Dr. Mumford's description of them is as follows: "C. is 14½ and H. is not yet 11. The whole family of seven brothers are very extraordinary in their precocious growth. The eldest one had mental trouble and destroyed himself. Two others I have had to send away to the seaside on account of threatened chest trouble. They all grow very rapidly, very early, and stop growing about 14 or 15. The eruption of H's teeth (i.e., the younger one) is about equal to his age, 10½-11, but his physical build is five or six years ahead of this; he is more precocious than C. None of the family possess normal distance vision and only one approximates to it. Radiograms of these two brothers are interesting. The elder one, although, from his family history he should not grow any more, shows normal epiphyses, and is, therefore, at any rate capable of further growth; but his younger brother, at the age of 10½, has the epiphyses of a boy of 16—a full five years in advance of what they should be. Actually, the radiograms of the two brothers, aged 10½ and 16 respectively, are almost identical." This is a very difficult case, but, although I have no proof, I imagine we are dealing with some upset of endocrine balance, which is apparently congenital. Mentally, both these brothers seemed normal except for the precocity of the younger one, which was to some extent mental as well as physical.

I have excluded from all these cases the known pathological conditions which affect epiphyseal growth, such as rickets, achondroplasia, scurvy, pseudo-coxalgia and diaphyseal aclasis, as I wish to draw attention to the non-pathological alterations in epiphyseal development. Unfortunately these cases are not very common, but from those which I have been able to investigate I draw the following conclusions:—

(1) That sex is an important factor in epiphyseal development, which is complete one to two years earlier in females in the long bones except the lower end of the humerus and upper end of the radius and ulna, in which it is complete four years in advance of the male.

(2) That the sexual glands play an important part in the control of epiphyseal growth.

(3) That any upset in the normal endocrine balance may profoundly affect the ossification.

(4) That excessive physical development and rapid growth in proportion to age, whether associated with mental precocity or not, are quite independent of epiphyseal growth, which remains unaffected.

(5) That certain types of congenital dwarfism exhibit normal epiphyseal growth for their age.

(6) That the removal of one testicle does not affect epiphyseal growth, provided that the other is normal.

In conclusion I may say that with the exception of my statement regarding the dates of union of the epiphyses round the elbow-joint, my deductions are made from a limited number of cases, but the comparative rarity of non-pathological disturbances of growth and the difficulty of collecting them, even in a densely populated area, must be my justification.

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A Radiographical Note on Injuries to the Distal Epiphyses of the Radius and Ulna.

By C. THURSTAN HOLLAND, D.L., F.R.C.S.

A LONG experience of X-ray work at both large general hospitals and at special hospitals for children has created an impression upon my mind that injuries to bone in childhood and early adolescence are very much more interesting radiographically, as well as clinically, than those which occur during adult life.

Apart altogether from the fact that during childhood we have to deal with a growing bone, which influences in no small degree the processes of repair after damage, a great point of interest is to be found in the growing ends of the long bones and their cartilaginous epiphyses. One finds injuries involving these epiphyses and growing ends which do not occur in adult life, and radiologically they present an interesting field for thought and investigation.

In the X-ray examination of children there are two most important, even if somewhat elementary, facts that must always be borne in mind. (1) Cartilaginous epiphyses are not shown by X-rays, owing to the extreme X-ray translucency of cartilage. (2) It is only when centres of ossification are present that any shadow can be shown, and then it is only the actual ossified centre and not the whole epiphysis which is seen. This second point is important, and is, I believe, often overlooked. We see a shadow at the end of a bone, and are apt to overlook the fact that it is merely the central opaque portion of a much larger structure, consisting of cartilage. In studying radiographs of epiphyseal injuries it is necessary always to try to visualize the epiphysis as a whole, and not merely concentrate attention upon the centre of ossification. I must also point out the somewhat elementary fact that in the case of epiphyses there are different times at which they begin to ossify and become X-ray visible, and that in the study of these injuries it is essential to know something about the ossification of any individual epiphysis under examination. Even elementary facts are sometimes overlooked, not only by the general practitioner, but even by orthopædic surgeons. Not so very long ago an orthopædic surgeon referred a case to me, with the request that I would inform him if the epiphyseal internal condyle of the humerus was displaced. The child was under 2 years of age. I had to report that the radiographs showed nothing abnormal, and, as diplomatically as I could, to draw attention to the ossifying age of this epiphysis, namely, the fifth year of life. Before this age it would be invisible, even to X-rays.

I will now exhibit a few normal radiographs indicating the ossification at the wrist-joint which will come under observation.

Slide 1. *Hand at birth.*—(*a*) No ossification of any of the eight carpal bones. (*b*) No ossification of phalangeal or metacarpal epiphyses. (*c*) No ossification of radial or ulnar epiphyses.

It is obvious, then, that at this age no injuries to any of these unossified cartilaginous elements could be shown by X-rays.

Slide 2. *Hand at* 2 *years to* 3 *years of age.*—There is considerable variability in the times of commencing ossification, both as regards the carpal bones as well as the epiphyses, but by the end of the second year or so the centre of the os magnum and unciform should be well advanced, and, as a rule, the epiphysis of the radius (the radial epiphysis is very variable). It may be visible as early as the sixth month of life, though commencing ossification is often delayed until the second year.

Slide 3. *At 6 years of age.*—The os magnum, unciform, cuneiform and semi-lunar should be visible: the radial epiphysis well formed, but that of the ulna still cartilaginous and not showing.

Slide 4. *At about the seventh year.*—Seven of the carpal bones are partially ossified—the remaining one, the pisiform, not appearing till the tenth year. The radial epiphysis is complete. The ulnar epiphysis has commenced ossification.

Slide 5.—By the twentieth year the carpal bones are complete in their ossification, and the epiphyses have definitely amalgamated with their shafts.

These radiological facts are of extreme importance and must be constantly borne in mind when we consider the injuries affecting (1) the distal radial epiphysis and (2) the distal ulnar epiphysis.

(1) *The Radial Epiphysis.*—Quain gives the ossifying age of this epiphysis as early in the second year of life. Poland says that towards the end of the second year the osseous nucleus of the lower epiphysis of the radius appears above the scaphoid facet. As I have already pointed out radiographically, this epiphysis frequently begins to ossify by the sixth month of life, and I would further point out that it is of no practical use to attempt to give any average time for the centres to appear, as it means little or nothing when applied to any individual case. What we require to know is that it may normally begin to ossify as early as the sixth month of life, and also quite normally as late as the end of the second year of life.

(2) *The Ulnar Epiphysis* ossifies much later: at any time from the fourth to the seventh year.

This is not a surgical paper and does not deal with treatment; it is merely a short note on the demonstration of these epiphyseal injuries in view of the light which has been thrown upon them by X-rays, i.e. it is mainly diagnostic. My statistics are obtained from the cases of children up to the age of 12 years only, although I shall refer to the epiphyses up to the time of their union to the diaphyses.

A careful survey of the monumental work of Poland published in book form in 1898[1] is instructive when compared with the X-ray knowledge of the present day. Poland reproduces a few radiographs said to show a backward displacement of the lower radial epiphysis—all taken from front to back. They are unconvincing, and for reasons which I shall discuss later, a diagnosis of this condition from this view even now is usually unreliable, and in those days was quite unreliable. Apart from these X-rays his material emanates from two sources: (1) Reports from cases in which the patients had died from other injuries; (2) reports from cases in which a clinical diagnosis (without an X-ray examination) had been made.

It is of interest to note that, as regards the first class, the histories of those who died from other injuries, and in whom at the post-mortem a displaced radial epiphysis was also found, gave rise to a curious and quite erroneous idea—that a backward displacement of the lower radial epiphysis was usually the result of a bad accident, such as a fall from a height.

Poland's statistics, therefore, and the statistics of others quoted in his book, are now merely of historical interest in respect to his material, and they cannot be accepted in any way as being accurate in view of accumulated X-ray experience.

Poland then quotes the following figures relating to his own cases:—

Upper extremity: Out of 426 epiphyseal injuries, 112 were those of the lower radial epiphysis; and out of 267 cases in the lower extremity 125 were injuries of the lower epiphysis of the femur. These figures give a completely wrong impression as to the frequency of these injuries, and I have no doubt that separation of the lower epiphysis of the radius is far more common than that of the lower end of the femur, although Poland saw more of the knee injury than of the wrist injury.

In the last seven years at the X-ray department of the Liverpool Children's Hospital, from a total of 10,991 cases examined, a definite X-ray diagnosis of injury to the lower epiphyses of the radius and ulna was made in ninety-five cases, sixty-nine males and twenty-six females. Of these, the injury was a backward

[1] Poland, John "Traumatic Separation of the Epiphyses," 1898.

displacement of the radial epiphysis in sixty-eight males and twenty-five females, and a forward displacement in one male and in one female. Both epiphyses were displaced backwards in one male and in one female. In thirteen cases there was a fractured ulna or ulnar styloid, in addition to the displacement of the radial epiphysis.

All these injuries occurred in children under 12 years of age.

Comparing these figures with other injuries within about 2 in. from the wrist-joint we find that this is the most common injury.

Out of 229 cases of injury we have the following:—

(*a*) Ninety-five cases of displaced radial epiphysis, in a few of which there was also a fracture of the ulna or ulnar styloid.

(*b*) Seventy-five cases of greenstick fracture of the radius alone or of both bones.

(*c*) Thirty-two cases of ordinary fracture of both bones.

(*d*) Twenty-three cases of fracture of the radius only.

(*e*) Four cases of fractured radius in addition to displaced ulnar epiphysis.

(*f*) No case of fractured ulna only.

These figures indicate very clearly the frequency of a displaced radial epiphysis, practically always in the backward direction, in young children.

A few remarks are here interpolated on the method of X-ray examination. In the large majority of the cases the usual palm-down view is of no value. Generally in the case of a badly displaced epiphysis an indication of the nature of the accident can be obtained by this view, but in the majority of slighter displacements the rule is that it furnishes no aid to the diagnosis; even if it does suggest an injury it never shows the extent of the displacement.

A side view—and it does not matter from which side, although practically always I use the ulnar side to film for convenience—is essential, and it alone will (1) justify the diagnosis, and (2) show the amount of displacement.

Stereoscopic radiographs in the palm-down position alone are quite unreliable and should never be depended upon. Additionally, if they do show the displaced epiphysis no reliance whatever can be placed on the supposed amount of displacement seen. I have proved this over and over again.

The correct way to examine these cases is by means of two views at right angles to one another: (1) Palm down, and (2) with the wrist placed laterally. Backward displacement of the radial epiphysis is the injury which is comparable in the child with the Colles's fracture of the adult, and is caused by exactly the same kind of trauma, namely a fall on the outstretched hand. It is usual, I find, for recently appointed house surgeons to send these cases for X-ray examination with a clinical diagnosis of Colles's fracture. Apparently students are not taught in the various schools that a *true* Colles's fracture before the age at which the epiphysis joins to the shaft is extremely rare; so rare that at one time I thought it never occurred. During the whole seven years we have had only one case of a true Colles's fracture under 12 years of age at the Children's Infirmary; and I have met with very few cases indeed of a true Colles's under 20 years of age in the course of thirty-three years of X-ray work. It is, however, more likely to occur the nearer we get to twenty years. In very young children it must be very rare.

In the consideration of traumatic separation of an epiphysis we must bear in mind two things:—

(1) That a genuine epiphyseal fracture, that is, one in which the fracture is exclusively in the line between the cartilage and the diaphysis, without any injury to the latter at all, is rare, if indeed it ever occurs.

(2) That whilst the main injury may be between the cartilage and diaphysis, in practically all cases there is some kind of diaphyseal injury as well.

Radiologically, in a so-called genuine case—that is, one without a diaphyseal injury—unless the epiphysis was definitely displaced and remained displaced, X-rays would show nothing. If the epiphysis was displaced then the relationship, at any

rate in the side view, of the ossified portion to the end of the diaphysis would be abnormal. The important point is that, supposing this injury happened, namely, that the radial epiphysis was displaced backwards, that it either fell back into its normal position or was reduced by a surgeon, then an X-ray examination would not show that any injury had taken place, if there was no injury to the end of the diaphysis.

From an examination of my own material, my opinion is that a definitely displaced radial epiphysis without the addition of some damage to the diaphysis is very rare indeed, even if it ever occurs.

Now when we come to the question of damage to the diaphysis we shall find two kinds of injury.

In a recent paper by Basken Werenskiold, of Oslo,[1] the author lays great stress upon the diagnostic importance of a thin lamella of bone which he describes as being detached from the under-surface of the diaphysis between the diaphysis and the cartilage. This should be shown radiographically in an antero-posterior radiograph in the X-ray gap between the diaphysis and the ossified part of the epiphysis. His paper is interesting, and there seems to be no doubt that if this "lamella" can be shown it is *ipso facto* evidence of epiphyseal injury. With many of his deductions, however, I cannot agree, especially three: (1) "That the injury may be entirely limited to the area of the detached lamella, as the separation need not necessarily be total." I cannot imagine any injury causing a small portion of bone to be torn off the lower surface of the diaphysis between the diaphysis and the epiphysis without an injury all along the epiphyseal line. (2) If this piece of bone is, in fact, torn off the diaphysis then the case ceases at once to come under the designation of "genuine," i.e., strictly limited to an injury along the epiphyseal line only, although Werenskiold wishes to consider it "genuine." (3) If it has to be sought for in many of the cases with a magnifying glass, as the author states, then it can hardly be reliable. I, personally, should not be inclined to rely for diagnosis on something on a radiograph which could only be seen through a magnifying glass.

There is, however, a second, and to my mind a much more important, diaphyseal injury, and I believe it occurs in every case in which there is, or has been, a definite displacement of the epiphysis. This injury consists in a piece of the diaphysis, from the edge of the surface in the direction in which the displacement takes place, being torn off and accompanying the epiphysis. That is, if the radial epiphysis is displaced backwards, then one must look for this tear of the diaphysis along its posterior surface. If, on the other hand, the epiphyseal displacement is forwards then we must look at the anterior diaphyseal line. In antero-posterior radiographs it is frequently indicated by a slight loss of continuity of outline on the inner or outer sides of the diaphysis.

In the lateral radiographs it will be shown as if torn off the posterior edge of the diaphysis. Sometimes it is of quite large size, sometimes very small indeed. It has to be looked for carefully in some cases, as one may not have the lateral view at quite the right angle to show it, and it may be hidden behind the rest of the bone. But my experience is that if carefully sought for it can always be found. Occasionally it is indicated radiographically merely by a small accentuated line at a slight angle to the end of the diaphysis,—what might be described as a "bending" of a portion of the cortex of the end of the diaphysis.

Now this small fracture of the diaphysis is of importance diagnostically. If you have it present it is positive evidence of epiphyseal injury in those cases in which the epiphysis itself is normally situated at the time it is X-rayed. I cannot conceive of a small fracture of this kind occurring unless in connexion with fracture and displacement along the epiphyseal line. It may be that, following the injury, the epiphysis springs back, so to speak, into correct position, but there is evidence

[1] *Acta Radiologica*, 1927, p. 419.

of the fracture if it leaves this chip from the diaphysis behind it. It may be also that the epiphysis has been replaced by reduction, still the diaphyseal chip "gives the show away," and a correct radiological diagnosis can be made with certainty.

Arrest of growth may follow an epiphyseal injury, and if this occurred and the diagnosis was missed at the time of the injury it might be unpleasant. Also, a few

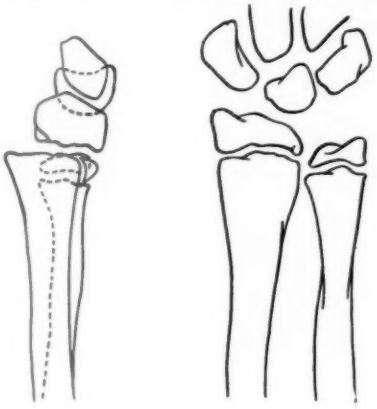


FIG. 1.

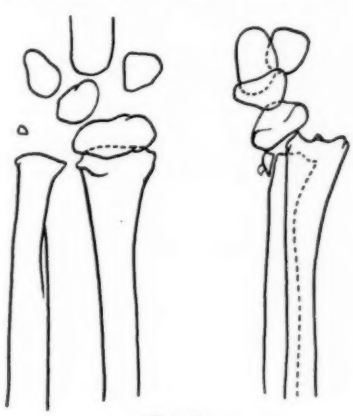


FIG. 2.

FIG. 1.—Typical backward displacement of lower radial epiphysis seen in the lateral view, whilst the antero-posterior view shows nothing. Diagnostic diaphyseal chip in side view only.

FIG. 2.—Antero-posterior and lateral radiographs. *Backward displacement* of radial epiphysis, amount not seen in the antero-posterior view.

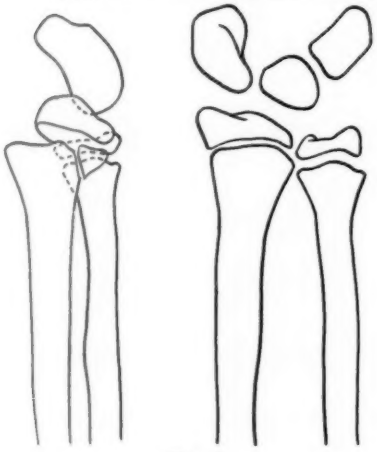


FIG. 3.

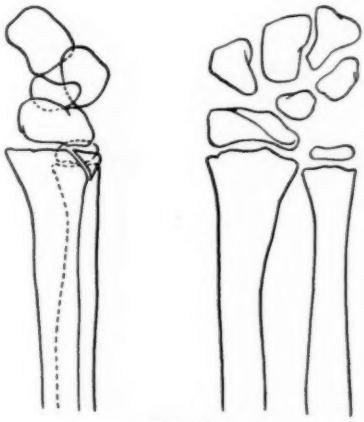


FIG. 4.

FIG. 3.—A typical case. On original radiographs when the diaphyseal chip overlies the shadow of the ulna it is sometimes difficult to make out.

FIG. 4.—Lateral view shows a very slight backward displacement of radial epiphysis and the typical chip from the posterior edge of the diaphysis. Antero-posterior view shows nothing.

weeks after an injury, the wrist being still painful and swollen, the radiographs will show a large deposit of new periosteal bone, extending some distance up the surface of the radium from the chip. This often happens, as when the diaphyseal chip is displaced any distance it means a certain amount of stripping off of periosteum

upwards—a point not visible on the X-ray at the time of the injury. This stripping of the periosteum occurs of course only on that side of the diaphysis in the direction of which the epiphysis is displaced, and it may extend up the diaphysis as much as an inch or even more.

I have already said that I believe that this slight, but diagnostic, injury to the diaphysis occurs in all the cases of lower radial epiphysis injury in which the latter

FIG. 5.

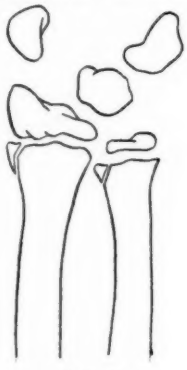


FIG. 6.

FIG. 5.—Seen in the antero-posterior view only a flake of bone partially torn off the distal surface of the diaphysis of the radius between the diaphysis and the epiphysis. Lateral view did not show it or any displacement of the epiphysis.

FIG. 6.—Antero-posterior view only. Diagnostic chips from the diaphyses of radius and ulna on the outer and posterior edges.

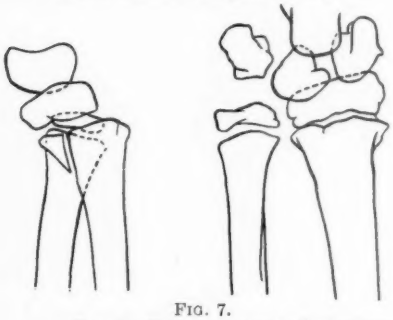


FIG. 7.

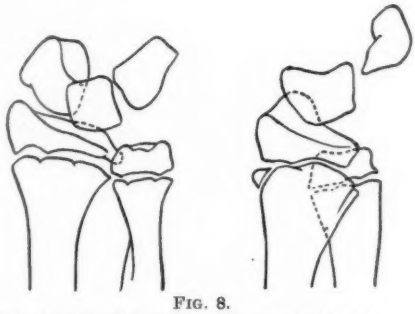


FIG. 8.

FIG. 7.—The rare anterior displacement of the lower radial epiphysis is seen in lateral view only. The chip of bone is torn off from the anterior edge of the diaphysis.

FIG. 8.—Injury to wrist. No displacement of radial epiphysis shown, but the diagnosis that there has been a displacement is made because of the separated chip of bone from the diaphysis which is shown in the lateral view.

may be said to be definitely displaced, and that when it is not shown on a radiograph this is due to its being obscured and hidden, owing to its position and to the angle at which the radiograph was taken. At the same time, though one must admit that in cases in which the injury along the epiphyseal line is a very slight one—perhaps merely a partial opening up, so to speak, without definite displacement—then the diaphysis would not necessarily be injured in this manner, and in such cases we should look carefully for the internal lamella already alluded to.

This injury to the diaphysis also occurs in displacements of the ulnar epiphysis and can be shown on radiographs. It is also found in connexion with other joints, and especially so at the lower end of the humerus.

Section of Surgery.

President—Sir HOLBURT WARING, C.B.E., M.S.

[*January* 2, 1929.]

Transillumination of the Breast as an Aid to Diagnosis.—Sir LENTHAL CHEATLE, K.C.B., C.V.O., F.R.C.S.—The following method of transilluminating the breast for the purposes of diagnosis was described to me by Dr. Max Cutler, of New York. All that is necessary is to take a patient into a dark room and gently impale the breast on the top of a curved Cameron light. All cysts are translucent; the subcutaneous veins on the surface of the breast are shown as branching shadows; papillomata in the breast are shown if there has been any hæmorrhage into the duct in which they exist, and solid tumours appear as diffuse shadows.

Sterilization of Surgical Drums.—Sir LENTHAL CHEATLE, K.C.B., C.V.O., F.R.C.S.—I will briefly demonstrate a new surgical drum which has been made for me. The drum consists of mono-metal and can be made in any shape. In the margin of the lid are two grooves which fix into a single groove surrounding the top edge of the drum. These grooves are filled with strands of cotton wool. The drum is filled, the lid is clamped and remains closed, during and after sterilization, and is opened only when the contents are removed. The contents of the drum, unlike all other surgical drums Dr. Creed and I examined, remain aseptic for months. It is impossible to sterilize the new drum in ordinary autoclaves. For sterilization it is necessary for three separate applications of superheated steam to be made. Dr. Creed discovered that all drums which are open when placed in the sterilizer run the risk of becoming septic while in the sterilizer for the following reason, namely, that the air which passes into the sterilizer in the last stage traverses the sterilizing medium at such a pace that it fails to become sterilized. The drums again run a risk of becoming septic when they are drawn out of the sterilizer before they are shut. The possibility of the contents becoming septic is assured by the inadequate protection from infection afforded by ordinary drums.

Chronic Thyroiditis: Early and Terminal Stages.—JULIAN TAYLOR, M.S.—M. J., aged 50, admitted to University College Hospital on August 10, 1925, with a history of swelling in the neck, especially on the right side, of seven weeks' duration, considerable increase in the swelling, huskiness and difficulty in breathing for three weeks.

A hard, nodular swelling of the whole thyroid gland was found, the enlargement on the right side being considerably greater than that on the left. There was inspiratory stridor, deviation of the trachea to the left, shown in the radiogram, and right abductor laryngeal palsy.

Wassermann reaction: Negative. *Blood-count*: Normal. No evidence of thyroid toxicity.

Operation.—Right lobe removed, after which the huskiness and the stridor disappeared.

In March, 1927, the patient was re-admitted to hospital suffering from carcinoma of the stomach (cardiac end) to which he rapidly succumbed. He showed no evidence of thyroid deficiency. The left lobe of the thyroid gland, palpable on his previous admission as a hard nodular mass, could not now be felt.

Description of Specimens.—The right and left lobes of a thyroid gland, the right removed at operation, the left at necropsy nineteen months later.

The specimens show the progress of the inflammation of the gland. The *right lobe* is considerably enlarged, is nodular, and when removed was found to be of a

yellowish colour, extremely hard and adherent to all neighbouring structures, including the carotid sheath and the trachea; the *left lobe* is small, smooth and fibrous.

Microscopically, the *right lobe* shows an intense round-celled infiltration of the gland, with absence of colloid material from the alveoli, which are undergoing destructive change; the *left lobe* shows complete absence of thyroid tissue, this having been replaced by dense fibrous tissue, in the interstices of which there persists in places a certain amount of round-celled infiltration.

The parathyroids, of which there were three in relation to the left lobe, although in contact with the diseased gland, have escaped involvement in the inflammatory process. The section shown has the appearance of normal parathyroid tissue.

Tumour of the Thoracic Spine compressing the Theca : Secondary to Small Renal Hypernephroma.—Shown by JULIAN TAYLOR, M.S.—A man, aged 60, was admitted to University College Hospital complaining of a rapidly-increasing paraplegia that had begun with an intense girdle pain. Later, the pain disappeared, so also did all sensation and motor power below the level of the seventh thoracic segment. There was a large bed-sore and a severe urinary infection to which he rapidly succumbed. No abdominal tumour was palpable.

In the left kidney is a small and apparently encapsulated tumour, having the typical appearances of renal hypernephroma.

Microscopical examination.—A section is shown of an intercostal vein containing a mass of vascular growth having the typical microscopical appearance of renal hypernephroma.

The specimen of the thoracic spine showed distension of the arachnoid above the point of pressure. There is also some slight distension of the arachnoid below the tumour.

Partial Occlusion of the Thoracic Aorta for Abdominal Aneurysm.—GWYNNE WILLIAMS, M.S.—G. B., male, aged 48, was operated upon at the request of Dr. Poynton in order to relieve pain.

December 19, 1923.—Exploratory laparotomy confirmed the diagnosis of abdominal aneurysm.

January 24, 1924.—The left pleura was opened after the removal of portions of the sixth to the ninth ribs close to their angles. The thoracic aorta was freed behind the pericardium, and a piece of aluminium ribbon $2\frac{1}{2}$ by $\frac{1}{2}$ in. was put around it and compressed until the pulse could only just be felt in the groin.

The day after the operation the pulse could not be felt at the ankles, but on the second day it was felt. No urine was passed in the first twenty-four hours after the operation, and in the second twenty-four hours 36 oz. were passed. The pain was distinctly relieved for the first week after the operation but then gradually returned to its original intensity. The blood-pressure, which before the operation was about 100 mm., rose within a few days of the operation to 140 mm. and remained at this point until just before death on March 29, 1924, i.e., about two months after the operation.

The specimen showed that the aluminium ring had not cut into the vessel but remained apparently embedded in the wall.

Specimen of Tumour removed from the Sensory Cortex of a Male aged 48.—CECIL P. G. WAKELEY, F.R.C.S.—The tumour consists of a hemispherical mass, measuring $2\frac{1}{4}$ in. in diameter and $1\frac{1}{2}$ in. thick. Its surface is lobulated, and resembles the surface of the brain in colour and appearance, but is more irregular. The tumour is fairly firm, and, on section, shows a more or less homogeneous, slight yellowish appearance. On microscopical examination the tumour is seen to be much "whorled," but shows no psammomata or calcification.

There is a considerable amount of fibrous tissue framework, and the tumour cells are not of the actively malignant type, but probably mildly and locally so. The tumour is a meningioma.

The patient was operated upon early in 1928 and made an excellent recovery.

Specimen of Myxochondroma of the base of the Skull, causing Pressure on the Brain.—CECIL P. G. WAKELEY, F.R.C.S.—*Clinical History.*—Patient, a female aged 32, for almost three years preceding her death complained of pain in the right side of her head and face. She vomited at intervals, without any relation to meals, was giddy, began to lose her vision and lost weight. Her walking became unsteady, the pain in the face increased, and she became progressively weaker.

Physical examination showed involvement of first, third, fourth, fifth, sixth, seventh and eighth cranial nerves on the right side and of the optic nerve on both sides. The right pupil was larger than the left, and was irregular and eccentric, and did not react to light or accommodation. There was marked nystagmus both to the right and the left. There was complete sensory loss over the distribution of the right trigeminal nerve.

Specimen.—In the region of the basi-sphenoid and basi-occiput there is a large pear-shaped mass expanding and replacing bone. The pituitary body is displaced and apparently embedded in the tumour, which compresses the right temporo-sphenoidal lobe, the right side of the pons, the middle peduncles and under-surface of the cerebellum.

Microscopically the tumour is a myxochondroma, with mucoid degeneration affecting the cartilage cells.

A Specimen of Cavernous Angioma of the Brain. — CECIL P. G. WAKELEY, F.R.C.S.—The specimen was removed post mortem from a female aged 25. The clinical history was interesting from the fact that there was no defect in the sensory system, while in the motor system there was only slight weakness of the left arm and leg, and constant tremor of the left hand. There was no incoordination, no intention tremor and no Rombergism. The tendon reflexes were normal and the plantar response was flexor.

There was a dark red, vascular tumour $1\frac{3}{4}$ in. by 1 in. below the corpora quadrigemina in the crura cerebri and around the aqueduct of Sylvius, extending anteriorly to within a $\frac{1}{4}$ in. of the posterior wall of the third ventricle, posteriorly to the upper part of the fourth ventricle, and laterally to the outer surface of the right crus, and on the left side not so far beyond the mid-line.

Microscopically, the tumour shows irregular vascular spaces filled with blood. There is moderate distension of the lateral and third ventricles, and distortion of the anterior part of the fourth ventricle.

Leiomyoma of Rectum.—W. S. PERRIN, M.Ch.—The specimen was taken from the rectum of a man aged 39. There was a history of bleeding and of a lump appearing at the anus on defæcation for some years. The tumour was attached to the anterior wall of the rectum just inside the anal canal with its long axis vertical. The pedicle was very short. The base was ligatured with stout silk and the tumour removed. It measured 3 in. by $1\frac{1}{2}$ in. by 1 in.

Large Single Cyst of Left Kidney.—W. S. PERRIN, M.Ch.—The specimen was taken from a woman aged 62. The cyst grew from the lower pole of the kidney and projected into the abdominal cavity, forming a tumour in the left side of the abdomen. It contained just over a pint of clear fluid. The fluid contained neither hooklets nor cells. A few yeast cells were seen. Cultures proved sterile.

The symptoms were of four years' duration, the principal one consisting of occasional pain in the left groin, occurring usually on rising from a chair.

Ruptured Kidney.—Shown by J. PATERSON ROSS, F.R.C.S. (for G. E. GASK, C.M.G., F.R.C.S.).—E. A., male, aged 18, was injured in the right loin by the shaft of a meat barrow, and admitted to hospital because of pain and faintness. The faintness passed off, but the pain in the loin persisted, accompanied by muscular rigidity in the flank. Six hours after the injury he vomited. Nine hours after the injury he had not passed urine, so a catheter was passed and 8 oz. of bloody urine were drawn off. This caused a great exacerbation of the pain in the loin. Twelve hours after the injury the pulse was still 84 and the patient's colour was good. No swelling was palpable in the loin (till under an anæsthetic), but the pain was much worse.

Operation revealed a hæmatoma containing about half a pint of blood and clot around the kidney. The kidney, which was in two portions connected only by a branch of the renal artery, was removed, complete recovery resulting.

Chondroma of Pelvis.—Shown by T. HOLMES SELLORS, L.R.C.P., M.R.C.S. (for G. GORDON-TAYLOR, O.B.E., M.S.).—Female, aged 29, had been operated upon in 1920 for a tumour, which was reported to be a chondroma, and was treated with radium.

During the next seven years the lump increased in size and in 1927 the patient was bed-ridden with massive œdema of the right leg and abdominal wall: dyspnœa was also present. The tumour filled the right side of the abdomen, pushing up the right diaphragmatic cupola and extending into the pelvis and across the mid-line. X-ray with a barium enema showed the extent to which the large gut was displaced medially.

In November, 1927, an operation was performed under paravertebral block and local infiltration with novocain and light general anæsthesia. The incision was oblique to transverse over the right iliac fossa and extended to the left costal arch: a second incision at a right angle extended to the pubes.

The tumour was found to be encapsulated, arising from the right sacro-iliac synchondrosis and was entirely extraperitoneal. Intestine was adherent to the capsule, the ureter ran over its surface, and the right iliac vessels were tightly stretched across its lower pole. The adherent structures were displaced medially with the capsule, and the base of the tumour was severed with a chisel.

Convalescence was satisfactory and though the patient had a raised temperature for 40 to 50 days due to *Bacillus coli* infection of the wound, the general condition remained good. On the fortieth day a small fæcal fistula appeared, but this subsided in three days. At the end of seven weeks the wound had healed. The œdema of the right leg subsided gradually, and, though all movements were possible, there was some loss of power.

A year later the patient had recovered most of the power of her leg and was able to do light work. X-ray examination revealed numerous opacities of vague outline at the site of the original tumour and some areas of rarefaction in the region of the ilium. These might be interpreted as evidence of an ossifying sarcoma, but in view of the absence of clinical evidence the shadows probably represent residual tumour cells or osteosclerosis occurring as a repair process.

Specimen.—The dimensions of the tumour on removal were 13 in. by 11 in. by 8 in., and the weight was over 18 lb. The tumour is lobulated, hard, and of a pinkish-white gelatinous appearance. Sections showed hyaline cartilage in somewhat atypical arrangement. Here and there an attempt at deposition of calcium is to be noticed, but there is no true formation of bone in any part. A firm surrounding fibrous capsule is seen. No evidence of malignancy was found.

Tumour, originating near the Greater Curvature of the Stomach.—Shown by T. HOLMES SELLORS, L.R.C.P., M.R.C.S. (for G. GORDON-TAYLOR, O.B.E., M.S.).—The specimen was removed from a woman, aged 24, in November, 1927. She had noticed a swelling in the left hypochondrium for some weeks. The swelling extended from under the left costal margin to the mid-line; it was nodular and moved on respiration.

Operation.—This was performed under left paravertebral and left post-splanchnic regional and light general anæsthesia. A large, pinkish-white, nodular tumour was found, involving the stomach and transverse colon, with numerous large veins coursing over its surface. The colon was divided with the cautery at the middle of the transverse and at the middle of the descending parts. The stomach was divided beyond the pylorus and just above the limit of the growth. The colon was united by an end-to-end junction, and an anterior anastomosis of the Polya-Balfour type was performed between the cut end of the stomach and the jejunum. A cæcostomy was established through a stab wound in the right iliac fossa, and a transfusion of 500 c.c. whole blood was given at the completion of the operation.

Specimen.—The tumour is a large, pinkish white, nodular mass of firm tissue, which would appear to have originated in the neighbourhood of the greater curvature of the stomach and to have extended into the transverse meso-colon, to the right of the splenic flexure. On section it showed firm, whitish growth with local areas of hæmorrhage. The mass includes stomach, spleen and colon.

Microscopically.—The muscle layers of the gut were infiltrated by masses of spindle cells. These masses are seen to merge gradually into adult fibrous tissue. The irregularity in size and shape of cells is not marked, nor is there marked hyperchromatism in the nuclei. Mitoses are rare.

These appearances are considered to be consistent with a diagnosis of locally malignant cellular fibroma, without metastases.

Postscript.—March, 1929.—The patient is in good health.

Carotid Body Tumour.—Shown by T. HOLMES SELLORS, L.R.C.P., M.R.C.S. (for G. GORDON-TAYLOR, O.B.E., M.S.).—The specimen was removed from a man, aged 54. A swelling was first noticed in the neck in 1925 following a dental abscess. In 1927 there was a definite mass of enlarged glands at the angle of the jaw, and as the tonsils were manifestly septic they were removed by enucleation; they showed no signs of malignancy. Hodgkin's disease was suspected, but no further evidence of it could be found. Wassermann reaction negative. The mass increased slowly in size and the patient lost weight. Examination for a primary malignant focus in the pharynx was negative.

Operation, March, 1927, by Mr. Gordon-Taylor, under local and light general anæsthesia. A block dissection of the neck was effected and it was found that there was a tumour at the bifurcation of the internal and external carotid arteries. This was very adherent and partly surrounded the vessels: it was separated from these with considerable difficulty. The vagus, however, was so incorporated with the tumour that a considerable section had to be resected: the sympathetic cervical chain was also divided. Convalescence was satisfactory and uneventful.

Division of the nerves has been followed by paralysis of the vocal cord, with some alteration in voice; also by enophthalmos and slight dropping of the eyelid.

In March, 1928, a year later, a swelling had appeared in the neighbourhood of the scar; this was excised. Section showed that it consisted of scar-tissue around ligatures with some foreign-body giant cells.

There have been no further signs and the patient at the present time is perfectly well.

Tumour of the Carotid Body.—Shown by T. HOLMES SELLORS, L.R.C.P., M.R.C.S. (for G. GORDON-TAYLOR, O.B.E., M.S.).—*Description of Specimen. Macroscopically.*—The tumour is of soft consistence with a well-defined capsule. The cut surface has a spongy appearance produced by the presence of numerous small vascular slits and channels. In places the surface is pale in colour, in others there are deep red vascular patches resembling cavernous angiomata in appearance.

Microscopically.—The bulk of the tumour is made up of cells with pale, reticular nuclei. Throughout the greater part of the section the tumour cells are arranged in

a number of closely packed, rounded masses somewhat resembling glomeruli in appearance, but measuring on the average only about half the diameter of a human glomerulus. Each of the small masses of cells is demarcated by a single or double layer of flattened cells. These flattened cells evidently represent a vascular endothelium, since in some cases they form the walls of a vascular space surrounding one of the cell masses. Another striking feature of the tumour consists in the presence of numerous thin-walled vascular channels, which, in places, form collections of large spaces separated from one another by endothelium only, and having the structure of a cavernous angioma.

Histological Examination of several lymphatic glands from the neighbourhood of the tumour failed to reveal any secondary deposits of growth.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

[*January* 16, 1929.]

Three Cases of Rectal Carcinoma treated with Radium after Two-and-three-quarter Years, One Year, and One Year respectively.—Sir CHARLES GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.—Especial attention is called to the case treated one year ago with radium needles passed into the growth through the peritoneal cavity. This is a new method which, so far as I can ascertain, has not been tried previously on the Continent. The case has been treated throughout without colostomy, and though at the time of radiation the colon was brought outside and prepared for colostomy it was never opened and has receded spontaneously without operative treatment. The growth extended above the peritoneum, surrounded the bowel and was fixed to both bladder and sacrum. No difficulty has been experienced with the action of the bowels during treatment. From the time of radium insertion the patient made a progressive recovery; he returned to work soon after leaving hospital, and has continued at work as a postman without interruption and without any inconvenience.

I have since employed the same method in other cases and am now applying radon seeds as a routine to the glandular areas within the abdomen both as a preliminary to rectal excision and rectal radium barrage.

The other two cases have been treated with radium by the open operation from the perineum after preliminary colostomy. The woman was shown before the Sub-section in November, 1927. She has continued well since that time and without sign of recurrence. It is now two years and nine months since the radium treatment was first given for a hopeless inoperable growth. The man was treated for an inoperable growth by the same method a year ago. I am not prepared to say that he is cured, but he is remarkably well and without evidence of rectal growth. He has gained much weight and has returned to work.

The microscopic specimen, taken after radium treatment, which Dr. Dukes put up for me from a man who developed recurrent carcinoma in the perineum two years after excision, shows that radium has a useful field of application in this direction.

Case I.—Male, aged 44. Postman.

History.—Five weeks' diarrhœa and loss of weight. Mucus and blood per rectum.

Diagnosis.—Carcinoma of rectum. Inoperable. Fixed to bladder. Supra-peritoneal. Commencing 4 in. from anal margin, annular stenosis.

January 2, 1928. *Colostomy.*—Liver free. Growth firmly adherent to bladder and sacrum. Annular.

Intraperitoneal needle-barrage applied to growth, nine needles. Total 20·5 mgm. 115 hours = 2357 mgm. hours.

Post-operative Result.—No pain. No cystitis. Colostomy not opened. Bowels open daily with paraffin.

February 10, 1928.—Finger passes easily into lumen of growth, no ulceration. Stony hardness beneath mucosa anteriorly.

May 15, 1928.—Sigmoidoscopy. Lumen somewhat narrowed at site of growth. Mucosa smooth and appears healthy. No visible growth.

Patient has gained 2 st. in weight, and is engaged in his normal work as postman. Bowels open daily, regularly. No medicine.

January 16, 1929.—Excellent health. Bowels open daily. Still gaining weight. Sigmoidoscopy. Rectum appears quite normal though slightly narrowed.

Case II.—Female, aged 39 in 1926. Inoperable carcinoma. (Microscopic section showed adeno-carcinoma.) Fixed, ulcerating growth completely surrounding

lumen and attached all round to the pelvic wall, commencing 2 in. from anal margin and extending upwards so as to involve the lower part of the peritoneal portion of the rectum.

March 16, 1926.—Colostomy. Liver free from metastasis.

April 12, 1926. — Perineal exposure of growth and radium barrage $\frac{38 \text{ mgm.}}{168 \text{ hrs.}} = 6{,}384$ mgm. hrs.

April 19, 1926.—Vaginal barrage $\frac{10 \text{ mgm.}}{72 \text{ hrs.}} = 720$ mgm. hrs.

Very severe constitutional reaction and patient nearly succumbed from asthenia.

June 5, 1928.—Lumen of rectum occluded by fibrous stenosis. No evidence of growth. Gained 3 st. in weight. General health excellent.

Case III.—Male, aged 52. Inoperable carcinoma (microscopic section — adeno-carcinoma) involving entire circumference except for half an inch posteriorly. Growth commences half an inch from the anal margin, extends upwards for 3 inches and is firmly fixed.

January 12, 1928.—Colostomy. Liver free from metastasis.

February 12, 1928.—Perineal exposure of growth with resection of coccyx. $\frac{51 \text{ mgm.}}{235 \text{ hrs.}} = 11{,}985$ mgm. hrs.

October 10, 1928.—Sigmoidoscopy. Tube passes through fibrous stricture. Some growth visible at 8 cm. anteriorly.

General health greatly improved. Gaining weight.

January 16, 1929.—Marked perirectal fibrosis, but no ulceration. General health very good. He has returned to work.

Microscopic Specimen from a Case of Recurrent Carcinoma of Rectum (following excision) treated with Radium.—Sir CHARLES GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.—Patient, male, aged 55.

January 21, 1926.—Colostomy and perineal excision in India.

June 4, 1928.—Developed abscess in perineal scar which was laid open. Dense indurated tissue found between prostate and perineum and spreading out into both ischio-rectal fossæ. Portion removed for section = adeno-carcinoma.

June 12, 1928.—Radium implantation. Twenty-two needles, 26·5 mgm. R. Total dose 8,416 mgm. hours.

June 26, 1928.—Columbia paste distance radiation to perineal and sacral area, 60 mgm. R. Total dose of distant radium therapy, 5,760 mgm. hours. Total radium application, 14,160 mgm. hours.

Result.—All deep indurated area resolved, and wound gradually closed except for small area, skin edges of which remained indurated.

October 29, 1928.—Condition of wound stationary. Superficial ulcer has made no progress for two months. Skin induration somewhat increased. No deep induration.

Whole thickness of ulcer and skin edges dissected away *en bloc* and submitted to microscopic examination and 50 mg. R. applied to base for seventy-two hours.

Report by Dr. Dukes :—

"There is no evidence of any growing recurrence of adeno-carcinoma in any portion of these two pieces of tissue. In one region there is a poorly staining, irregular mass of nuclear material surrounded and infiltrated with large phagocytic cells. This may perhaps represent a healed recurrence now being absorbed, but the histological appearances are not sufficiently definite to make this more than a conjecture. Elsewhere there is certainly no sign of any form of malignant disease. The tissue consists of a poorly-vascularized dense fibrous tissue such as is developed in the healing of an inflammatory focus."

Subsequent History.—Radium reaction involving skin continues and wound has not yet healed but there is no induration of the parts and no trace of malignant tissue. General condition good.

Prolapsed Hæmorrhoids in an Erythræmic Patient. with Psoriasis.—J. P. LOCKHART-MUMMERY, F.R.C.S.—Male, aged 53; well-marked generalized psoriasis. The patient had previously undergone a Whitehead's operation performed for hæmorrhoids in 1912. Spleen enlarged downwards to umbilicus. Liver, two fingers'-breadth below ribs.

Sigmoidoscopic examination to 20 c.mm. showed marked varicosity of veins. Red cells, 6,600,000. Hæmoglobin, 110 per cent., leucocytes, 7,000.

Specimen: Malignant Ulceration of both Pelvic Colon and Rectum.—J. P. LOCKHART-MUMMERY, F.R.C.S.—A portion of rectum and adherent loop of pelvic colon bound together by dense adhesions; an ulcer was seen in the lumen of the rectum at the recto-sigmoidal junction and another in the pelvic colon, giving the appearance of two separate growths. On dissection, however, a continuous band of malignant growth was found connecting the two ulcers together.

Microscopic examination does not show definitely which is the primary growth, but in all probability this is an adeno-carcinoma of the recto-sigmoidal junction, which has spread by direct continuity through the bowel wall and re-erupted into the pelvic colon. A portion of the left ureter was adherent to the mass of tissue and was removed together with the tumour at operation. The ureter was tied above and below as there was not enough length to enable an anastomosis to the bladder to be carried out. The ureter itself does not show any pathological changes and is not actually infiltrated with malignant growth, but was simply bound down by inflammatory adhesions. No bad results followed the tying of the ureter and the patient made a good recovery. From the point of view of prognosis this is a C-case.

Specimen: Cancer of the Pelvic Colon removed by Perineal Excision.—J. P. LOCKHART-MUMMERY, F.R.C.S.—The patient, a male, was sent to me on account of carcinoma of the rectum. On examination there was a growth about the size of half-a-crown easily felt just within the rectum; on one occasion it had actually prolapsed. There were no signs of glands or of secondary deposits, and the case appeared to be a very favourable one. A colostomy was performed, and the patient was prepared for removal of the growth by perineal resection. When I came to perform the operation I found that the tumour, which I had supposed to be in the lower part of the rectum, really occupied the junction of the lower and middle third of the pelvic colon. When I had first seen it, it had been prolapsed down into the rectum with an intussusception of the bowel. In order to remove the growth it was necessary to resect an enormous length of colon in addition to the rectum. The actual length of bowel removed was nearly 2 ft.; this illustrates the length of bowel that can be removed by a perineal resection in a suitable case, although I am not advocating this as a general routine. The patient made a good recovery.

Specimen: Prolapsed Hæmorrhoids of Excessive Dimensions.—J. P. LOCKHART-MUMMERY, F.R.C.S.—Patient, a male, admitted to one of my beds at St. Mark's Hospital with strangulated, prolapsed hæmorrhoids. The swelling was so huge that it was thought at first to be a case of prolapse of the rectum. On examination it was obvious that it simply consisted of three enormous prolapsed piles. I have never heard of, or seen, anything approaching the size of the tumour that occurred in this case. The mass was nearly as large as a soup-plate and actually produced ulceration of the buttock through pressure. It seems difficult to account for the enormous size that the hæmorrhoids reached, which is well shown in the picture exhibited. Undoubtedly the whole mass would have become gangrenous but for the fact that the sphincter gave way, thus preserving the blood-supply. The patient was first treated by simply raising the lower end of the bed for a week,

but it was then seen that the swelling was not diminishing and it was decided to operate. I removed the piles by the ordinary method of ligature, and he made quite an uneventful recovery, except that the tremendous stretching of the sphincter necessitated further operation to shorten the muscle. He is now perfectly well and normal. The largest pile must have been at least 4 in. in diameter.

Hypertrophic Tuberculous Ulceration of the Anus.—J. P. LOCKHART-MUMMERY, F.R.C.S.—Patient, a male, aged 30. This is rather a rare form of tuberculous ulceration. In this case it appears to have been primary as there was no evidence of tuberculosis in the lungs or elsewhere in the body. There was a large flat surface, covered with hard papules, with very little actual ulceration although the skin and tissues were completely destroyed over the area involved. The condition was not particularly painful, and it was clearly a case of tuberculosis of the chronic type with hypertrophy rather than ulceration.

I treated it by diathermy, the whole surface being thoroughly cooked with a diathermy applicator. The condition now, a fortnight after operation, is satisfactory; you will notice that there is a healthy granulating surface which is rapidly skinning over. It seems to me that this is far better than any attempt to remove the area by cutting, as it prevents any fear of spreading the infection to other parts of the body, and the elimination of the disease is more effectual.

Case of Perineal Hernia following Perineal Excision of the Rectum.—LIONEL E. C. NORBURY, O.B.E., F.R.C.S.—G. B., male, aged 60. The operation was followed by a large hernia at the site of operation. This does not cause much discomfort. Such herniæ are more likely to occur when the operation necessitates removal of much bone. In this case a portion of the sacrum has been removed.

Specimen. Fibro-leiomyoma of Small Intestine.—LIONEL E. C. NORBURY, O.B.E., F.R.C.S.—Male, aged 68. Symptoms of increasing intestinal obstruction, thought to be due to growth of pelvic colon. Abdomen distended and well covered. Tumour not palpable until patient was anæsthetized.

Abdominal Exploration.—Tumour found growing from small intestine, jejunum or upper ileum. Resection of portion of gut involved and end-to-end anastomosis. Patient died a few days later from acute dilatation of stomach.

Report on Tumour by Dr. Cuthbert Dukes.—The large, round tumour sent for examination weighed 2¾ lb. It was slightly ovoid in shape, the lengths of its axes being 6, 5 and 3½ in. The tumour was firmly attached to a segment of the small intestine, which was removed also at the operation. On sectioning, the tumour had a firm yellowish white consistence which was homogeneous throughout except towards its lower pole, where a large hæmorrhage had occurred. In this region the tissue is more friable.

Microscopic examination shows the tumour to be composed almost entirely of young fibroblasts and interlacing bands of fibrous tissue. It is firmly attached to the bowel, from the muscle coat of which it appears to have arisen. It is completely encapsulated and shows no evidence of malignant changes. Frozen sections show no evidence of important fatty degeneration or infiltration. In the region where the hæmorrhage was observed, a mass of necrotic tissue is found infiltrated with wandering cells. In some regions the tumour is formed of less well developed fibrous tissue than in others, having here the characters of a soft fibroma.

In the upper pole of the tumour, near its attachment to the intestine, there are some undoubted smooth muscle cells incorporated into the tumour mass, and elsewhere in the tumour are to be seen cells which almost certainly are of muscle origin.

I look upon the tumour as being a fibro-leiomyoma, which has arisen from the smooth muscle and connective tissue of the bowel, and which has come to be built up almost entirely of fibrous tissue.

Specimen. Carcinoma Arising in Pedunculated Adenoma of Pelvic Colon.—LIONEL E. C. NORBURY, O.B.E., F.R.C.S.—Mrs. M., aged 54. Hæmorrhage from bowel for past six months; loss of weight.

Sigmoidoscopy showed polypus, possibly malignant, at 20 cm. from anus.

Laparotomy.—Removal of polypus with long pedicle by incision of pelvic colon. Temporary cæcostomy. Recovery. Distance of tumour from anus much greater than appeared on sigmoidoscopy, which shows that tumours of the pelvic colon tend to intussuscept into the colon or rectum.

Report on Tumour by Dr. Cuthbert Dukes.—Early malignant change is to be observed in the tip of this adenoma. The malignant change appears to have begun in several separate spots all situated at the tip of the adenoma. The appearances therefore show separate foci of cancer separated by areas of normal epithelium. There are no changes to be observed in the stalk of the adenoma. Pathological diagnosis is *early malignant change at the tip of a pedunculated adenoma.*

Specimen. Tuberculous Ulcer of Cæcum with large Enterolith.—LIONEL E. C. NORBURY, O.B.E., F.R.C.S.—Female, aged 50. History of chronic constipation and wasting. Mass palpable in cæcal region. Diagnosed as probable carcinoma of cæcum.

Laparotomy.—Large mass felt involving cæcum. Multiple small, hard glands along ascending colon. Resection of lower portion of ileum, cæcum and ascending colon.

Lateral anastomosis of ileum to transverse colon. Recovery.

Pathological Report.—A case of chronic obstruction, with a palpable mass in the cæcal region, showing the clinical features of carcinoma. Operation revealed a scarred contracted condition of the affected piece of gut with the presence of a large stercolith (seen in the specimen) in a pocket connected with the lumen of the gut.

Sections of the thickened gut in the vicinity show that the scarring and contraction are due to chronic fibrosing tuberculosis. This infection was probably the initial event and the formation of the stone a sequel. By irritation the stone would appear to have partially eroded the gut and to have become lodged in a pocket so formed.

Specimen. Fibrous Polypi associated with Internal Hæmorrhoids.—LIONEL E. C. NORBURY, O.B.E., F.R.C.S.—This shows the method of formation of fibrous polypi, which result from thrombosed internal piles, in which fibrosis has taken place. In many cases of prolapsing internal piles of long standing, such piles will be found converted into fibrous polypi.

Carcinoma of the Rectum treated by Radium without Colostomy.—W. S. PERRIN, M.Ch.—I have three cases to describe and one to show of carcinoma of the rectum treated by radium without colostomy. In each case the patient refused excision, with its inevitable accompaniment of colostomy, but was willing to risk treatment by radium. The method of access was that described by Sir Charles Gordon-Watson in his admirable address recently [1] delivered before this Section. Except in minor details as to the length of needles and concentration of emanation, or radium, in each needle the technique employed was as close an imitation of his as possible. I am indebted to Sir Charles for inviting me to St. Bartholomew's Hospital, and for showing me his method of attack. The first patient has already been examined by several Members of the Section who all agree that there is now no sign of carcinoma.

[1] *Proc. Roy. Soc. Med.*, 1928, xxi (Sect. Surg., Sub.-Sect. Proct.), 9-19.

Case I.—Patient, a male, aged 60, complained that for seven months he had passed blood on defæcation and with difficulty, and had felt pain in the process. Motions soft and formed; appetite good; no loss of weight.

On examination a carcinomatous ulcer could be felt on the posterior surface of the rectum, just above the anal orifice. The ulcer was about four centimetres in diameter. It was freely movable, with raised everted edges.

Operation.—July 13, 1928, the growth was treated, as illustrated in the accompanying diagram, with six 4 cm. needles and eighteen 2 cm. needles (fig. 1). Each needle contained 1·5 millicuries of radon to each centimetre of over-all length. The needles were left in for five days and then removed.

Allowing for the rate of decay of the radon the total dosage amounted to 6,000 mgm. hours of treatment with radium element.

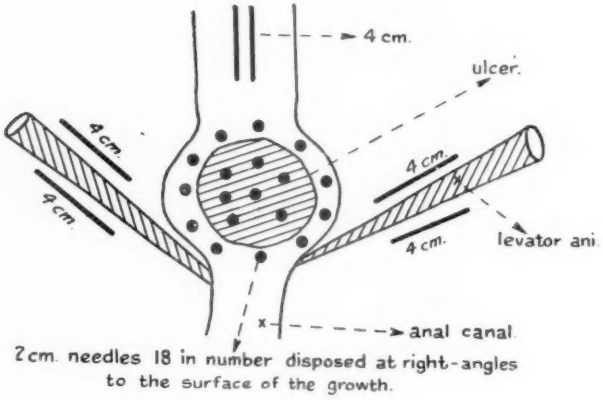


FIG. 1.

After-history.—On August 10, exactly four weeks after the operation, the wound was almost healed and the patient was discharged from hospital. On August 15 the carcinoma felt to the finger like a disc of india-rubber. It was not tender, and the wound was healed. On August 28, six weeks after operation, the ulcer was still recognizable, with its limits defined, but felt much shrunken. On September 28, two and a half months after operation, the patient, a chemical worker, had resumed his occupation. There was no growth to be felt and his condition has remained unchanged until the present date.

Case II.—Patient, a female, aged 42, complained of a discharge of blood and mucus for the preceding six months, particularly in the early morning, and a feeling that something was present in the rectum which she could not pass.

On examination a carcinomatous ulcer, just above the anal canal, and extending completely round the rectum, except for about one fourth of the circumference, could be felt. It was impossible for the finger to reach the upper edge of the growth.

Operation.—On September 21, 1928, the following needles were inserted, each containing 1 mgm. of radium for each centimetre of over-all length: Four 6 cm. needles, eight 4 cm. needles, six 3 cm. needles, two 2 cm. needles.

The needles were left for five days and the total dosage expressed in terms of radium element was 9,360 mgm. hours. At the time of operation a carcinomatous gland was removed from above the levator ani. The section showed secondary columnar tubular carcinoma of lymphatic gland.

Owing to the mass of the growth, which almost completely surrounded the gut, it was impossible to insert the needles in the manner indicated in the first case. Instead of transfixing the carcinoma with short needles placed at right angles to the surface, many of the needles

were inserted parallel to the surface of the growth in the form of a raft. The same general principle, however, was observed, that the distance between each needle should be as nearly 1·5 cm. as practicable. The accompanying diagram gives an idea of the method of insertion (fig. 2).

After-history.—Convalescence was protracted and stormy. Just before operation the patient had been treated for tertiary syphilis. About three and a half weeks after operation,

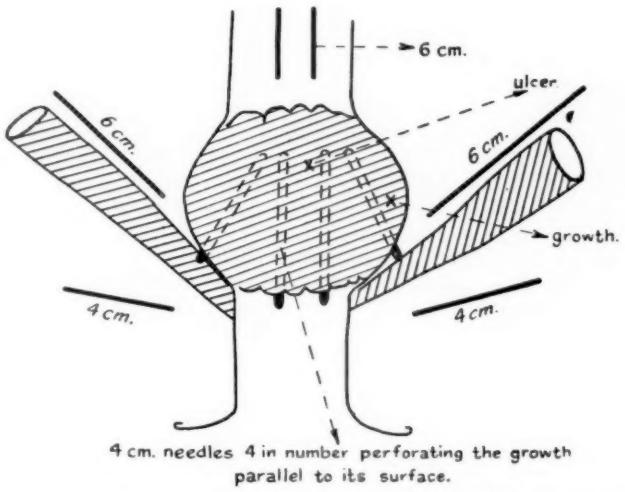


FIG. 2.—The remaining two 4-cm., six 3-cm. and two 2-cm. needles were distributed irregularly into and around the growth.

when the wound was healing well, she contracted erysipelas which further prolonged the period of recovery. However on January 3, 1929, sigmoidoscopy showed that the growth had completely vanished. Slight difficulty was encountered in passing the sigmoidoscope owing to fibrous contraction as a result of treatment with radium. It is probable that this patient will require a colostomy.

Case III.—Patient, a male, aged 48, complained that three months ago he had passed about a quarter of a pint of blood from the rectum and for the last few weeks had a feeling that something was there which he could not pass. Seven years before, a polypus had been removed from the rectum. He was sigmoidoscoped on November 19, 1928, when carcinoma of the rectum was discovered and a portion removed for section. The section revealed an adeno-carcinoma of the rectum of a polypoid type. The cells showed hyperchromatosis and numerous mitotic figures.

On examination, a carcinomatous ulcer was found on the posterior surface of the rectum just above the anal canal. The edges were raised and everted. The finger could not reach above the growth, which was freely movable and bled readily.

Operation.—December 6, 1928, the following needles were inserted, each containing one milligramme of radium element for each centimetre of over-all length: Two 6 cm. needles, six 4 cm. needles, five 3 cm. needles, three 2 cm. needles.

The needles were left *in situ* for five days and the total dosage, expressed in terms of radium element, was 6,840 mgm. hours.

After-history.—The wound healed well and on January 1 the patient was discharged with the wound almost healed. He was last seen on February 1, rather less than two months after the operation. The carcinomatous ulcer had almost disappeared. A small polypoid mass could be felt at the site of the lower edge of the original ulcer. This may or may not require further treatment.

Conclusions.—(1) It is possible from a consideration of these three cases to attempt the abolition of a carcinoma of the rectum by the application of radium without performing a colostomy.

(2) The second case shows that when the carcinoma is very extensive and almost surrounds the rectum, the development of a stricture is to be feared. This may necessitate a subsequent colostomy. The delayed healing in this case must not be unduly emphasized. The presence of tertiary syphilis and the unlucky accident of the patient developing erysipelas are quite enough to account for the slow healing.

(3) The presence of a polypoid mass in the third patient at the site of the lower margin of the ulcer as late as seven weeks after the insertion of the radium shows the need for accurate distribution of the needles. I think that if the initial insertion of radium had been successful this polypoid mass should not have been present so long after the operation. A further insertion of radium will probably be necessary.

(4) Patients treated by this method under favourable circumstances need not undergo a prolonged convalescence.

Specimen : Constricting Carcinoma of the Pelvic Colon.—W. B. GABRIEL, M.S.—Patient, an emaciated man, aged 57, weighing 7 st. 12 lb., gave a history of increasing difficulty in getting his bowels open. This difficulty in defæcation finally developed into an acute intestinal obstruction. Shortly before this event his medical attendant had felt a mass below the left costal margin and it appeared likely that this would prove to be carcinoma of the splenic flexure of the colon. On September 23, 1928, I operated for relief of the obstruction ; he was then in an almost moribund condition ; his temperature was subnormal, extremities cold, his abdomen acutely distended and very tense. I did a blind cæcostomy which drained very well.

The second operation was done four days later, September 27, through a left paramedian incision. The tumour of the left upper abdomen proved to be an enlarged and very hard kidney, and the distended colon extended down as far as the pelvic colon where, after considerable search, I located the "string" carcinoma now shown. In view of the patient's grave condition I rapidly brought the loop outside the abdomen, excised the growth and established a double-barrelled colostomy which I hoped to close later by the crushing method. His condition remained satisfactory for some days ; then it began to change for the worse, and he died on October 13, i.e., sixteen days after the laparotomy. A post-mortem examination was not carried out and the nature of the renal tumour was not established.

Specimen showing Fixation of an Early Rectal Carcinoma by Inflammatory Adhesions.—W. B. GABRIEL, M.S.—Patient, a male, aged 57, admitted to St. Mark's Hospital under my care in November, 1928. A carcinoma in the upper third of the rectum was diagnosed ; the lower border of this could just be felt digitally. His abdomen was distended and numerous large fæcal masses could be felt. On November 13 I explored the growth through a mid-line sub-umbilical incision ; it was situated at the recto-sigmoidal junction and there appeared to be direct and very extensive spread into the glands in the pelvic mesocolon behind the growth, infiltration extending up along the inferior mesenteric vessels. The whole mass of growth and the glands almost completely filled his pelvis which was very narrow. On account of these difficulties an abdomino-perineal excision seemed impracticable. His colon above the growth was loaded with hard fæces. I therefore performed a left inguinal colostomy in continuity, with closure of the lateral space. The colostomy was opened forty-eight hours later and the next three weeks were devoted to clearing out the bowel by olive oil injections and daily lavages.

The second operation was carried out on December 4. I had determined to effect an abdomino-perineal excision, beginning with the perineal part of the operation. At the perineal stage I freed the rectum below and opened the peritoneal

cavity widely anteriorly. The patient was then turned on to his back and the abdominal operation completed in the usual way except that instead of the growth being pushed down into the pelvis it was delivered upwards, and the colon proximal to the growth was finally divided and closed in a few inches below the colostomy. Some soft adhesions to the bladder had to be separated. The pelvic floor was repaired from above.

Pathological investigation proves the growth to be an *early* adeno-carcinoma which has only penetrated circular muscle. The lymphatic glands along the superior hæmorrhoidal vessels are greatly enlarged but contain no metastases. In fact, the whole mass in the meso-colon is inflammatory.

Commentary.—The case illustrates the difficulty in assessing operability clinically. It appeared to be on the border-line of operability; pathologically it proves to be an early and favourable case after radical operation. I believe the method adopted here was the only one by which the difficulties of the large tumour and the narrow pelvis could have been surmounted. The man has made a good recovery; he has already put on weight and is able to walk about the ward.

Specimens: Submucous Implantation Cysts of the Rectum.—CUTHBERT DUKES, O.B.E., M.D.—Submucous implantation cysts are due to the continued growth in the submucosa of traumatically misplaced columnar epithelium. The accident of traumatic epithelial displacement is likely to occur in all surgical operations, but presumably the disseminated cells are not as a rule able to grow in

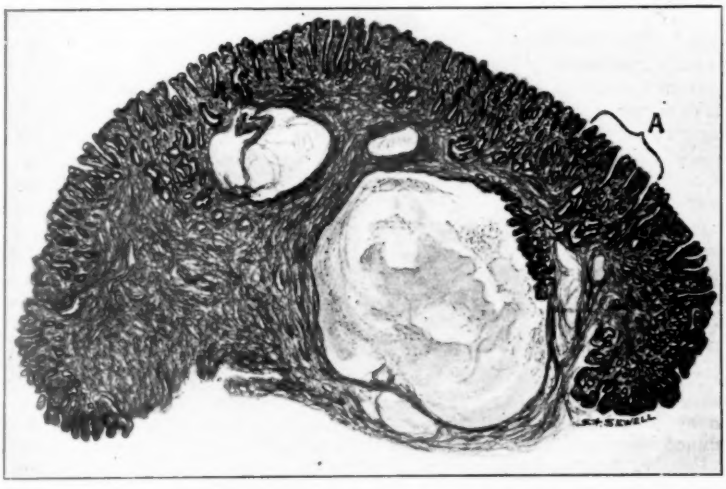


FIG. 1.—The cysts lie within the submucous coat of the rectum, and are partially lined with healthy mucus-secreting columnar epithelium. The cysts are separated from the mucous membrane by the muscularis mucosæ, but in some of the cysts a connecting link of glandular epithelium can be traced from the mucous membrane to the cyst. A higher magnification of the region marked A is shown in fig. 2.

their new environment, since implantation cysts are a rare sequel to injury in any region of the body. In the large intestine such implantation dermoids must be very rare. I have not been able to find any reference to them in the special surgical and pathological text-books nor in the literature I have had the opportunity of consulting.

In each of the two cases I am about to describe the implantation cyst occurred after a previous surgical operation on the rectum. The cyst increased in size and formed a troublesome mass in the rectal wall, causing pain and difficulty in defæcation. The true nature of the condition was not anticipated from naked-eye inspection, although, as may be seen from the specimens, the appearances are fairly characteristic.

Case I.—Patient, a female, aged 36, who was admitted to St. Mark's Hospital on March 23, 1928, with the complaint of painful defæcation, aching over the sacrum, hæmorrhage and general weakness. Three and a quarter years previously, in another hospital, she had been operated upon for prolapse of the rectum and a lump removed from the posterior rectal wall which histological examination showed to be a chronic inflammatory lesion. The above-mentioned symptoms came on gradually after discharge from hospital and their occurrence finally determined her return. On admission a tender swelling was discovered at the lower end of the rectum on the posterior wall.

Mr. Gabriel removed the tumour by local excision, April 2, 1928. It was a round mass about $\frac{3}{4}$ in. in diameter, and $\frac{1}{2}$ in. in depth, smooth on the surface, and appeared on naked-eye examination to be a sessile adenoma. Microscopic examination shows that the different coats of the rectal wall are arranged in their natural order (fig. 1, p. 27). The mucous membrane is of natural thickness and the muscularis mucosæ occupies its normal position. In the submucous coat there are several cystic spaces, some of which are partially lined with columnar epithelium but in others no epithelial lining is to be seen. The spaces contain mucus, granular débris, and a few leucocytes. The epithelium lining the cysts resembles exactly the columnar cells occupying Lieberkühn's follicles (fig. 2). The tumour is due to the growth of misplaced epithelium and the progressive distension of the cyst by the secretion of the living cells.

Judging from the appearances presented by the regions through which the sections pass the majority of the submucous cysts are islands of epithelium cut off entirely from the mucous membrane. In some cases, however, as shown in figs. 1 and 2, a connecting link of glandular epithelium can be traced from the mucous membrane to the cyst. The presence of such a connecting strand of cells would explain the capacity of the epithelium to grow in this unusual situation. Those cystic spaces which retain anatomical connexion with the mucous membrane may be regarded as submucous diverticula which have become cystic through occlusion of their communication with the lumen of the rectum. Whether or not cysts which are now completely independent have broken away from the mucous membrane to which they were once united, or, in other words, whether or not the submucous cysts were once submucous diverticula, is a question which cannot be answered from a study of this case alone. In the second case no connexion was found between the mucous membrane and any of the submucous cysts.

Case II.—Patient a middle-aged woman. In 1916 she underwent local excision of an early cancer of the rectum. In 1922 another rectal tumour was removed from the same situation by local excision and this was regarded as a recurrence, but the growth was not examined microscopically. In 1928 she again sought medical treatment and a mass in the rectal wall at the site of the previous operation was found. The tumour was removed by Mr. Lockhart-Mummery. Longitudinal section through the piece of tissue excised showed the presence of several cysts in the submucosa, the largest of which was about $\frac{1}{2}$ in. in diameter. Each cyst had a clearly defined lining and was distended with a jelly-like substance. Microscopic examination shows that the piece of tissue is covered with a layer of healthy glandular epithelium of the type present in the rectum. The mucous membrane exhibits no abnormality except that it is slightly thinner over the most prominent part of the swelling. The muscularis mucosæ is intact and shows no abnormality. Within the submucosa are seen several large cysts distended with a structureless substance which gives all the staining reactions of mucus. Some of the cysts are lined by healthy columnar epithelium but others have no epithelial lining in the region through which the section happens to pass. The muscular coat shows no abnormality other than slight fibrotic change in one region.

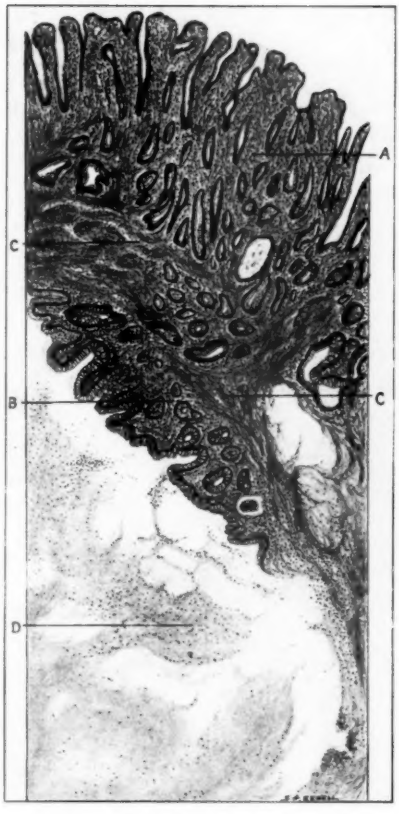


FIG. 2 (higher magnification of fig. 1).—A, Rectal mucous membrane, normal in appearance; B, Submucous cyst lined by healthy mucus-secreting columnar epithelium; C, Muscularis mucosæ, interrupted in one place by a connecting link of glandular epithelium joining the cyst to the mucous membrane; D, Mucus and epithelial débris within the cyst.

Differential Diagnosis of Implantation Cysts.—The distinctive feature of a submucous implantation cyst is the presence in the submucosa of cystic spaces filled with mucus and lined by healthy-looking mucus-secreting columnar epithelium. The situation of the cells lying beneath the muscularis mucosæ distinguishes an implantation cyst from an adenoma, and the healthy appearance and sharp boundary of the misplaced epithelium dismiss any suspicion of malignancy. The histological diagnosis presents no difficulty if the possibility of such a lesion be kept in mind and if a history of previous operation or injury be supplied.

The gross appearance of the lesion in the two cases described suggested adenoma in one case and a recurrence of colloid cancer in another. It is unlikely that the true nature of such a condition could ever be decided by naked-eye examination alone.

A submucous implantation cyst has a clinical interest from the fact that it provides yet another possibility for the surgeon to consider when a painful lump appears at the site of a previous operation.

Section of Tropical Diseases and Parasitology.

[*December* 6, 1928.]

Clinical and Pathological Correlation in Typhoid and Allied Diseases.

By H. Harold Scott, M.D., F.R.C.P., and
H. B. Day, M.C., M.D., F.R.C.P.

(I) **Dr. H. Harold Scott.**—In dealing with a subject so wide as the group of enteric fevers, it is advisable to state at the outset the limited aspect now proposed for presentation.

I shall omit any statement of the ordinary straightforward case and the more usual complications, with their pathological conditions; also, as my records are almost entirely of *typhosus* infections, I shall mention the paratyphoid fevers only incidentally, if at all. My object is to show briefly that where there are anomalous clinical signs and symptoms there are usually pathological findings or processes to which they may be ascribed; nevertheless, if death occurs and we have the chance of subsequent investigations, we are sometimes, but not invariably even then, able to find lesions which can account for the anomalies.

With this aim in view I have divided my remarks into three groups:—

(I) Unusual modes of onset, or cases in which some prominent symptom may lead to an erroneous diagnosis.

(II) Special symptoms or unusual complications.

(III) The masking of the basal condition by coincident disease.

(1) *Mode of Onset.*—(*a*) *With a Chill.* A chill, even a definite rigor, may occur at the onset, as in the case I am about to refer to. Chills may recur at intervals during the course of the disease—the so-called *sudoral* form of typhoid fever; or, they may recur with the advent of complications, such as pneumonia, malaria, phlebitis, otitis media. It is with the first event only that I am now dealing, and the following is a brief account of an illustrative case.

A white woman, aged 27, stated that she got up in the morning feeling well. About 10.30 a.m. she felt chilly and had a severe shivering attack lasting nearly an hour. She was eight months' pregnant and at the end of the rigor labour began. This progressed normally, delivery taking place the same evening. The fever continued and she went through an attack of typhoid fever, the temperature reaching normal in the middle of the fourth week. There were no more rigors. The infant was healthy, though small (it was an eight months' child), and was unaffected by the mother's illness.[1]

One more case is worth mentioning because it forms the end of the sequence. H. G., a native woman, aged 19, was taken ill with a rigor, dyspnœa and the signs of lobar pneumonia. The temperature remained high and she died about the middle of the second week. At the post-mortem, lobar pneumonia was present, but to the surprise of all there were also typhoid ulcers in the intestine. This case is a good example of the pneumotyphus which German authors describe.

(*b*) *With Signs of Peritonitis.*

R. S., native, male, aged 22. This should not strictly, perhaps, be related as a case of anomalous onset, but the patient had been going about and no one had noticed him to be seriously ill. The first striking symptom was most acute abdominal pain; a doctor was called in and found all the signs of acute peritonitis. Operation was refused and the man died. The post-mortem revealed peritonitis, the result of perforation of a typhoid ulcer.

(*c*) *Ambulatory Form.*—According to Osler, and it is the experience of most of us, the man who keeps up at his work, and, though ill, refuses to go to bed in the early days of an attack of enteric fever, runs a very grave risk, indeed such cases contribute largely to the high mortality of typhoid abroad. If I may state a personal reference I will briefly relate my own attack which occurred in 1918:

[1] Reports of two other cases have been omitted for want of space.

I had been feeling wretchedly ill for some days with rising temperature and very severe headache which nothing would relieve. I remember spending whole nights sitting up in bed or tramping the room with my head in my hands. I examined my blood for malaria, and, after ten days of this fever, for agglutination against *B. typhosus* and *paratyphosus* A and B (we had not advanced further than B along the paratyphosus alphabet in those days), with negative results. Again, five days later, the result was again negative. On the seventeenth day, with a temperature of 103·6° F. I had to take a journey of some hours to give evidence in a murder case. On my return I went to bed and I do not remember much more, except that the symptoms thereafter were clearly those of severe enteric, with two relapses, the temperature finally reaching normal on the sixty-third day.

Blood was taken again and again, but the Widal reaction did not become positive until the onset of the second relapse on the forty-eighth day of illness. It remained so, however, for nearly nine months after.

(II) *Special Symptoms or Unusual Complications.*—(1) *Nervous System.* True meningitis may occur from invasion by the bacilli producing the serous form without suppuration; the suppurative is rarer and I have no instances recorded of this. Sometimes the symptoms are strongly indicative of this condition: intense headache, retraction of the head, Kernig's sign and so forth, but if death occurs and a post-mortem examination is made, no definite lesions are discovered to account for them, and we get out of the difficulty by calling it "meningismus." This is not rare enough for me to quote examples.

I have notes, however, of the following interesting case. J. B. L., a negro, aged 35, was not seen by a doctor until he had been ill for some days. He was then found to be *hemiplegic*, the paralysis involving the right side and being accompanied by marked affection of speech. He was sent to hospital as a case of cerebral hæmorrhage and died. At the autopsy the brain appeared normal, except for some œdema, but typhoid ulcers were present in the intestine. Whether the symptoms were due to an encephalitis I cannot say; there was no definite thrombosis. The condition is mentioned by Osler as a rare complication.

I have notes of one interesting case of local neuritis affecting both legs. This occurred in a white man, a Canadian, and was one of the worst cases in my experience. It was his third attack of typhoid; the first, according to the history given, was of the usual severity at the age of nine years, the second, rather more severe, eight years later; both these had occurred in Canada. The third and worst took place in Jamaica. He had three severe hæmorrhages; there were two relapses; during the second, the temperature remained between 105° F. and 106·4° F. for six days, and no measures seemed to affect it for more than an hour or two. About the eighth to ninth week he developed meningitic symptoms which cleared off in about seventy-two hours. He complained greatly of the "tender toes," ascribed by some to neuritis, by others to phlebitis. He had excruciating pain in the calves and soles of the feet, which soon gave place to numbness, and there formed what I can only describe as an acute bed-sore on each heel; the tissues over these becoming dark and gangrenous in appearance and sloughed. There were no signs of bed-sore over the usual sites of spine, shoulders, or ilia. In spite of all this he recovered, but for about two years afterwards, during which time I saw him frequently, he very easily tired and the legs would drag after any unusual or prolonged exertion. Even now he feels a little numbness or heaviness, especially in the right foot, when he is exhausted. In the case of this patient, however, though there was profound involvement of the nerve centres, the acute pain immediately preceding the sloughing points to a trophic lesion due to neuritis.

(2) *Peculiar Type of Fever.*—(*a*) I have seen three cases of "afebrile typhoid." It savours of an Hibernianism to place such under the heading of "peculiar type of fever," but they will not fit in anywhere else. Osler disposes of this form by saying: "The occurrence of this is doubtful, and the cases so termed are probably mild attacks with slight fever for a few days." My experience has been different. All three cases were exceptionally severe and the patients died within a few days, prostrated from the outset by overwhelming toxæmia.

(*b*) There is the case with fever of the "inverse type," and at the same time definitely intermittent. I only remember one such case under my personal

observation and that was accompanied by tuberculosis, so is hardly a fair instance, since in tuberculosis, as we know, this inverse temperature is not so rare.

(III) *The Effect of Coincident Disease.*—The effect of pneumonia in masking the typical onset of enteric fever has already been mentioned, and also that of an initial rigor leading to premature parturition.

Among my notes are cases of typhoid occurring in a tuberculous patient, of typhoid with early localization in the lungs or early prominence of pulmonary symptoms, and of tuberculosis simulating typhoid and being so diagnosed, but all these are comparatively frequently met with, and need not detain us now.

There is one condition of associated infection, however, which well merits attention, namely :—

Typhoid Fever and Malaria.—In 1919 I recounted several cases of this association in a paper published in the *Annals of Tropical Medicine and Parasitology*,[1] and the mutual effect of the two conditions is indeed very remarkable.

It is well known that in malarious subjects the advent of an attack of enteric fever may be preceded by three or four paroxysms practically indistinguishable from those of ordinary ague, except that the fever is not checked by quinine. The recollection that such do occur will make us bear in mind the caution that malaria-like fluctuations of temperature, and even the presence of parasites in the blood, do not exclude typhoid, i.e., typhoid complicated by malaria, the only condition which we have any right to call "typho-malaria," a term which, fortunately, has fallen into disuse.

Any disease in a malarious subject is apt to assume an intermittent or periodic character, e.g., lobar pneumonia may (I admit, not often) present periodic remissions till the crisis. This must not be confused with the "pneumonic form of malaria" such as was described as occurring in the war among the troops at Salonika. To state the matter briefly: typhoid fever may simulate malaria and malaria may simulate typhoid fever. The point I wish to lay stress upon now, however, is the actual association of the two infections.

There are three periods at which this conjunction or association may arise. First; at the onset. The typical "normal" chart is then changed. Instead of the "staircase" rise we may have rigors, sweating, and partial remissions, and the condition is usually diagnosed as malaria (excusably, if parasites are found); quinine is given, which checks the rigors and marked oscillations, but the temperature remains high. It is the persistence of the fever which usually leads the medical attendant to look for signs of enteric.

Second; at the height of the disease, perhaps after two or three weeks of typical temperature, rigors and oscillations may appear, causing much anxiety and a fear of serious complications. Quinine steadies the temperature and the typhoid resumes its course.

Third; at the termination of, or during convalescence from, an attack of enteric fever.

It is of the first only that I will speak, because it affords an excellent example of what I may designate the "antagonism of diseases." It is a principle now employed therapeutically—the treatment of general paralysis by malaria, for example, but it was not so when the following cases were first recorded.

Case I.—B. M., female, aged 21. Admitted to hospital with a history of "fever and headache" for the previous three days. There had been no shivering attack. Her temperature on admission was 102° F. *Plasmodium falciparum* was found in a smear of the blood and *B. typhosus* was obtained by hæmoculture. The tongue was coated, with red tip and edges, and the general aspect was that of a typhoid patient. Quinine was given, but otherwise the treatment was that of enteric fever. During the succeeding five days the temperature once fell to 99° F., but except for this remained between 102° and 104° F. On the ninth

[1] "Coincident Malaria and Enteric Fever," *Ann. Trop. Med. and Parasitology*, 1919-20, xiii, 195-214.

day of the illness it fell rapidly to 98° F., and remained down till the day of discharge three weeks later (Chart I).

Case II.—C. P., male, aged 28. Was admitted to hospital stating that he had been having "fever" for nine days. Apart from "feeling feverish," he affirmed that he did "not feel at all ill," nor did he appear so. Except for a temperature of 101° F., a somewhat coated tongue and a just palpable spleen, there were no objective signs. Examination of blood-smears showed a heavy infection with *P. falciparum*, often as many as eight or ten in a

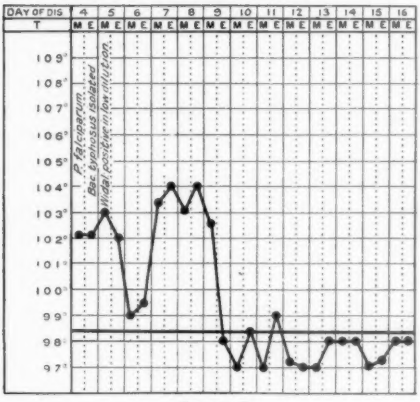


CHART I.

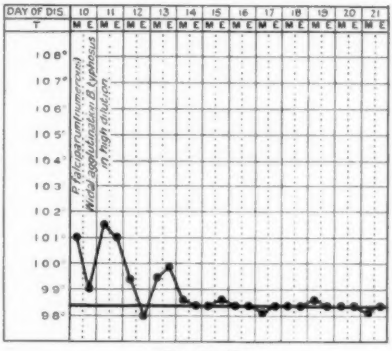


CHART II.

single field, and several corpuscles contained two and occasionally three parasites. His serum gave a strong agglutination of *B. typhosus*, negative with the paratyphoids. Blood-culture was negative (he did not come in till the tenth day of disease) but the bacilli were isolated readily from the stool.

The course of the illness was exceptionally mild; the patient was most cheerful and

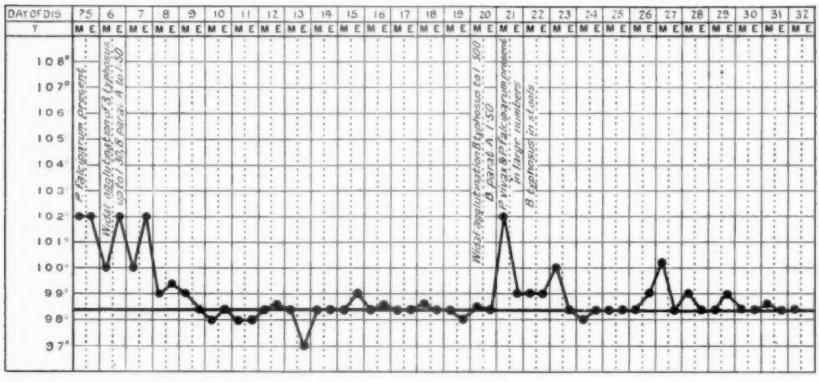

DAY OF DIS
T
P. falciparum present.
Widal agglutination B. typhosus to 1/30 & paratyphoid A to 1/30
Widal agglutination B. typhosus to 1/500 & paratyphoid A 1/30
P. vivax & P. falciparum present in large numbers
B. typhosus to 1/1000

CHART III.

ridiculed the idea of his being ill enough to be kept in bed. The temperature became normal on the fourteenth day and convalescence was rapid and uninterrupted (Chart II).

Case III.—L. R., male, aged 20. On coming to hospital this man gave a history of "fever and headache" for the preceding two days, though further questioning elicited the fact that he had not been feeling really well for three or four days. The temperature was 102° F., tongue slightly coated, but moist, spleen distinctly enlarged and liver palpable.

Blood examination showed the presence of *P. falciparum* and his serum gave agglutination of *B. typhosus* in low dilution, 1 : 30 only. Temperature became normal five days after admission, that is, on the seventh, or more probably ninth, day of the disease. Ten days later the patient stated that he felt quite well and complained at being kept in hospital. His blood was consequently examined again and a specimen of the stool taken for culture. The Widal agglutination had risen greatly, giving a marked positive reaction at 1 : 500 (higher not tried), and the smears, to our great surprise, revealed *P. falciparum* and *P. vivax*, both in fair numbers, one or two in every field, and many corpuscles showed multiple infection. *B. typhosus* was isolated from the stool. The patient, on being told that he could not leave just yet, became very angry, stating that he felt perfectly well. The temperature was taken two-hourly on that day, latterly it had been taken morning and evening only. It was found to be 102° F. at 2 p.m.; it occasioned very little discomfort, and after this never exceeded 100° F., being soon controlled by quinine (Chart III).

All these cases, which have been selected from a larger series, occurred in a place where enteric fever is a serious disease and associated with a high mortality; also where malaria is severe, 80 per cent. of cases being malignant tertian, and occurring with a high mortality.

In spite of these facts, we find that the presence simultaneously of two diseases, and, what is perhaps more striking, one bacterial, the other protozoal, each of which is usually severe in type and effects, leads to an illness of comparative and sometimes very remarkable mildness, from which recovery is rapid and complete; whereas, either separately is nearly always associated with prolonged convalescence and much debility.

The question of antagonism of diseases is one of great obscurity, complexity, and certainly of much interest. I therefore commend it as a subject for future discussion.

(II) **Dr. H. B. Day.**—The clinical course of typhoid and the diseases allied to it is the outcome of a sequence of pathological events: (1) Infection of the alimentary tract by the causative bacilli; (2) invasion of the body tissues and blood-stream, and (3) the destruction and elimination of the bacilli with recovery in most cases.

Ingestion of a sufficient number of typhoid bacilli is followed by their proliferation in the lower portion of the small intestine; the conditions in the other parts of the alimentary tract do not appear favourable for their growth. Invasion of the tissues occurs mainly through the Lieberkühn follicles in the small intestine. To effect this entry it is necessary that there be sufficient numbers of bacilli present to damage the intestinal epithelium. Should the number be small or the resistance of the body tissues be increased by a previous infection or by vaccination through inoculation or ingestion, general invasion may be prevented or checked at an early stage. Such a person may become infected and pass living bacilli in the stools, but show no clinical symptoms of illness.

In the unprotected patient typhoid bacilli pass through the epithelium and enter the lymphatic channels or lacteals. This probably occurs early after infection—some days before the definite onset of fever.

Typhoid bacilli do not cause an acute inflammatory reaction such as the pyogenic cocci provoke; they have no chemiotaxic attraction for leucocytes (polymorphs), but the pathway of invasion is marked by a toxic reaction in the lymphatic channels and nodules—Peyer's patches, solitary follicles and the mesenteric glands.

It was just thirty years ago that Mallory first clearly showed that the characteristic lesions of typhoid are due to a proliferative reaction of the reticulo-endothelial system. The enlargement of the lymphatic nodules is chiefly due to aggregations of these large endothelial cells in the lymph sinuses, where they act as macrophages, ingesting lymphocytes, red blood-corpuscles and plasma cells. A similar reaction is seen along the lymphatic vessels of the intestinal mucosa.

The first clinical symptoms—rising temperature, etc.—are due to the toxic products in circulation increasing faster than they can be eliminated or neutralized.

Blood-culture shows the presence of bacilli which may persist as long as two weeks or more. On the clinical side the fact that an acute nephritis may coincide with the initial fever shows the general infection at this early time.

The reaction of tissues in the body to bacilli arrested in different parts causes the clinical signs which appear at the end of the first week of fever, namely, the eruptions of rose spots and the enlargement of the spleen. Here and in the liver and bone-marrow the same proliferation of the reticulo-endothelial cells takes place as in the intestinal lymph nodules. Living bacilli are frequently excreted in the bile and urine and may proliferate rapidly in the biliary and urinary passages.

The macrophages provided by the reticulo-endothelial system frequently succumb in their struggle; they undergo necrosis and become enmeshed in coagulated fibrin. According to Mallory it is this degeneration occurring in the perivascular lymphatics of the local veins that leads to death of the endothelium lining the vein, with consequent thrombosis and macroscopic necrosis. In this secondary way necrosis and ulceration of Peyer's patches and focal necroses in the liver are produced.

[Illustrations from Mallory's original paper were shown.]

Time does not allow of reference to complications and sequelæ that may follow, for the laboratory aids to diagnosis require consideration. Here we include:—

(*a*) *Leucocyte Count.*—The characteristic leucopenia with diminution of the polymorphonuclears reflects the conditions found at the sites of reaction in the tissues. Sections of blood-clot reveal the presence of endothelial macrophages, which are disintegrated by the ordinary process of spreading films.

(*b*) *Urine Tests*, such as the diazo reaction, ferric chloride and iodine tests do not seem sufficiently elective to be of much assistance.

(*c*) *Blood-culture.*—This is usually positive in the first week and often in the second. It is the earliest and by far the best means of obtaining a positive diagnosis, and has the great value of showing which type of enteric infection is present. There appears to be no confirmation of the opinion expressed early in the war that hæmoculture fails in cases of previously inoculated patients. The method however is not uniformly successful, for only small numbers of bacilli escape into the general circulation at any one time.

(*d*) *Stool or Urine Culture.*—These methods are not so conclusive, but are available to a later date, even in convalescence. A positive result may give a false diagnosis of enteric in the case of a carrier suffering from another fever, while, on the other hand the result may be negative in an undoubted case.

(*e*) *Widal or Agglutination Test.*—The serum test has been ousted from its former pre-eminence by direct cultural methods, but still remains the most convenient bacteriological method when a laboratory is not close at hand. The results obtained may be misleading under the following circumstances.

(1) *Persons recently vaccinated against typhoid and enteric carriers will give a positive result both in health and during an infection due to another agent.*—To overcome this difficulty in the early days of the war, it became customary to estimate the agglutination titre of the serum of a fever patient week by week against typhoid and paratyphoid bacilli. If the serum exhibited a rising titre against a particular organism then a diagnosis of this infection was made on laboratory findings alone.

There is little doubt that such diagnoses were often mistaken and that the discrepancy between the laboratory and clinical views was attributed to modifications in the course, due to previous vaccination. Further experience showed that true typhoid ran much the same course both in the unprotected and inoculated. It was the prevalence of irregular pyrexia due to trench fever that led to confusion in the great majority of cases. Since, in severe cases of trench fever, there was continued pyrexia, eruption of rose spots and enlargement of the spleen, the resemblance to enteric was considerable. It was the peculiar bone pains and later regular recurrences of short

attacks of fever every five days or so that were the main distinctive points in trench fever.

Thus experience in the war supported the experimental work of Dudgeon, and showed that when the body cells had been reacting to a specific antigen by the production of antibody, a fresh stimulus by another antigen might lead to an increased yield of accustomed antibody. Of recent years the converse method has been exploited therapeutically and injections of typhoid vaccine employed to stimulate resistance to other infections. It is a pity that the term "protein shock" has been applied to this procedure, for the toxic antigen of typhoid appears to be non-protein in nature.

(2) *In mild cases of typhoid the agglutination test may not give a positive result until late, even after defervescence.*—It is somewhat remarkable, considering the excellent specific response to injections of killed bacilli in man and experimental animals, that we do not get earlier and stronger agglutination reactions in the natural infection. Now typhoid vaccine, even only a few hours after preparation, already contains a large amount of active antigen dissolved out of the bacilli, so that its rapid effects are easily explained. In natural infection it is probable that disintegration of the bacilli may occur under conditions which severely restrict the free dispersion of their antigen.

To ascertain the action of such a tissue as the blood on typhoid antigen I performed the following experiment:—

To some defibrinated blood collected from a rabbit a solution of typhoid antigen in saline was added and the mixture kept at room temperature for twenty-four hours. No hæmolysis occurred: the supernatant fluid was removed and the corpuscles were washed in two changes of saline and suspended in a third amount of saline. Half this suspension was injected intravenously back into the same rabbit that had been bled. The corpuscles in the remaining half were disintegrated by the addition of distilled water and the resulting solution cleared by centrifuge. The clear, red fluid above the deposit was removed, rendered isotonic with salt solution and injected into a second rabbit.

The rabbit injected with intact corpuscles showed a very slight and transient reaction in its serum, but the animal which received the blood extract gave a good reaction, 1:15, 1:250 respectively.

This experiment showed that typhoid antigen was readily absorbed by the blood elements, but was so firmly taken up that it was only effectively freed by their artificial disintegration. It would appear that the macrophages of the reticulo-endothelial system are engaged in the removal of lymphocytes and red corpuscles thus poisoned by absorption.

If, then, in typhoid fever there may be free antigen in circulation insufficient to stimulate vigorous antibody formation, the treatment by vaccine therapy is thereby justified. Personally, I have found it of distinct service and have seen termination by crisis as the result. In patients dangerously ill, transfusion with the blood of a recently vaccinated donor is of course the method of choice.

In conclusion, I refer to another phase which invites discussion, namely, the occurrence and origin of a relapse. Since it reproduces all the features of the original attack, and may follow closely any sudden change in diet in early convalescence, it appears to depend on a fresh invasion from the intestinal tract. Possibly ulceration of Peyer's patches may leave a weak point in the defence and bacilli incubated in the bile passages effect a re-entry.

Discussion.—Dr. PHILIP MANSON-BAHR said he was able to corroborate in the main the theme of the two openers of the discussion. For many years he had been a strong advocate of close co-operation between the pathologist and the clinician, for otherwise mistakes, often grievous, were apt to occur. He would strongly advocate that every young and less experienced pathologist should spend at least half his time in the wards studying the clinical aspect of cases, and in no class of disease was this so important as in the study of the typhoid-paratyphoid

group. Undoubtedly severe clinical cases of typhoid did occur and ran a normal course without at any time affording evidence of their true nature by means of pathological tests. The Widal reaction might be negative throughout and yet the typhoid bacillus could be cultured from the blood-stream. He (the speaker) was a believer in Marris's atropine test as a useful adjunct to clinical pathology. He was also firmly convinced that Dr. Day was correct in stating that other definite infections occasioned a rise in the specific typhoid and paratyphoid agglutinins.

Dr. E. W. GOODALL said that his experience had lain chiefly in this country, where he had seen a large number of cases, and also in France, Belgium and Poland. He was impressed by the fact that there appeared to be no difference whatever between typhoid fever as he had seen it and as it occurred in the tropics. Many years ago he had divided it into the very same groups as Dr. Scott had stated that evening, and he could recollect from his own experience parallel instances of every single case that Dr. Scott had mentioned, although of course at the Fever Hospital they did not see the cases so early as the general practitioner did. He recalled a case of typhoid brought before the Clinical Society by Mr. Walter Spencer,[1] who had been asked to see the patient for appendicitis. Mr. Spencer removed the appendix and the patient then went through an ordinary attack of typhoid. He was surprised that typhoid did not more often begin in this way: the appendix was often affected, and he had known it to perforate. In another case he had seen many years ago the patient had had typical meningitis, and he had refused to diagnose typhoid, but the patient had died from hæmorrhage and a perforated bowel. The list of errors in the diagnosis of typhoid in every fever hospital was headed by acute pneumonia. Patients came in with marked consolidation; sometimes their spleens were enlarged, sometimes they had diarrhœa and abdominal pain, which might account for the wrong diagnosis. He was glad that we were now going back to clinical medicine; we had been rather led away by the bacteriologists as to the importance of their investigations. There had not been enough co-operation between clinician and bacteriologist, and specimens were too often sent up to the laboratory without any clinical notes. He had never seen typhoid with malaria, and was surprised not to hear anything that evening about typhoid with dysentery; this would probably be most difficult to diagnose without bacteriological assistance. About thirty years ago he had seen a young girl with signs of typhoid—rose spots, enlarged spleen and diarrhœa; just as she was beginning to get well she had acute diarrhœa with abdominal pain and retraction, and hæmoglobinuria. She died from perforation of the descending colon, and autopsy had shown extensive ulceration of the large and small intestines. This was, perhaps, a combination of enteric and dysentery. He (Dr. Goodall) had tried the serum of horses immunized by McFadyean's method, and in one or two of the cases the patients had recovered by crisis. He had never seen a case get well naturally by crisis. Then he had tried the toxin and had had one or two remarkable results; the patients had had a rigor soon after the injection and the temperature had gone down and had never risen again. Thirdly—and chiefly in France—he had tried injections of vaccines; most of the patients had not been affected, but three or four had had a rigor followed by a sudden rise of temperature, and then the fever ceased and they had recovered. All three therapeutic methods had given the same result. Except in very severe cases, however, these remedies should not be used. He always had serum reactions carried out directly the patient came under observation, and at regular intervals subsequently. Sometimes sanitary administrators took too much credit to themselves, but there had been a remarkable reduction of typhoid in this country as a result of their efforts. When he had begun fever work at the Homerton hospital he used to see about a hundred cases every winter; now the Metropolitan Asylums Board would hardly have a hundred cases in all its ten hospitals throughout the whole winter. The sanitary administrators might be congratulated on that satisfactory result, and especially on their improvement of the water supply. For about two or three years before the war he had had all his typhoid cases examined serologically for paratyphoid. In London before the war the incidence of paratyphoid was about 3 per cent. of all enteric cases. The fact that since the war "enteric" was nearly always a B infection required explanation. Paratyphoid B infection was very mild compared with typhoid infection, but the two could not be clinically distinguished. For the sake of the prognosis, therefore, it was always worth while to carry out serological tests.

Dr. A. P. CAWADIAS said: I have observed many cases of typhoid fever with sudden onset, and I think that this onset has a serious prognostic significance, except when it occurs

1 *Trans. Clin. Soc. Lond.*, 1906, xxxix, 59 62.

in individuals who have had malaria, in which cases the sudden onset and the rigors are determined by the special malaria disposition. In Greece I have studied thoroughly the effects of combination between malaria and typhoid fever. The simple antityphoid vaccination causes intense reactions in all who have had malaria, and even awakens a malaria infection. The same occurs in individuals who contract typhoid fever and who have had malaria attacks previously. Although I have observed many cases of this combination, I cannot state any definite conclusion as to whether this combined disease runs a smoother course than typhoid fever alone. Theoretically the point is interesting, because attacks of malaria engender a kind of shock therapy, but my clinical observations do not allow me a confirmation of that theoretical point of view.

I have observed many cases of the so-called colo-typhus frequent in children. The disease (the diagnosis of which was always confirmed by hæmoculture) was marked by intense diarrhœa, and, on post-mortem, apart from the ulcerations of Peyer's patches, I found multiple large but superficial ulcerations of the colon. I think that in these cases—in which I have eliminated any superimposed infection by dysenteric bacilli—there is secondary auto-infection of the colon from the usual colon proteolytes.

From the therapeutical study of a very great number of patients in Paris, as well as in Greece, I have come to the conclusion that the best method consists in systematic balneotherapy, in careful nursing and in fluid diet. I have not seen any definite results from vaccines, and I think these should be tried with very great caution in typhoid fever. The question of immunity in typhoid fever is very obscure, but what we do know is that it is very capricious and very fragile, and the possible negative phase following the injection of vaccines, as I have seen in certain cases, may lead to disastrous results.

CORRIGENDUM.

Page 562, fourth paragraph, sixth line, Joint Discussion No. 1, (with the Section of Therapeutics and Pharmacology) on "The Special Uses of Antimony":

For "on the suggestion of CUSHING"

Read "on the suggestion of A. R. CUSHNY—the well-known Edinburgh chemist."

JOINT DISCUSSION No. 2.

Sections of Pathology and Surgery with the Section of Obstetrics and Gynæcology.

[*January* 18, 1929.]

DISCUSSION ON POST-OPERATIVE THROMBOSIS.

Dr. W. Howel Evans.—The problem of thrombosis is a many-sided one. One question which has always interested clinician and pathologist alike in endeavouring to account for post-operative thrombosis is this: Is there any evidence of alterations in the blood itself which may tend to promote coagulation, and if so, what is their importance relative to other factors?" There is now evidence that at any rate some of the factors concerned in coagulation may be disturbed. The significance is open to discussion: in any case such blood changes can only be one side of the problem. I must ask your patience if, in addition to post-operative thrombosis, I refer also to thrombosis occurring after parturition, fractures, and acute fevers—e.g., pneumonia—for there are certain points of similarity between all these forms.

(1) All are inevitably associated with tissue injury and subsequent absorption of breakdown products.

(2) In association with operations, parturition, fevers, and fractures of the lower limbs an enforced period of restraint or immobility is imposed upon the patient.

(3) All are associated in a small but fairly constant number of cases with thrombosis, with or without embolism. The majority of such accidents occur at an interval of some days after the original trauma. The sites of the original thrombosis are remarkably constant, the most usual being the great veins of the lower extremities and pelvis, occasionally the lungs or the auricles.

(4) In all of them changes in the blood can be demonstrated. The regular feature of increase in the blood-platelets is common to them all; in some at least there is evidence of increase of fibrinogen in the blood.

There is no time to discuss the theories of coagulation in detail, but only to indicate briefly the importance of the factors I have mentioned, namely, tissue juices, fibrinogen, and platelets.

Tissue Juices.—For many years it has been known that tissue juices contain a thromboplastic material or thrombokinase, which is capable of producing coagulation. It can do this by the activation of a mother-substance, prothrombin, in the presence of calcium ions, so forming thrombin, which clots fibrinogen.

Another property possessed by something contained or developed in the injured tissue is the power of provoking the increase in platelets and fibrinogen already referred to. The mechanism and the nature of the substances producing these effects seem at present obscure.

Tissue injury is of course inseparable from surgical procedure.

Fibrinogen.—It is also beyond dispute that the conversion of fibrinogen into fibrin is a central feature of the process of coagulation. During recent years evidence has accumulated showing that there is an increase of the blood-fibrinogen in many morbid states associated with tissue destruction, e.g., trauma, sepsis, pneumonia and others. Lately Allen [1] (1927), at the Mayo Clinic, has pointed out that, following operation, there is a marked rise in the blood-fibrinogen, apparent by the third day, and persisting until at any rate the tenth day. He regards the increase as a "non-specific physiologic response to trauma." An increase of the blood-fibrinogen is also present during pregnancy.

The Blood-platelets.—Much controversy has centred round the question of mammalian blood-platelets. Their very existence has been doubted, but probably most now agree that they are a definite kind of cell, arising in the bone-marrow,

and destroyed by the reticulo-endothelial system. While their precise rôle in clotting has also been disputed, it is generally agreed that they are at least of importance. By their disintegration they provide material which, in some way or other, is intimately associated with the formation of thrombin and the coagulation of fibrinogen. If, when blood is shed, they are prevented from disintegration by means of contact with paraffin, oil, or anti-coagulants such as sodium citrate, or if they are filtered off, the blood shows no tendency to spontaneous coagulation. In the vessels, when injury occurs they collect on the damaged surface and so lay the foundation of the protective clot. It has been pointed out, by Welch and Aschoff especially, that in the so-called static thrombi now under discussion the first step in the building up of the thrombus is the adhesion of platelets to the vessel wall, later followed by the formation of fibrin. This has received experimental confirmation through the work of L. G. Rowntree and Shionoya [2] (1927), who, however, also showed that if clotting were produced sufficiently rapidly the white or platelet part of the thrombus might be insignificant or unrecognizable. Apart from injury to the endothelium, slowing or irregularity of the blood flow may determine the adhesion of the platelets to the vessel wall—also possibly alterations in the agglutinability of the platelets, as by the action of tissue juices or bacterial toxins.

There is also a considerable amount of evidence scattered throughout the literature suggesting a parallelism between the number of the circulating platelets and the tendency to coagulation in the various diseases. This has been referred to by Wells [3] (1926) in his "Chemical Pathology" and also by Hittmair [4] (1927) in a comprehensive review of the platelets in *Folia Hæmatologica*.

Platelets after Operation.—On the basis of consideration of such evidence we studied a series of fifty cases following operation. The operations deliberately ranged from those of minor character to the usual major operations, abdominal and other. The large majority were "clean" operations. In most the platelet count before operation was within normal limits, but following operation it rose, after an interval of about four to six days, reaching a maximum about the tenth day and declining to normal during a further ten days. The average increase for the whole series was 60 per cent., but while a number comprising chiefly the trivial cases showed little or no rise, many of the cases of major type yielded increases of 100 per cent. to 150 per cent. Even in similar operations, however, there was considerable variation. The age of the patient did not affect the increase, and we have recently observed that exercise—i.e., massage, movement and breathing exercises following operation—does not abolish or obviously affect it.

When this work was finished we found an abstract of a communication by Hueck [5], published in 1926, and therefore antedating ours, which is in fairly close agreement with our results.

> "The platelet counts after surgical operation show on the other hand a certain definite behaviour, namely, a small sinking of the number after operation, sometimes lasting several days, and then, from the seventh day on, a strong rise reaching various degrees according to the severity of the operation or the sensitiveness of the patient. Sometimes values double or triple the normal are obtained. About the tenth or thirteenth day the platelets sink again to about normal."

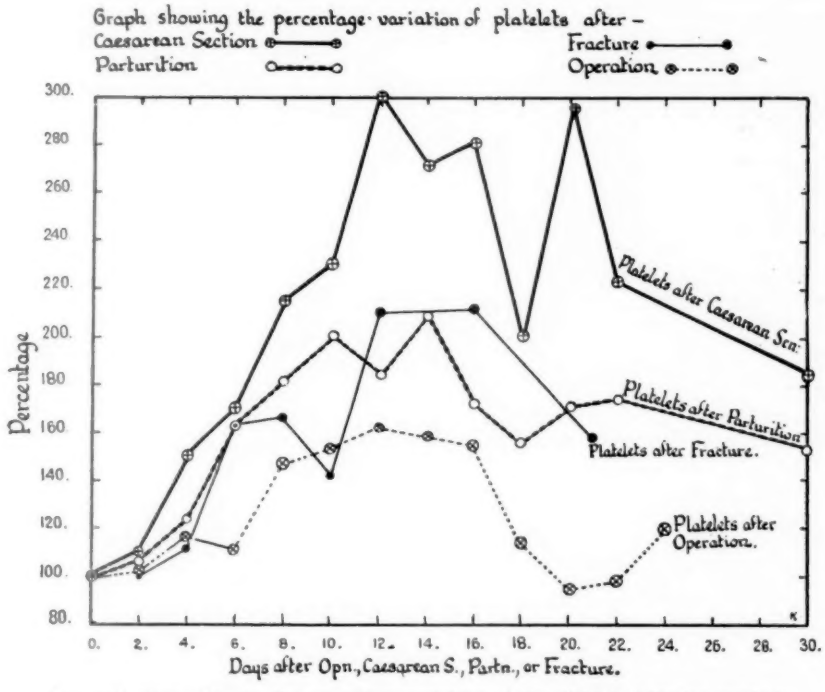
The cases Hueck observed are described as "carcinoma, fractures, tubercle, ulcus ventriculi, and acute inflammation, etc."

Platelets after Parturition.—A study of twenty-eight cases following parturition gave a similar result. The platelets began to rise about the fourth day, reaching a maximum representing approximately 100 per cent. increase from the tenth to twelfth day, after which they fell, as in the operation series. The same variability as in the operation series was demonstrated, six out of twenty-eight showing no appreciable rise. Hare has also observed this increase in ewes after lambing.

In five cases of Cæsarean section the results were most striking. The platelets began to rise rather more quickly and gave an average increase of approximately 200 per cent.; the maximum was again reached in about ten days, but the return to normal was somewhat slower.

Platelets after Fractures.—Recent examination of eleven cases of simple fracture, mostly fractures of the lower limb, with confinement to bed, has shown a very similar curve. One case, that of fractured tibia and fibula, showed practically no rise at any stage.

Control Observations.—Simple withdrawal of blood, e.g., donors giving 500 c.c. of blood, simple rest in bed, simple anæsthesia for examination or manipulation, had no effect on the platelet count.



Composite chart, showing the percentage variations of the platelets following surgical operation, parturition, Cæsarean section, and fractures.

Sepsis.—In the few operations and deliveries in which local sepsis was encountered and in a few animal experiments, the variations were irregular, as is also stated by Hittmair.

Relation of the Platelets to Clotting Time in vitro.—Dale and Laidlaw's [6] (1911) method was used throughout. In numerous cases with low platelet counts we could regularly demonstrate a definite lengthening of clotting time. After operation (splenectomy) and after parturition the clotting time shortened slightly as the platelets increased, though not markedly so, being in no way proportional to the degree of the increase of the platelets. Clot retraction, however, seemed definitely proportional to the number of the platelets.

General Infections.—Thrombosis and embolism are also well known to occur in convalescence from acute generalized infections, e.g., pneumonia, typhoid fever, and influenza. A number of observations in the literature suggest that at this period the platelets are increased in number. Reimann [7], 1924, in particular, has shown that during the febrile period of pneumonia the platelets are usually diminished, but that following the crisis they begin to rise, doubling or tripling in a week or ten days, and returning to normal in a further similar period.

Thus, in relation to a number of different conditions—operation, parturition, fracture, acute generalized infections—there appears this curious slow platelet reaction in the circulating blood, which, from our control observations, appears to be independent of mere loss of blood, confinement to bed, anæsthesia, and sepsis—factors which may all be related to operation. Apparently the only factor common to all these conditions is destruction of tissue and adsorption of unknown breakdown products. The degree of the platelet reaction seems proportional to the amount of tissue injury. Though the mechanism and the delay in the appearance of the reaction are obscure it can only be regarded as a physiological response to tissue injury, and the same is probably true of the fibrinogen increase.

It follows therefore that these blood-changes must be expected in varying degree as a result of any operative procedure. Further, post-operative thrombosis manifests itself most frequently at an interval of some days after the operation. Probably, if the tenth day be taken as the central point, a large proportion of the accidents occur within two or three days on either side of that point, showing a close correspondence with the time of the platelet reaction. The increase of two of the important participants in clotting, namely—platelets and fibrinogen, serves to explain to some extent the frequency of the clotting which takes place in surgical patients *at this time*, and must therefore be admitted as *one* of the possible factors in post-operative thrombosis.

That such increase of platelets and fibrinogen is not essential for the occurrence of thrombosis is shown by the early fatalities observed where embolism occurs within a few hours or more after the operation. Here, probably, the decisive factor is the direct action of tissue-juices, combined with many other factors, such as the extent and shock of the operation, the previous disease, trauma to vessels, and, possibly, personal idiosyncrasy.

Nor is it suggested that the altered state of the blood alone can determine the onset of thrombosis. The very fact that thrombosis does not occur at all in the vast majority of cases after operation, delivery, etc., would indicate—even if we did not recognize it on definite experimental grounds—the existence in living blood of a very powerful mechanism capable, within limits, of resisting agencies favouring coagulation. In combination with other factors, however, the blood changes may be of great importance. One would especially mention the admitted factor of stasis as affording opportunity for the deposition and attachment of the platelets. Once started, thrombosis might be expected to progress rapidly in blood rich in platelets and fibrinogen. The application of measures to minimize stasis receives additional support.

Sepsis.—While there is no convincing proof of the septic origin of the primary thrombi, and while thrombosis may follow the cleanest operation, it nevertheless does not appear possible to overlook the question of sepsis altogether. Sepsis may cause fluctuations in the number, and possibly in the agglutinability, of the platelets, increase of fibrinogen, and possibly, again, changes in the vascular endothelium through the agencies of tissue breakdown-products and bacterial toxins. Thus, while the evidence is clear that sepsis is not a *necessary* factor it yet may be a contributory one in some cases.

In conclusion, while it may be said that the blood-changes so far demonstrated as following operation, parturition, etc., are of interest, and without doubt of importance, we should like to know much more about them. Particularly should we

like to know more of the conditions affecting physical changes in the platelets. Important advance might be made in treatment if it were possible to find an anticoagulant substance or drug which might diminish their agglutinability or hinder their lysis.

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Mr. D. H. Patey.—Post-operative thrombosis may be met with in four forms:—

(i) The thrombus which remains at its site of formation, and gives rise to clinical signs by distending and obstructing the vein.

(ii) The thrombus which migrates *en masse* to occlude the main pulmonary artery or one of its large branches.

(iii) The thrombus from which small fragments only are shed into the circulation, causing minor pulmonary embolism, or pulmonary infarction.

(iv) The thrombus which remains latent, unless the accident of death, followed by a routine post-mortem examination, causes it to become manifest.

Of these four types, massive pulmonary embolism and obstructive thrombosis lend themselves best to study, owing to the definiteness of their symptomatology and the comparative ease of diagnosis. Though the contrary opinion has sometimes been expressed, we shall assume for the purposes of the present discussion that both are due to the same cause or causes.

Thrombokinase Theories.—The first aspect of the subject I wish to consider is the theory that thrombokinase absorption is an ætiological factor. That the absorption of tissue juices from the neighbourhood of the wound is one of the factors determining the special liability of operations to be followed by thrombosis and embolism has been asserted by Fellner in 1911 [1], Lockhart-Mummery [2] at a meeting of this Society in 1922, and Kellogg Speed [3] as recently as 1927. The experimental basis for all theories of this type is the well-known fact that by the injection of tissue extracts under certain conditions into the vessels of animals, death from intravascular coagulation may be produced.

(Photographs showing the results of a typical experiment in a rabbit were here exhibited.)

It should be noted that in all experiments of the type demonstrated in the photographs the tissue extract is injected directly into the circulation. Now, in man after an ordinary operation, it is inconceivable that any massive dose of tissue juices suddenly gets into the circulation, as in the experiment. Any absorption of thrombokinase which takes place is presumably a soaking-up from the tissue spaces in the neighbourhood of a wound through the lymphatics and capillaries. A series of experiments was, therefore, conducted, in which the tissue extract was injected intraperitoneally into animals, whence, presumably, it is absorbed by lymphatic and capillary channels. In this way, the conditions of tissue juice absorption are more comparable to those which obtain in man after operation.

(i) First, a group of rabbits was injected intraperitoneally with large doses—very many times the fatal intravascular dose—of tissue extract. The result in all cases was quite negative.

(ii) A similar negative result followed, when the dose was fractionated, and given in repeated daily doses.

In some instances, estimations of the coagulation time were carried out. It was found that this was not appreciably affected by the injections—if anything, it was lengthened.

From the foregoing results it was concluded that at present the gap between the facts of experimental tissue-extract coagulation in animals, and human thrombosis

and embolism, is so wide as to render any theories based on the former of very doubtful value.

Variation in the Veins.—The rôle of anatomical and pathological variations in the veins in post-operative thrombosis has now to be considered. An illustration (shown on screen) of a plaster cast of the right iliac and femoral veins, taken from a post-mortem subject, presents an outline similar to that of the letter S, the summit of the lower curve corresponding to the position of Poupart's ligament, and the deepest part of the upper curve to the origin of the internal iliac vein. There are two noteworthy points in connexion with this cast. First, it has been suggested that the semi-flexed position of the thigh, associated with the sitting-up position commonly adopted after abdominal operations, by kinking the femoral vein, may predispose to thrombosis. If a cast is made with the thigh flexed, however, the only difference noticed is a tendency to straightening out of the lower curve. It is, therefore, doubtful whether post-operative flexion of the thigh does cause any kinking or partial obstruction of the femoral vein. This view is supported by the fact that thrombosis occurs just as frequently in the practice of a surgeon who has his patients nursed flat on their backs after operation, as of those who follow the more usual procedure. The second point is that in certain individuals, owing to an undue projection of the sacral promontory, the common iliac vein rises up very sharply from the junction of its two tributaries. A dependent pool is thus produced, a condition which must favour eddy formation, and may account for the frequency with which a thrombus starts in this position.

(Next was shown a photograph of a femoral vein with a thrombus *in situ*.) On removing the thrombus a thickened valve was seen guarding the orifice of a large tributary vein, while, running from the valve to the vein wall, was a thick, branched fibrous band. The thrombus, loosely attached elsewhere, was firmly adherent at this point, and it is probable that this curious condition of the valve was responsible for localizing the thrombotic process. The next photograph (shown) illustrates a septate condition of the left common iliac vein, the channel being double. This was found in a patient who died from pulmonary embolism after an operation for hernia, the source of the thrombus being proved to be this vessel. Whether this condition of the vein had anything to do with the development of the thrombus can only be guessed at. I have shown these last two photographs because, as I have found out myself as a result of numerous dissections in post-mortem room subjects, these veins are as a rule exasperatingly normal.

Blood-platelets.—Dr. Howel Evans and his co-workers [5], by their demonstration of a late platelet rise after operation, have made a definite and important addition to our knowledge of the subject. For the firm establishment of the doctrine that there is a relationship between the numerical platelet-increase and the liability to post-operative thrombosis, I would suggest that two things further are necessary. The first is the production of unequivocal proof that the operation of splenectomy (following which the platelet rise is most marked) is more liable to be complicated by thrombosis and embolism than other operations. The best series known to me in support of this view is that reported by Lee, Minot and Vincent in 1916 [6], in which out of 15 cases of splenectomy for pernicious anæmia, no less than three developed post-operative thrombosis. On the other hand, a fairly extensive examination of the literature of pulmonary embolism has not revealed any particular frequency of this complication after splenectomy. This may be owing to the comparative rarity of the operation, and the question perhaps needs further investigation. At present it seems to be an anomaly which requires an explanation.

The second requirement I think necessary is experimental proof. Stimulated by the demonstration of a post-operative platelet rise I conducted a series of animal experiments in which the factors of stasis and platelet increase were combined. I found that, in rabbits, by removal of the spleen the platelets could be increased per cubic millimetre from a normal of 400-600

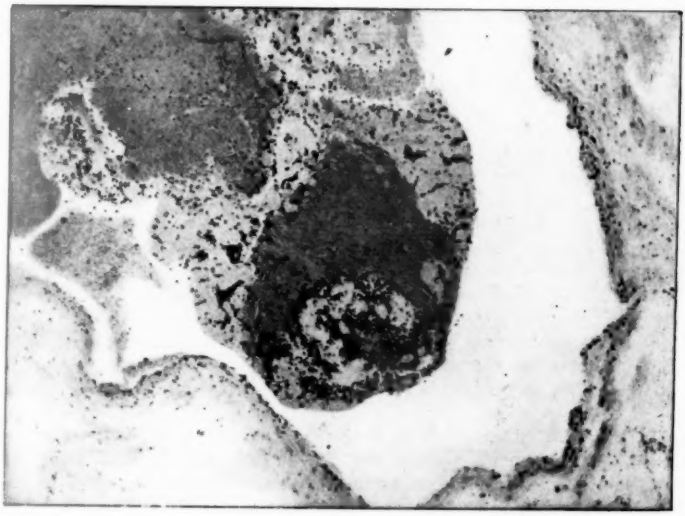

FIG. 1.—Ante-mortem thrombus produced experimentally by increasing blood-platelets, and slowing circulation in one limb. (In the process of preparation, the thrombus has become detached from the vein-wall.)

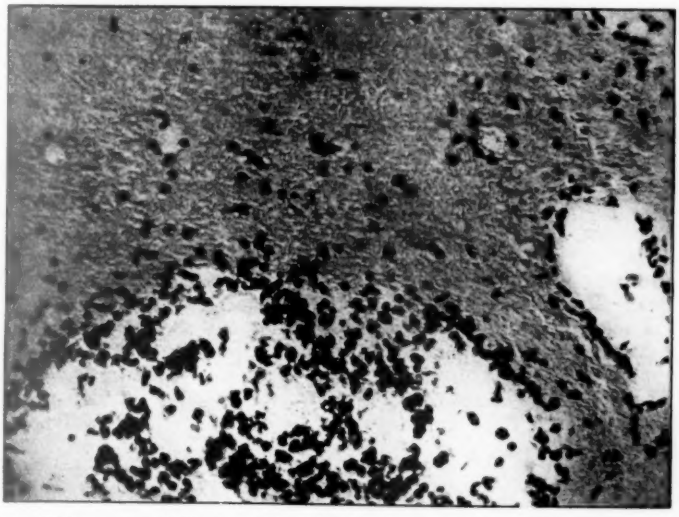

FIG. 2.—Higher power of Fig. 1—to show platelet columns with marginal leucocytes typical of ante-mortem thrombus. Upper part of field is composed of ordinary red coagulum.

thousand to a million or more. Stasis was produced in one leg by cutting the anterior crural and sciatic nerves in order to abolish the pump-like action of the muscles, and tying the femoral artery to cut off the *vis a tergo*. The animals were killed two to three weeks after operation, and immediately examined. Out of eleven such experiments conducted to date, nine were completely negative. In one, two lesions were found in the lungs which, macroscopically, appeared to be typical infarcts, being wedge-shaped, and sharply demarcated from the rest of the lungs, which were otherwise quite healthy. Microscopical examination, however, showed that these were broncho-pneumonic patches: that they were embolic it is impossible to state, but the macroscopical appearance would suggest this. The remaining animal showed in the femoral vein of the treated side a thrombus, which both to the naked eye, and on microscopical examination, was typically ante-mortem. Figs. 1 and 2 (p. 17) are photomicrographs of this thrombus, and the platelet columns with the fringes of marginal leucocytes may quite clearly be seen. I would remind you that the experimental production of a typical ante-mortem thrombus (as opposed to ordinary blood coagulum) in a vein has always been found extremely difficult by all who have worked on the subject, so that one positive case in such a small series as this is rather suggestive. More work, however, will obviously have to be done.

Venous Stasis.—Finally, to pass from theoretical considerations to practical measures, the only important factor we are at present capable of controlling in any degree is that of post-operative venous stasis. To one aspect of this question I should like now to refer quite briefly. It has been well established by Lister and others that an abdominal incision definitely conduces to pulmonary embolism, the special mode of action being presumed to be inhibition of the diaphragm. If a

FIG. 3.—Tracing showing normal variation in intra-abdominal pressure on deep respiration. The rise corresponds to inspiration and the fall to expiration. Note the secondary curves due to the pulse. (Recorded by polygraph connected to intra-rectal tambour.)

series of patients is investigated by means of X-rays before and after an abdominal operation, one finds that in practically all there is a reduction of the limits of diaphragmatic excursion, the cases falling into three rough groups according as this is slight, moderate, or marked. This lessened diaphragmatic movement not only diminishes the aspiratory action of the thorax on the veins, but what I believe to be more important, interferes with the abdominal respiratory pump mechanism. Fig. 3 explains my meaning—it is a tracing of the intra-abdominal pressure of a normal patient breathing deeply, and shows well the regular rise of pressure with inspiration, and the fall with expiration. The whole mechanism thus constitutes a pump, the piston of which is the diaphragm while the abdominal walls form the counter-resistance. That the piston-like action of the diaphragm is interfered with by an abdominal incision has already been shown, and no really satisfactory method of getting the diaphragm to work freely in the presence of an abdominal wound has yet been devised. I have investigated the effect of several suggested measures, but the one now to be particularly considered is that of deep-breathing exercises, as they are so frequently recommended. These, I concluded, would only induce to move freely a sluggish diaphragm, not one which is being seriously interfered with by pain impulses from an abdominal wound. The patient in these exercises frequently disguises the fact that the diaphragm is not moving freely by exaggerated movements of the shoulder muscles, and other accessory muscles of respiration.

Since, therefore, measures directed towards the diaphragm are uncertain attention may perhaps profitably be concentrated on the other agent in the abdominal venous pump mechanism—the tone of the abdominal musculature. The direct effect of an abdominal incision on the latter is that its counter-resistance is diminished, and irregular in its application. Fig. 4, which is a tracing of the intra-abdominal pressure of a patient shortly after an abdominal operation, illustrates this well.

If the above argument is correct, then two practical conclusions emerge. The first is that a firm abdominal binder, so far from being a disadvantage, as is sometimes alleged, markedly aids the venous return by supporting and splinting the abdominal muscles, provided it is not excessively tight. Perhaps the more carefully applied pressure of an abdominal belt would be even more efficacious in this respect. The second conclusion is that, since our efforts to induce the diaphragm to descend are often so unsatisfactory, one might take a lesson from Mahomet, and send the abdominal wall to the diaphragm. By massaging the abdomen it is quite easy to raise the intra-abdominal pressure. Fig. 5 shows how kneading the abdomen at once raises the intra-abdominal pressure. But in order

FIG. 4.—Curve of intra-abdominal pressure taken after an abdominal operation. Shows great irregularity of curve due to the interference with the abdominal musculature produced by the abdominal incision.

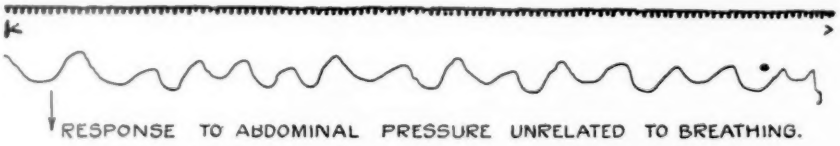

FIG. 5.—Showing rises of intra-abdominal pressure produced by abdominal massage.

FIG. 6.—Showing how abdominal massage applied during inspiration reinforces the abdominal pump mechanism.

that the rise thus produced shall be effective in acting on the intra-abdominal venous stasis, the massage must not be applied haphazard, but during inspiration only. Fig. 6, from a week-old appendicectomy case, shows how, when this is done, the respiratory abdominal pump mechanism is re-inforced. Properly applied abdominal massage, therefore, given regularly at periodic intervals throughout the day, should be of great value. Whether this, together with other measures directed towards venous stasis, which time will not allow us to consider further here, would succeed in reducing the frequency of post-operative thrombosis and embolism can only be settled by carefully controlled clinical experiments.

Conclusions.—Briefly, my impressions of the present position of the subject are the following: Though probably there is still a difficult and trying path ahead, there are signs that our knowledge is advancing. Elucidation will come, I believe, mainly from the laboratory, but speaking as a surgeon, I think that much help may still be afforded by clinical experiment, accurately observed and carefully recorded.

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Mr. Victor Bonney.—The subject of post-operative thrombosis is one of great difficulty, for the discordance that exists between the clinical facts concerning it is so extreme that from whatever avenue of deduction the problem is approached, certain of them flatly contradict the solution aimed at.

The process must in its first inception be either (1) bacterial, (2) toxæmic, or (3) hæmic, the latter term implying an alteration in the constitution of the blood, not secondary either to bacterial infection or the absorption of bacterial toxins, but primary in the blood itself.

In favour of a bacterial origin is the fact that thrombosis, obvious either by the clot itself being palpable or by the visible signs of venous blockage, is always *preceded* by fever and local pain. On the other hand, the majority of cases of pulmonary embolism do not exhibit these signs or symptoms preceding embolism, though pain at the site of infarction and fever occur after it. In fact, embolism is rare when there is prior evidence of thrombosis, which suggests that clots due to or accompanied by definite signs of phlebitis are too firmly anchored in the vein to shift easily. It is noteworthy, however, that most cases of pulmonary embolism have exhibited slight abnormality of temperature before the occurrence, and this, conjoined with the fact that those cases which recover develop signs of pleurisy and local pneumonitis in the infarcted area, suggests that the clot came from an infected site, although there were few or no symptoms pointing to the existence of such prior to the embolism. But if post-operative thrombosis be a bacterial process it bears no relation to the degree of septicity of the operation area, but on the contrary is as common, or even commoner, after "clean" operations such as a simple hysterectomy, Cæsarean section, or shortening of the round ligaments, as after operations for pyosalpinx or suppurative appendicitis; and when it occurs after labour it is not ordinarily preceded by the symptoms of typical "puerperal fever," but comes on in the second or third week as though it were an entity by itself.

It is true that cases of immediate and acute post-operative and post-parturitional sepsis sometimes develop "white leg" later on, but this is not the ordinary course of events, and it seems probable that when it occurs the "white leg" is something superimposed upon the acute sepsis that preceded it, and is not a consequence of that sepsis.

A toxic origin appears unlikely, for we have no knowledge of any toxæmic conditions the lesions due to which exhibit such an anomalous distribution. Obvious post-operative or post-parturitional thrombosis is almost entirely limited to the veins of the lower half of the body, and in the great majority of the cases to the veins of the legs, and again in the bulk of these it is the left leg that is first or only affected. So far as we know toxæmic lesions are generalized, and though they may exhibit a preference for certain anatomical structures, as for instance the effusions into joints that may follow the intravenous injection of immune sera, they do not select any particular region of the body for invasion. Similar objections

apply to the theory that the process is primarily a hæmic one. We know very little about conditions in which the clotting power of the blood is increased, though we have learnt more about them this evening than we have done hitherto. But in the reverse case, hæmophilia, the lesions are generalized, and "bleeder's joint" occurs in the arms as well as in the legs.

The balance of evidence, in my opinion, is therefore in favour of the process being an inflammatory and probably a bacterial one, but a number of difficulties still confront us. Though thrombosis may follow any operation, it is commonest by far after operations involving the peritoneal cavity, and of such operations those on the uterus exhibit the greatest liability to it. This fact negatives the idea that thrombosis is a septicæmic lesion, for septicæmia is no more likely to follow an abdominal operation than any other operation; moreover, such undoubted septicæmic lesions as we are acquainted with are generalized and show no preference for the legs rather than for the arms, and certainly not for the left leg rather than for the right. Thrombosis is very rare after operations on the uterus by way of the vagina: thus it very rarely occurs after amputation of the cervix or vaginal myomectomy—operations which are, at least, as liable to be followed by post-operative sepsis as those involving the peritoneal cavity. This would seem to suggest that trauma to the uterus is not sufficient for its production unless accompanied by trauma to the peritoneum. Against this, however, is to be set the fact of the occurrence of post-parturitional thrombosis in which there is no peritoneal trauma and usually there are no signs of peritoneal infection. The preference for the left leg points to the lesion being due to a local infection, but the fact that left leg thrombosis often occurs after right-sided operations, e.g., after operations on the gall-bladder or appendix, negatives the idea that the route of infection is through veins or lymphatics directly connecting with the leg. The veins coming up from the left leg differ from those of the right leg in their close relation with the colon, and this suggests the possibility of the infection coming from the bowel. It is difficult, however, to explain why, if this is the case, the site of the thrombosis should be so variable; in one case apparently beginning in the external iliac vein—in another, in the femoral vein, in another in the popliteal vein, and in another in the internal or external saphenous veins or some of their tributaries.

The period at which the symptoms of thrombosis usually appear is significant, namely, from the tenth day onwards. Ordinary infection introduced into a wound at the time of operation does not exhibit this long incubation period: symptoms and signs appear within two or three days. This, together with the fact that ordinary, frank, post-operative sepsis is not commonly followed by white leg or embolism, suggests that the organism that is the common agent of post-operative thrombosis (if organism it be) does not belong to the group of ordinary septic organisms. Besides thrombosis there are other tenth to fourteenth-day post-operative affections, such as slowly developing inflammatory effusions in the operation area, most of which, after a period of pain, fever and marked illness, subside without suppuration, their cause being quite unknown to us. Then, again, there are the well-known "catgut blisters"—effusions of blood-serum often mixed with free fat globules and sometimes with fragments of necrotic gut which appear in the abdominal wound in the second or third week after the use of catgut. There are reasons for thinking that catgut blisters are due to the growth of certain anaerobic sporing organisms present in the catgut, the incubation-period of which is a long one. Such organisms are derived from the bowel of the animal from whence the catgut was obtained.

It is conceivable that post-operative thrombosis, occurring as it does most often from the tenth to the fourteenth day, is due to anaerobic infection, the source of the infection being the patient's own bowel. That organisms do migrate through the bowel wall after practically every abdominal operation is fairly certain. Alexander Foulerton showed many years ago that *Bacillus coli* appeared in the urine after

practically every abdominal section, and it is generally held that in bacteriuria the organisms come from the bowel and find entrance to the urinary tract by way of the kidney. If such migration through the bowel wall does occur, then after every abdominal operation the peritoneal cavity contains for a while at least a greater or lesser number of free organisms. Some of these might find their way into the tissues of the leg by lymphatic conduction, and it may be that the anatomical relations of the parts on the left side of the peritoneal cavity favour such transmission more than do those on the right. Such a hypothesis would explain why the peritoneal cavity is so common a factor in the complex required for the production of post-operative thrombosis, and also why the legs are unequally affected. But on the other hand it fails to explain those cases in which thrombosis follows operations not involving the peritoneal cavity, as for instance those in the vagina or on the breast; nor would it meet the case of post-parturitional thrombosis.

I have deliberately instanced all these conflicting facts (and there are many more) because they seem to me to indicate that we are ignorant of certain factors fundamental to the production of post-operative thrombosis, and that until we discover them we shall continue in the future, as in the past, to wander in a maze of conjecture without ever getting nearer to that which we seek. The excellent contribution on the subject of blood-platelets to which we have just listened appears to have revealed one of these factors; but there are certainly others, and not until we find them will the clinical discords to which I have drawn attention be changed into harmonies.

Mr. J. P. Lockhart-Mummery said he thought this problem of post-operative thrombosis was a physiological one, and that it would not be completely solved until more was understood about the whole subject of intra-vascular clotting.

Certain facts had been proved in regard to post-operative thrombosis. The first was that thrombosis was far commoner after abdominal than after other operations. It was generally assumed—and rightly— that one of the important factors was the interference with the movements of the abdominal wall which resulted from an operation in this area. Mr. Victor Bonney had just said that the interference which occurred in the abdominal wall did not explain why the accident took place after ordinary labour: but he (the speaker) reminded them that after labour there was considerable stretching of the abdominal wall, and proper contraction did not take place, so that a very similar condition to that following an abdominal operation of the abdominal wall was present after labour.

The second point on which there was agreement was that the common period for the occurrence of post-operative infarct occurred at about the tenth day. He believed that the thrombus formed in the vein at the time of the operation, or immediately afterwards, and that the reason for the interval was that the clot did not get softened and begin to move till after the lapse of some days.

In the way of causation he did not think sepsis need be seriously discussed. Mr. Victor Bonney had proved, at least to the speaker's satisfaction, that bacterial infection played no part in the causation of post-operative thrombosis. To believe otherwise meant a considerable stretching of the facts.

There were two experiments in connexion with the subject, the significance of which one could not get away from. The first was that which John Hunter carried out over a hundred years ago, namely, the tying of the jugular vein in a normal horse in two places, when it was found that intra-vascular clotting did not occur, even after the lapse of a fortnight. At the end of a fortnight the blood in the occluded portion was still fluid. That experiment had been repeated many times. Therefore mere stasis did not cause clotting.

The second experiment, equally important, was as follows: If, with suitable precautions, such as the use of a double needle, blood was drawn off from a vein

without the needle having touched any tissue, the blood so withdrawn could be kept fluid in the laboratory in an open test-tube. It would not clot, though during the time of the experiment—three weeks—it might have begun to decompose. In order to bring about intra-vascular clotting something must be added to normal blood. What that factor—in addition to stasis—was could not be definitely stated; it might be excess of blood-platelets, it might be thrombokinase. It was a remarkable fact that if only the slightest trace of thrombokinase was added to blood which had been drawn off from the middle of a vein, as mentioned, that blood would immediately clot.

The theory which appealed to him (the speaker) as probably the right one was that if a patient received a cut, or broke his leg, or was otherwise damaged, thrombokinase was liberated, and if that happened to be associated with stasis—which might be only partial or confined to the wall of the vein—the necessary conditions were present for intra-vascular clotting. There was obviously no means of preventing thrombokinase from being liberated during an operation, but to some extent, at any rate, stasis could be prevented, and that seemed the only way, at present, of preventing the formation of post-operative infarcts.

It seemed to him (Mr. Lockhart-Mummery) certain that the cause of post-operative thrombosis was a double one: (1) stasis or partial stasis in the large veins as the result of interference with the movements of the abdominal wall, plus recumbency, and (2) an alteration in the chemical constituents of the blood as the result of trauma to the tissues. Our only method of diminishing the incidence of the condition at present was the adoption of improved technique during operation and care afterwards to prevent stasis in the abdominal veins.

Professor Tom Hare and Professor E. E. Glynn.—In the course of our investigations into the etiology of lamb dysentery, which bears many resemblances to melæna neonatorum of human babies, Glynn and I have examined the blood of a number of ewes and of their lambs [1]. The major part of these observations were not published in our reports of 1927 and 1928 as they did not throw any light on the main problem of pathogenesis. Recently my old colleague, Dr. Howel Evans, suggested that our figures for healthy ewes might be examined from the point of view of the problem which he has been pursuing for several years past. Our figures are presented in the form published by Dawbarn, Earlam and Howel Evans (1928), [2] and the comparison between the human and animal readings is made all the more striking from the fact that we had adopted the technique described by these authors.

Our observations bearing upon the problem before this meeting were confined to eleven healthy ewes. The initial counts were made within five to fifteen days before parturition, which in all cases was normal and unassisted. As shown in the following table of the results, only one post-partum count was made in each case; but allowing for this fact and the small number of cases examined, the mean of the counts agree with the findings of Howel Evans and his fellow-workers.

Following parturition, the ewes' platelets rise while the coagulation time shortens. The white cells rise within twenty-four hours (neutrophil leucocytes) and thereafter fall to normal limits (see fig. p. 24). The red cell count remains stable.

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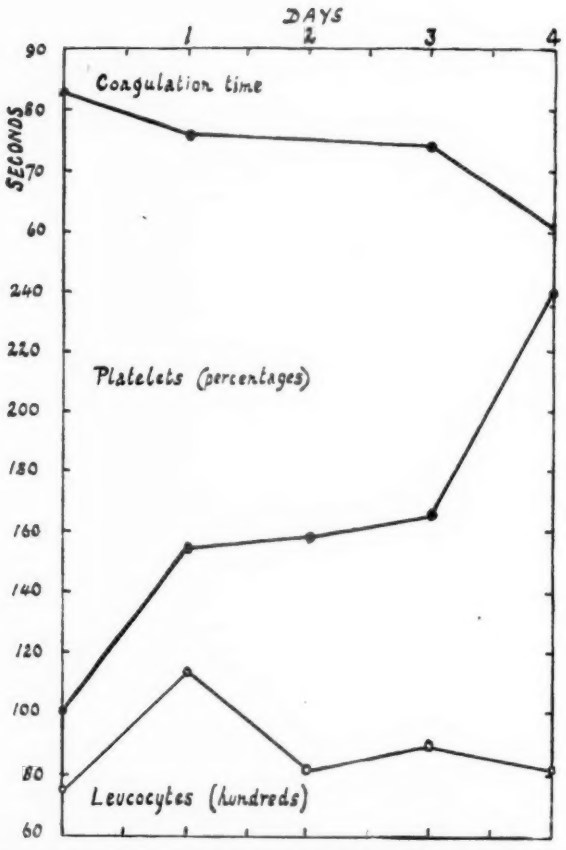

Showing the variation in the leucocytes, platelets, and coagulation-time of eleven healthy ewes following normal parturition.

Table showing the variations in the red cells, leucocytes, platelets, and coagulation time of eleven healthy ewes following normal parturition. The first figure represents platelets in thousands, the second figure red cells in thousands, the third figure the absolute white count, and the fourth figure coagulation time in seconds. The figures in brackets denote percentages calculated from the first observation (100 per cent.).

Ewe No.	Before parturition	After parturition			
		First day	Second day	Third day	Fourth day
2	377 (100) 7,920 5,300 76	— 8,800 8,400 79			
29	438 (100) 11,200 7,500 101	466 (106) 11,200 16,500 77			
5	365 (100) 7,760 11,200 81	460 (126) 9,320 17,200 65			
119	491 (100) 6,880 8,400 92	576 (111) 9,760 7,200 —			
77	320 (100) 7,600 7,500 81	955 (298) 9,120 10,000 69			
13	461 (100) 9,200 5,600 88	600 (130) 9,560 9,000 91			
34	720 (100) 10,520 7,500 77	— — — —	1,144 (157) 10,360 8,100 —		
3	357 (100) 9,600 4,000 76	— — — —	— — — —	734 (206) 10,900 4,700 —	
28	533 (100) 9,960 6,800 75	— — — —	— — — —	778 (146) 10,800 8,400 —	
76	385 (100) 9,200 5,000 86	— — — —	— — — —	553 (143) 9,600 13,700 74	
108	303 (100) 11,200 14,300 75	— — — —	— — — —	— — — —	717 (236) 10,520 8,100 61
Mean platelets	432	611 (154)	1,144 (157)	688 (165)	717 (236)
Mean red blood-cells	9,185	9,640	10,360	10,430	10,520
Mean white blood-cells	7,500	11,400	8,100	8,900	8,100
Mean coagulation time	83	76	—	74	61

Dr. F. J. McCann said that whilst the pathologists had been arguing as to the causation of this condition he had been quietly observing what could be done clinically to prevent it. He believed that—whatever the ultimate cause might be decided to be—in the great majority of cases the cause was the surgeon who operated, being guilty either of errors in technique during the operation or errors in after-treatment.

It had been said that something must be done to bring the abdominal wall to the diaphragm. What could be done (this was not mentioned) was to diminish the pain in the abdominal wall from which the patient suffered. It seemed to be forgotten that a stitch which was tied very tightly caused agony until it had cut through, and this agony had an extraordinary influence on diaphragmatic contractions. If the abdominal wall were carefully stitched and brought together without constriction,

merely apposing the surfaces and allowing Nature's glue to do the rest, but little pain ensued. In his own cases there was little or no complaint of pain in the abdominal wound. In the conduct of the operation he believed that transfixion was a fatal error, for not infrequently a ligature was passed through a vein and left there; and that ligature might be slightly infected, or it might become infected at a later date.

Another important error consisted in tying "chunks" of tissue by means of thick material. Surely it was much better to pick up isolated vessels and tie them with thin material, while accurate hæmostasis was essential.

In the after-treatment the great point was movement from the first day. Patients should be allowed to lie in any position which was comfortable, and should not be kept immobile for long periods. Deep breathing should be encouraged and knee-pillows abolished as the latter were factors in producing stasis in the femoral veins. For the past ten years he had permitted his patients to get up on the seventh day, before removal of the through-and-through stitches. Any fluid in the wound, serum, dissolving catgut or liquid fat, ran out at the lower end of the wound. There was thus a more rapid healing of the wound edges. When the patient was lying on her back, the muscular pull was outwards, whilst in the erect position the abdominal muscles contracting forced the wound edges together. Patients soon walked erect, without feeling that the abdominal wall was tied down. For the last twenty-five years he had not had, either in his private or his hospital practice, a case of pulmonary embolism.

Mr. Cecil Rowntree said that this was a subject in which most surgeons took an intense interest, and he would state some points which had occurred to him when wondering why these dreadful complications sometimes took place.

All practical surgeons would congratulate Dr. McCann on the great success which had attended his surgical endeavours, though what he had said to-night was familiar to most surgeons of experience.

Was there not too great a tendency to make facts fit hypotheses? It was true that post-operative thrombosis did generally follow surgical operations, but it did not always, and he had hoped to hear from Mr. Victor Bonney what was the percentage of this complication after normal labour. He once experienced an extraordinary occurrence. He did a pelvic operation on a patient on a Monday, and on the following Saturday she died from post-operative embolism. On the following day a case of carcinoma of the cervix was admitted to the same bed, and the patient died two days afterwards from pre-operative embolism, and she had not then been interfered with in any way. Considerations of that kind enlarged the range of facts requiring explanation.

With regard to the period at which embolism took place, it was generally agreed to be somewhere about the tenth day, but not always so. In his last case, one of strangulated hernia, the patient died from embolus on the fourth day.

Several had strongly asserted that this complication most commonly followed abdominal operations. But embolism occurred sometimes after breast operations. Therefore if the problem was to be assigned to their pathological friends to work out in the laboratory, it would be necessary to give them a more comprehensive set of facts than those arrived at by a consideration merely of the average time of occurrence, the average time of onset, and the average kind of case.

He agreed with Mr. Victor Bonney that if a patient was attacked with an obvious femoral thrombosis, he (the speaker) should feel happy, because he knew that that patient would not suffer from a pulmonary embolus. Pulmonary embolism, in his experience, had always been entirely unexpected, for no previous warning had occurred. The explanation was possibly a mechanical one, having to do with the site of the thrombosis. He had always believed that femoral thrombosis was fairly safe because the clot filled the vein; and that a pulmonary embolus came from the iliac vein, when there was not a complete thrombosis but a marginal clot which oscillated

in the blood-stream, became detached, and so was carried into the pulmonary artery. Perhaps that was why pathologists often could not find the site of origin of the clot on naked-eye examination of the veins. But it was likely that in these cases microscopical sections might reveal evidence of the site of origin.

He felt that this accident was not concerned with infection, as ordinarily understood; but whether it arose from infection with some other type of organism was a different matter.

He had been impressed by a letter in one of the medical journals from a surgeon in Glasgow who had himself developed post-operative thrombosis after an operation for hernia. The writer suggested that it might be due to infection with a spirillum or some such parasite from the mouth, for just before the operation he had had a tooth extracted. That at least seemed to indicate a possible line for investigation.

Though the cause of thrombosis was not known, it was presumed that its incidence might be diminished if the clotting power of the blood could be reduced, and he had asked his chemical friends to help him on that point. Eighteen months ago it was suggested to him that large doses of alkali given immediately after operations might alter the hydrogen-ion concentration of the blood and increase the clotting time and so diminish the incidence of thrombosis. That had been tried, but it was yet too early to discuss the results.

He was now carrying out a clinical experiment in post-operative thrombosis on rather a large scale. He was attached to two small hospitals in the country several miles apart, where the operative and surgical conditions were precisely similar. In one of them in the last ten years he had had five cases of post-operative embolus, and in the other none. It might be mere coincidence, but he was trying to find out whether there was any difference in the after-treatment, the nursing or the food, which might throw some light upon this anxious question which was causing so much concern.

Dr. W. Cramer said that in the occurrence of thrombosis, as seen clinically, two entirely distinct processes took place, as shown by the microscopic structure of a thrombus, which, as a rule, was a mixed one, and consisted partly of a mass of agglutinated platelets and partly of a blood-clot. The formation of a blood-clot, which involved the formation of fibrin from fibrinogen, occurred as the result of the disintegration of blood-platelets. Platelets disintegrated when they came into contact with a water-wettable surface, and as a result of this disintegration a coagulating agent, capable of transforming fibrinogen into fibrin, was liberated. This coagulating agent was not liberated as the result of an agglutination of platelets. The clinical process of thrombosis began as an agglutination of platelets. (A number of factors which might be concerned in this process were discussed by the speaker.) The mass of agglutinated platelets presented a water-wettable surface, which induced a disintegration of platelets with the formation of a clot. The process of thrombosis as seen clinically was therefore a slow one. There might be a variety of causes for it, and it might be induced many days before the clinical signs of thrombosis made their appearance.

Miss Frances Ivens said that Dr. Howel Evans had made a valuable contribution to the study of post-operative thrombosis. She hoped that he would also investigate cases of pregnancy toxæmia. In her experience the incidence of femoral thrombosis in these was unduly high and a latent focus of infection, streptococcal or coliform, could usually be demonstrated by bacteriological examination of the urine during pregnancy. She (Miss Ivens) had noticed that in the small number of deaths from pulmonary embolism among her cases of abdominal section, viz., about one per 1,000, two had occurred after operation for tarry cysts of the ovary, which burst during removal. This altered blood might have contained some of the substances referred to by previous speakers as predisposing to the formation of clot.

Dr. Howel Evans (in reply) said that in all matters concerning the coagulation of the blood advances in knowledge had been very slow; as the result of the work of the last hundred years there were but few points generally accepted.

He did not contend that what he had pointed out about the platelets explained post-operative thrombosis and embolism. It was however striking to note how regularly this feature of a slow increase of platelets—and also increase of fibrinogen—occurred regularly in the types of tissue injury mentioned, e.g., operation, parturition, fractures and acute fevers. It could only be regarded as due to absorption of products of tissue injury.

Mr. Patey (in referring to splenectomy) suggested that to confirm the association of thrombosis with thrombocythæmia it would be necessary to show that splenectomy—an operation followed by an extremely marked increase of the platelets—predisposed to thrombosis. As far as he (the speaker) knew, splenectomy did not predispose to pulmonary thrombosis or embolism, but in certain cases of splenic anæmia, or splenomegaly with anæmia, mesenteric thrombosis not infrequently followed splenectomy. In 1925, Rosenthal [1] published a series of cases of splenic anæmia in which the behaviour of the platelets after splenectomy was studied. In the majority the platelets showed a rapid temporary increase followed by a slow but permanent fall to normal, the patients making good recoveries. In other cases the platelets remained persistently high, while in others they fell temporarily, but again rose to high levels. Three of these patients died from mesenteric thrombosis, and three remained unimproved after having suffered thrombosis in various sites—the mesenteric, internal saphenous and external jugular being mentioned. He (the speaker) had encountered one such case of splenomegaly with anæmia, in which, following splenectomy, the platelets rapidly reached one-and-half millions, and persisted at that level until the thirty-first day, when the patient died. Autopsy revealed thrombosis spreading from the pedicle into the superior mesenteric vein, with gangrene of the small intestine.

He (the speaker) did not believe that the whole story had yet been told. He brought forward his results in regard to the platelets, not as an elucidation of, but as a contribution to, the problem of post-operative thrombosis. In any case the platelets could only be regarded as one factor.

REFERENCE.

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Mr. Victor Bonney (in reply) said that the point made by Mr. Lockhart-Mummery raised the question whether two things were not being considered as one: embolism cases, in which there was no prior evidence of thrombosis or of any pre-existing inflammation, or at most but little evidence of it; and cases of thrombosis of the leg, in which one could not doubt that there was acute inflammation of the vein, with the ordinary signs of inflammation and septic absorption.

With regard to reinforcing the abdominal wall after operation, he at one time strapped patients into what was in effect a rigid armour, but as they would not tolerate it he had to abandon this practice.

In reply to Mr. Rowntree, he had not forgotten embolism following normal labour and the other disquieting facts that the speaker had referred to, all of which stood as contradictions to the various theories advanced; but he thought he had given a sufficient number of instances to depress his hearers at the meeting as it was. With reference to pre-operative embolism, after operation for cancer of the cervix he (the speaker) had had a case similar in which the patient died in the theatre just before he was going to operate.

In conclusion, he felt sure he was expressing the opinion of those present when he said this meeting had not been held in vain. Dr. Howel Evans' work alone had constituted a very valuable step towards the discovery of the cause, but there must be several factors, one of which at least must be local, needed to determine the actual site of the thrombosis.

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